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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Fine aerosol bulk composition measured on WP-3D research aircraft in vicinity of the Northeastern United States – results from NEAQS" *by* R. E. Peltier et al.

R. E. Peltier et al.

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We thank the reviewer for their helpful commentary. We will address each main point for this manuscript below. The reviewer's comments have been copied to this response. Our response follows each comment.

REVIEWER 2 COMMENTS

REVIEWER: Page 3075 line 4: Given the timing it might be useful to cite newer references as this summary will very shortly be superseded and represents a review of the state of knowledge at around the turn of the millennium.

AUTHORS: We agree, and will update the reference to the most recent version of

IPCC's report.

REVIEWER: Page 3076 line 24: Given that the cut size is one micron at 1 atmosphere and the sample line is downstream of an LTI, what is the upstream pressure and how does this affect the cut? How much does it vary with altitude?

AUTHORS: The cutsize is marginally affected by the sample pressure, which is at ambient pressure. The MOUDI impactor is designed for a sharp cut at 1.0 micron (aerodynamic diameter) at 1 atmosphere. With the deployed configuration, the aircraft had a maximum altitude of approximately 6500 meters, or approximately 0.4 atmospheres. With an estimate of ambient pressure of 0.40 atmospheres, we estimate the cut size of the impactor decreases to 0.90 microns.

REVIEWER: Page 3076 line 26: The RH is said to close to ambient in the inlet line inside the cabin. How was this maintained? Even if flying unpressurised, the thermal lag inside the aircraft will lead to substantial changes in temperature between outside and in.

AUTHORS: Our statement here is incorrect. In fact, the impactor will restrict particle size to less than 1 micron aerodynamic diameter at a relative humidity that may be significant lower than ambient due to sample heating within the aircraft. Therefore, we will change P 3076 L 26 to "removed particles with aerodynamic diameters greater than 1.0 μm aerodynamic diameter at relative humidity that may have been significantly lower than ambient due to sample heating within the aircraft."

REVIEWER: Page 3077 line 10: If these detection limits are independent of pressure then this should be stated. It should also be stated whether the concentrations presented throughout the paper are at ambient pressure or standard pressure.

AUTHORS: The detection limits are independent of pressure, and the results presented are at 20 degrees C and 1 atmosphere (stated on page 3080, line 5).

REVIEWER: Page 3078 line 10-11: It is perhaps worth pointing out that water solubility

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of the organic carbon is instrumentally defined and may not represent the water soluble organic carbon in the ambient aerosol.

AUTHORS: The reviewer is correct in that WSOC is operationally defined and may not represent all water-soluble organic carbon aerosol in the ambient environment. Sullivan et al (2006) described the WSOC measurement in detail, and in the interest of brevity, we refer readers to that paper for additional discussion on Page 3076, Line 21. Also, WSOC in Tokyo has, at least in one study, been shown to be similar to a measurement of oxygenated organic aerosol.

REVIEWER: Page 3080 footnote: author name incorrect

AUTHORS: Error corrected.

REVIEWER: Page 3083 One might expect that the WSOC/CO changes with photochemical age as SO partitions to the aerosol, is there any evidence for this?

AUTHORS: There is evidence for this as aerosol ages. Work by Sulivan et al (JGR 2006), Weber et al (accepted, JGR, 2007), and de Gouw, et al (in prep) discuss this finding in greater detail.

REVIEWER: Page 3084: line 7 Is figure 3 really necessary?, the statistics provided in the text convey the main point.

AUTHORS: We believe that Figure 3 does contribute useful information to this paper. In addition to reinforcing that sulfate and WSOC have different sources, it further clarifies to the reader that three distinct air masses were observed during this work.

REVIEWER: Page 3086: The terms PMvol and PMpils need to be related to the terms in equation 1.

AUTHORS: We will add a descriptive to Equation 1, showing that the LHS of equation refers to PMvol, and the RHS refers to PMpils.

REVIEWER: Page 3088 When the profiles are discussed, it would be preferable to

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present the concentrations at a constant pressure, has this been done? It isn't clear.

AUTHORS: All data has been converted to STP conditions (see Page 3080, line 5).

REVIEWER: Page 3089 Whilst figure 6 presents all the data and is therefore an important plot, it is very difficult to obtain statistics from it. Other users of the data may find median and interquartile values binned as a function of height an important statistic.

AUTHORS: This is a complex plot and provides a great deal of information for the reader. However, we are not aware of a simple way to convey all of the data in Figure 6 without separating the different traces into individual figures. Since each trace is somewhat related to one another, we believe it is best presented as a single, complex figure.

REVIEWER: Page 3090 line 5: It is again important to be clear how the concentrations are defined when comparing the concentration profiles with others again

AUTHORS: All data has been converted to STP conditions (see Page 3080, line 5).

REVIEWER: Page 3090 line 23 This estimate should be compared with those of other published values.

AUTHORS: We will add the following line and references:

This estimate is consistent with the findings of Heald et al (2005), Maria et al (2003), Park et al (2003) and Jaffe et al (2005) where background concentrations of organic matter ranged from 0.43 to 3 μm/m3.

REVIEWER: Page 3091 line 10 The authors mean precipitation scavenging, without precipitation, cloud scavenging will simply act to enhance the rate of SO2 conversion to sulfate.

AUTHORS: The reviewer is correct - we intended to discuss precipitation scavenging, and will change this line to more accurately reflect this argument.

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REVIEWER: Page 3095 The flight described in this section in the same one discussed by Brown et al (Science, 2006) who discuss the changing uptake of N2O5 as a result of changing aerosol type. The emphasis in this paper appears to be the relationship to the sources in a general sense. However, despite the considerable similarity compared to the previous work of Brown there is simply a reference to state that the same data have been used. I suggest that the Brown et al paper is discussed alongside the data presented here.

The authors state the reason for looking at this flight is that a wide range of sources can be targeted. I would also like to see a more thorough discussion of what the data tell us about the sources than the rather general comments made at the bottom of page 3095.

AUTHORS: The Brown et al Science paper provides an analysis of specific nitrogen chemistry that is occurring during this particular flight. In this paper, presentation of data from this flight is useful because two distinct source regions are sampled, and observations were of a relatively long (2-4 hour) duration. The focus of this section was to provide broad, bulk composition discussion, as well as more detailed comparison between a source region that is dominated by coal-fired power plant emissions with another region that was dominated by urban sources (NYC outflow). While this flight could be explored in greater detail, in the interest of manuscript length we feel that as-is, it most appropriately conveys the message that we sought to provide.

REVIEWER: Page 3107 One of the regression lines in the top left panel of figure 4 does not appear to match any of the data. Is the line correct? What does it represent?

AUTHORS: The regression line for 'non-biomass burning influenced' data is incorrect. Per suggestions by reviewer 1, we will remove the meaningless regression line for biomass burning data, and correct the non-biomass burning data regression line. The regression statistics are, however, correct.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 3073, 2007.

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Interactive Comment