

Interactive comment on “Measurements of size-resolved hygroscopicity in the California coastal zone” by D. A. Hegg et al.

Anonymous Referee #4

Received and published: 18 July 2008

General comments:

This paper presents new data on size-resolved aerosol hygroscopicity at low and high altitudes off of the coast of California during the CARMA-IV field campaign. Combining two measures of aerosol hygroscopicity with quantitative chemical analysis of the aerosol samples and receptor modeling, the authors have demonstrated some interesting results and possible explanations for their observations. They were able to reasonably reconstruct the expected hygroscopicity based on aerosol source for sub-micron particles but not for super-micron particles. They have provided a possible explanation for increased aerosol hygroscopicity with altitude that is supported by the data.

Specific comments:

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Page 10532, Line 6: The abstract states "examine the nature of aerosol aging" which is a little misleading from what was actually done.

Page 10532, Line 8: The receptor modeling does not suggest three sources but rather supports the authors initial hypothesis of 3 sources. The data was forced to fit the authors presuppositions regarding source and was not used to create the ultimate number of sources.

Page 10532, Line 11: I would add commentary about the effectiveness of the method on sub- and super-micron sized aerosols as a last sentence to thoroughly complete the abstract from the conclusions.

Page 10534, Lines 6-12: How long was sampling performed to get the filters and at what total flow rate? How does this compare to the time allotted to get a growth measurement during flight? What types of small particle losses are expected at a 2 micron pore size? Did possible oxidation of samples occur between collection and analysis and were all samples measured in the same time frame so as to avoid bias?

Page 10535, Lines 24-26: This is confusing. 15 flights were conducted but "24 of which were applicable" does not make any sense in this context. Also, MBL and MSL are first used here without definition. Please clarify these lines.

Page 10536, Line 2: "DHGF" is first used here and is never defined.

Page 10536, Lines 6-7: Was there any difference in 100 m versus 500 m samples? What was the "higher-level" from CARMA-III and is it comparable to the 100-500 m from CARMA-IV? Please clarify and specify on Figure 1.

Page 10537, Line 7: Formate and oxalate are present in very low concentrations compared to all soluble ions from Figure 2. Earlier, it was stated that species with concentrations less than formate and oxalate were ignored due to their low concentrations but here the authors explain how important formate and oxalate are; how do they know that other low-concentration species are not important? These two comments seem to

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go against each other in the earlier statements/arguments.

Page 10541, Line 3: What is "bootstrapping"? This is unclear to this reviewer and likely to other readers.

Technical corrections:

Page 10532, Line 1: "Aircraft - based" to "Aircraft-based"

Page 10538, Line 27: What is the actual "close" altitude?

Page 10540, Line 15: a priori should be italicized.

Page 10544, Line 16: "marine" MBL is redundant.

Tables 1 & 2: the number of significant figures seems excessive. "source_3" etc. do not correspond with the text descriptions of "Source three" or "second source" - please bring them into agreement.

General Figures: Please correct errors of units and capitalization. These figures do not reproduce well in black & white and could be altered to do so.

Figs 2,4,6: Error bars would be useful.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 10531, 2008.

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