Atmos. Chem. Phys. Discuss., 7, S9434–S9438, 2008
www.atmos-chem-phys-discuss.net/7/S9434/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigating the sources and atmospheric processing of fine particles from Asia and the Northwestern United States measured during INTEXB" *by* R. E. Peltier et al.

R. E. Peltier et al.

Received and published: 27 February 2008

We thank the reviewer for the helpful comments. Responses to each comment are addressed below, and we will modify the manuscript as described:

1. At the beginning part of the manuscript, authors should elucidate (or define) the concepts of SOAs, TOC, WSOC, OC, and carbonaceous aerosols for the sake of readers' convenience. These terminologies were frequently used in the text, but I am a bit confused with these different and undefined terminologies.

The terms used (SOA, TOC, WSOC, OC, etc) can be somewhat confusing, but represent current terminology used in the literature. Each term has been defined within the text (e.g. pp17430, line 5; pp17444, line 13). The term TOC refers to the capabilities

of the analyzer, which measures “Total Organic Carbon” in a liquid sample.

2. As far as I understand, references should be cited in a chronological order through out the manuscript. Authors should check this out.

According to the reference format requirement, citation order within the manuscript is ambiguous (only alphabetic order in the reference list). However, we will reformat the in-manuscript citations to be in chronological order.

3. In section 3.2.1: Discussions on concentrations between ACE-Asia and INTEX-B air masses appear to be a bit redundant. Over the Northwestern part of Pacific ocean, Asia-influenced air masses certainly have lower concentrations than those influenced by NA, due to dilution, "wet deposition into cloud droplets" (authors have omitted this atmospheric physical process in their discussions), and precipitation scavenging. I think it is quite obvious! The section would better be shortened. Also, in section 3.5.2. the general explanation about the multivariate regression method is already well-known and also needs to be shortened.

We are confused by this comment by the reviewer. They state that ‘over the Northwestern part of Pacific ocean, Asia-influenced air masses certainly have lower concentrations than those influenced by NA, due to dilution’, though we have shown this not to be true. In this manuscript, we are making comparisons between ACE-Asia and INTEX-B concentration profiles of organics (OC in ACE-Asia, WSOC in INTEX-B) and sulfate, and found significantly different profiles from similar source regions (Asia). We have shown profiles of organics, sulfate, and (WS)OC/sulfate at a location where emissions are relatively fresh (e.g. during ACE-Asia, measurements were made directly above OC and sulfate sources), and compared this with a well-aged air mass consisting mainly of Asian emissions, that had been transported across the Pacific (and encountered near the North American coast during INTEX-B). We assert that these changes are largely due to precipitation scavenging of aerosol and subsequent significant re-formation of sulfates, and not due solely to dilution.

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While multivariate regression is reasonably well known, it is less frequently applied to airborne measurements in order to better understand WSOC variability. Thus, since a number of mathematic transformations were made to the data (e.g. distribution shifting, standardizing), we want to ensure that we adequately provide our methodology in a concise manner. We feel that 2 paragraphs of description are sufficient to convey the background and technique while remaining somewhat concise.

4. p. 17430: 6-7, ...trajectory "model" (HYSPLIT) and ...."were" used....

Changed.

5. p.17432:28 .....main sources";"–> ....main sources":"

Changed.

6. p.17434 ; 1, ... "lowest ratios".... What ratio did authors indicate here?WSOC/WISOC or WSOC/OC or WSOC/SOAs? Define it first

We assume the reviewer is referring to page 17433, line 1 (and not page 17434). This line will be changed to read '...WSOC comprises typically 30–80% (gC/gC) of OC, with the lowest WSOC/OC ratios generally recorded near sources with large primary emissions and highest ratios in aged air...'

7. p.17439: 10, What distribution is log-normally distributed? Frequency distribution (i.e.frequency vs. concentration)? Authors need to clarify this. If it is log-normally distributed, can the geometric mean and standard deviation better describe the distribution than arithmetic mean and standard deviation, as in the aerosol-size distributions?

Our data represent bulk chemical composition measurements. The frequency distribution of the observed concentrations are lognormally distributed, largely a result of many low-concentration measurements of Asian air masses coupled with sporadic high concentration measurements (e.g. Central Valley, Seattle metro area), are thus log-normally distributed. Therefore, geometric mean and standard deviation are presented

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in table 1. We have modified the manuscript to clarify this point. It now reads:

Because the frequency distribution of the bulk concentration data were lognormally distributed, prior to regression analysis, the natural log for each data point for all variables was calculated to transform the frequency distributions to a more normal distributions.

8. p. 17443:20-22, p17444:25-29, & p.17445:8-10, These paragraphs (and several more throughout the manuscript) could mislead readers that WSOC is mixed externally with sulfate. I think that authors need to rephrase these paragraphs. In addition, at least once at the beginning part of the manuscript, authors need to elucidate that both key species likely exist in the same particles (i.e., internally mixed).

WSOC most likely exists as an internally mixed particle in the ambient environment. In this work, we can not determine the mixing status of the observed organic, and thus do not address it. However, we will add a passage in the introduction on WSOC the discusses the likely internally-mixed nature of organics and sulfate.

9. p.17444:12-15, "characteristic time-scale" is a better terminology than "time constant". Also, specify SO2 lifetime, chemical processing time or characteristic time-scale from Brock et al. [2007] (i.e., give a number to readers).

The term ‘time constant’ has been used by Brock, et al (2007), and de Gouw, et al (JGR, 2007) and others when describing formation time scales for certain compounds. In our manuscript, we discuss a ‘time constant’, but do not denote the chemical formation process. Thus, we will change the manuscript to read ‘oxidation time constant’, which is more consistent with Brock, et al, and de Gouw, et al.

We will also include the specific oxidation time constant for sulfate formation discussed in the reference (3-4 days).

10. p.17449: 2-4, The e-folding lifetimes of isoprene, acetaldehyde, benzene, toluene, iso-butane are short (˜ several hours).

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While this may be true for some of these compounds, all of the species were measured in detectable levels aboard the research aircraft, even after an apparent 3-10 day transport from Asia and so can still be used as tracers.

11. In Figure 2, use the same x-axis scales for the same species for the direct comparison.

This is good suggestion, and we will follow accordingly.

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