Atmos. Chem. Phys. Discuss., 7, S7935–S7938, 2007
www.atmos-chem-phys-discuss.net/7/S7935/2007/

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.

Anonymous Referee #2

Received and published: 22 December 2007

This manuscript mainly investigated the atmospheric formation/scavenging characteristics of sulfate and WSOC measured during NASA INTEX-B campaign conducted over the northern Pacific Ocean and Northwestern parts of United States in the spring of 2006. In general, the manuscript is discussing an interesting topic, and the measurements and analysis are based on sound scientific methodologies. Text is well written. Tables and Figures are of high quality. This work is publishable, however I have also found that there would be some more rooms for further improving the manuscript.

The main caveat of the manuscript appears to be the WSOC-to-sulfate ratio issue. Authors found low WSOC-to-sulfate ratios in the Asia-influenced air masses, compared to

those ratios observed in the North America (NA)-influenced air masses and air masses that had been observed in the ACE-Asia and NEAQS campaigns. Authors attributed this to precipitation scavenging and subsequent sulfate reformation during the advection from Asia to NA. I agree that this is a possibly correct speculation, since SO2 lifetime is relative long and SO2 is hydrophobic (therefore, not easily scavenged by precipitation), whereas VOCs are short-live species and its oxidation products are often found to be hydrophilic. As authors indicated, initial air masses originated from Asia may have high WSOC-to-sulfate ratios. But, the particulate WSOC is also "semi-volatile". As air masses chemically age, SO2-to-sulfate formation becomes active because of: (i) relatively long SO2 chemical lifetime; (ii) hydrophobicity of SO2; and (iii) thus relatively higher levels of SO2 than those of VOCs. Sulfate can then substitute the existing particulate WSOCs, since sulfate is almost non-volatile and strongly acidic (although there is another possibility that the strong particulate acidity could also enhance catalytic polymerization, for example, of glyoxal and other monomer compounds). If this is the case, then, as air masses age, the WSOC-to-sulfate ratios probably get decreased. I do not think that "all the air masses" intercepted by C130 flights during the INTEX-B campaign (˜3500 interception points for the Asia-influenced air masses) had passed precipitation areas during their advection from Asia to NA (although satellite images showed the presences of clouds and precipitations over East Asia). Some or many air masses intercepted by C130 flights could be affected by other atmospheric heterogeneous processes. I think that authors need to add further discussions on this argument (and/or some more analyses, if possible).

Minor issues

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.

2. As far as I understand, references should be cited in a chronological order through-

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out the manuscript. Authors should check this out.

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 the section 3.5.2. the general explanation about the multivariate regression method is already well-known and also needs to be shortened.

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

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

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

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?

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).

8. 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).

10. p.17449: 2-4, The e-folding lifetimes of isoprene, acetaldehyde, benzene, toluene,

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iso-butane are short (~ several hours).

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

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 17429, 2007.