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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 #3

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Review of R. E. Peltier, A. H. Hecobian, R. J. Weber, A. Stohl, E. L. Atlas, D. D. Riemer, D. R. Blake, E. Apel, T. Campos, and T. Karl – “Investigating the sources and atmospheric processing of fine particles from Asia and the Northwestern United States measured during INTEX B” for ACPD – January 2008.

This is a nice paper – it is well written and has some interesting and useful results.

Please address the following comments: 1) Much of the insoluble OC might be smaller than 0.1 um – by $<$0.1 um do you mean OC mixed with other components (e.g. sulphate, water soluble OC)? For example, could particles composed of insoluble OC

mixed with sulphate that are combined larger than 0.1 um but the insoluble OC component alone is <0.1 um be detected? Is the threshold you have evaluated set by the activation point in the PILS, and were your tests done on pure WSOC, mixed or both? Section 3.1 2) Is sufficient dissolution time such that all species that are even slightly soluble will be completely measured? For example, could some species of OC or some calcium compounds not be fully dissolved in the time allotted in the PILS? 3) Your mean Asian sulphate seems lower than that discussed by van Donkelaar et al. (this issue). In particular, their (Fig 6) presentation of the C130 PILS data looks a little different than what I would estimate by combining your Figures 2b and 2d. Are they consistent? 4) North American nitrate is 7x’s higher than Asian nitrate, but there are 10x’s more Asian samples than nitrate samples. Is the NA nitrate just an anomaly? 5) Page 17440, lines 25-26 - The low correlation of sulphate and WSOC suggests different sources and atmospheric processing. I can imagine that varying amounts of SO2 conversion could drive such a poor correlation. This would be one form of atmospheric processing, but I find it interesting that later on in discussing the North American aerosol you invoke atmospheric processing to explain your WSOC observations. So why does it occur over North America, but not over the North Pacific? In section 3.4, you refer to the short time constant for SOA formation. Is this the explanation? Section 3.2 6) When you refer to Asian air masses in terms of vertical profile data, is the air mass of Asian origin all the way from the surface to 6 km? That is, when you bin data in altitude blocks is each bin categorized by a trajectory? Section 3.2.1 7) Lines 11-13, page 17442 – clarify “lost in transport” – the reduction in OC was 10-20 times versus the reduction in CO of about 3 in CO. So do you mean that deposition (dry and wet) contributed significantly as well as dilution during transport? 8) Can the references to observations near the Asian continent be presumed to be representative of air precursor to that impacting western North America? Section 3.3 9) How much “less statistical significance”? 10) You separate your sample points into 30-60 s intervals based on FlexPart analyses. How are the points distributed with respect to flights? If

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some flights are represented by more points than others then are your results improperly weighted towards those flights? Section 3.5.1 11) P 17446, lines 26-29 – it is not unusual to find water vapour correlated with pollution, such as particles and CO (e.g. Kleinman et al., JGR, 1996). Combustion is a source of water vapour. Whether RH correlates or not depends on how the temperature in the air mass is changing as it is transported (cooling or warming). The increased correlation downwind of cloud is interesting, but it may be only marginal as there are only about 3 points out of about 25 in the fresh case that prevent a much higher correlation. 12) Line 1, page 17447 – section 3.5.3. Section 3.5.2 13) Page 17449, lines 18-19 – It would be helpful to this discussion to include Henry’s Law values for the SVOCs at one temperature. 14) Page 17449, lines 22-23 – did you include BDL values of WSOC in the analysis? And if you did, why should this significantly reduce the regression? If the variability of WSOC on the scale below detection limit is important to this analysis, then how significant is it overall (it does not correlate with CO)? 15) Tables 1 and 3 – Nitrate is only about 2% of the sulphate in the Aisan data, but you find significant correlations of WSOC with organic nitrates in the gas phase and your particles appear to be acidic. Can you measure any organic nitrates with your PILS or could there be a significant missing nitrate component? 16) What about an oceanic component to the WSOC. The aerosol is spending a several days over the North Pacific, and your profile of Asian WSOC shows it virtually all below 3 km. Phinney et al. (Deep Sea Research, 2006) measured an average OC of 0.3 g m-3 over the North Pacific Ocean in the summertime. They also measured an average MSA of 0.16 g m-3. These values represent a significant fraction of your total WSOC. Did you measure MSA explicitly or would it be part of WSOC? 17) Page 17452, lines 17-25 – I see little in this analysis, other than that the observations were made generally downwind wind of a cloudy region, to suggest that “the generally higher WSOC/sulfate ratios” were the result of cloud processing. Please explain why it could not be preferential removal of sulphate by the cloud? How much SOA would have to have been produced to give the result, and is it consistent with the mechanisms

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that the authors refer to? Conclusions 18) Page 17453, lines 20-22 – I thought that this hypothesis was previously presented by Brock et al. [2004]? 19) The final statement referring to cloud processing is important, but the authors have not made a sufficient case that their “enhancement” of WSOC was not really a reduction in sulphate.

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

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