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Atmospheric
Chemistry
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Discussions

Interactive comment on "Evidence for Asian dust effects from aerosol plume measurements during INTEX-B 2006 near Whistler, BC" *by* W. R. Leaitch et al.

Anonymous Referee #1

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This paper describes measurements made from a small aircraft, with additional data from a mountain top site, to investigate the concentration profiles of ozone and aerosols at the west coast of North America near the Canada US boarder. Based on the analysis, emissions from N. American and Asia are identified and the characteristics and unique features of the Asian plumes are investigated. The paper is apparently one of a series of papers from this research group resulting from this study (given the many references cited in this work).

Overall, the paper is scientifically important and interesting, especially the main focus, which is investigating processes that lead to the observed variability in aerosol composition as a function of particle size, eg, fine versus coarse. The authors are suggesting that Asian dust is a site for organic aerosol formation during transport from Asia to NA. Given the focus of the discussion, it appears that this is a major point of the paper. However, the basis for the conclusions do not appear to be well supported by the data, at least as presented.

My interpretation is the authors are suggesting that secondary aerosol formation occurred during transport by the following routes: SO2 produces fine sulphate particles, but VOCs do not lead to fine organic aerosol. However, some SO2 conversion to sulfate on coarse dust occurs, as does some VOC conversion somehow linked to coarse dust leads to coarse organic aerosol. Thus, for the conditions experienced by these plumes during transport, and based on measured concentrations, sulphate formation preferentially forms fine particles with also some sulphate-reacted dust, but in contrast secondary organic aerosol formation is by far related to coarse dust particles.

The authors base this on the following evidence. 1) An observed trend of reduced fine particle organic concentrations with increasing coarse particle concentration, Figure 17. 2) The observation that organics are often found in/on dust particles.

The authors go through a detailed and fairly clear argument as to why they believe this is true. The idea is interesting but the data, and hence conclusions, are very weak for the following reasons.

1) From Figure 17, the correlation between mass concentration of organic aerosol to essentially dust concentration is weak, r2 is only 0.38. However, even that is questionable since what correlation there is appears to depend largely on one (the lowest) data point. If this one data point were removed would there be any correlation? (Based on my extracting the data and doing the analysis there would be no correlation). Furthermore, if error bars associated with the composition data (see discussion on this below) were added to this plot, it would throw the conclusion into even more doubt (based on this graph). These same concerns also apply to the sulphate curve in Figure 17.

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2) The observation of organics on dust was based on analysis of 37 individual particles, measured from a different aircraft (C130). The authors should comment on how statistically representative this is. Can broad conclusion be made based on such a limited data set? What reliable insight does this data provide to this paper.

Overall, the paper is interesting, but at this point it is not clear the analysis supports one of the main conclusion. One suggestion is to remove the OC composition discussion and data, and greatly qualify the discussion of fine organic versus coarse organic and dust. This would also make the paper a more reasonable length.

Other issues are discussed below:

Issues with data and data quality:

First, are aerosol concentrations reported at standard T and p, or ambient? This has implications when comparing observations at various altitudes.

Given there is substantial discussion on comparing various measurements, it would be prudent to provide some idea of the expected uncertainty in the various data presented, especially given the results from AMS comparison with filters.

Ion chromatography is used to quantify dust, (eg, Ca2+, Mg2+), however, it is possible that only a small fraction of the dust is water soluble and can be analyzed by this method. The soluble fraction also depends on the dust composition (ie, pH of sample, is it calcium carbonate, calcium sulphate, calcium nitrate etc). Example, line 500, it really should say, Water-soluble calcium represents …. The implications of this measurement limitation should be discussed.

The sampling approach was sub-isokinetic on the aircraft since ram air (no pump) is used to transport sample through inlet to instruments within the cabin. What are the implications for possible sampling bias as a function of particle size? For example, calculated inlet and tubing transmission efficiencies for a range of appropriate particle sizes would be useful information to provide.

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There are many issues with the aircraft Q-AMS data. Using a single comparison with a filter is fraught with possible errors, some of which may be the cause of the poor correlation between them, as shown in Fig 2. Line 21 pg 18539; as stated, there may not be a systematic underestimation of sulphate, on average, but there could be larger differences; up to a factor of 2 just based on the single-flight averaged data of Fig 2. Furthermore, this says nothing about possible wide variability by the AMS at any time during the flights.

The comparisons shown in Figure 3 and discussed in the text are not quantitative, and is generally true for all the comparisons discussed. Why not give more detailed information, such as percent differences etc.

The altitude unit of m-MSL is used, I believe, without definition (I assume it is mean sea level)

Line 20, pg 18544 – This line is unclear

Line 4 and 5 pg 18547 is unclear.

Line 17 pg 18547, assuming a spherical particle

Lines 19 to 25, page 18548; Regarding discussion on ion balance. What about Mg2+? If indeed all the calcium (ie, lone cation) balances the nitrate plus sulphate ions, then that suggests no calcium carbonate. Does that seem reasonable? Secondly, maybe plot equivalent concentrations in figs 13 and 14 so that one can see the ion balance.

Line 15 page 18551, AMS sulphate 0.1 to 0.7 um, is that vacuum diam (can the geometric or aerodynamic diameter be given as done at another point in the paper?)

Line 21 page 18553, typo? Is it 1.9 ug m-3.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 18531, 2008.