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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud condensation nuclei measurements in the eastern Mediterranean marine boundary layer: CCN closure and droplet growth kinetics" *by* A. Bougiatioti et al.

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We would like to thank the referee for the thorough comments; our responses follow.

Specific comments *Page 10310, line 15 Here the authors describe that both PM1 and PM2.5 were measured. Later on page 10312, it is mentioned that PM10, PM1.3, and PM1.3-10 for PTFE and PM1 and PM10 for quartz filter samples were analyzed. Are these different measurements?*

Indeed, there were no PM2.5 measurements. The filters that were collected were: i) PM1 and PM10 Quartz filters, and, *ii*) PM1.3, PM1.3-10 and PM10 (their sum) PTFE filters

Page 10312, line 15-21 At Qs= 1 Lmin -1 and sheath-to-aerosol flow ratio of 10:1, the size range measured by TSI SMPS 3080 is somewhat limited, and is likely much smaller than 20-460 nm.

Good point! The sample flow rate was misquoted – it was 0.5 lpm.

Page 10313, line 12-13: Similar CCN concentrations at 0.6, 0.76 and 0.87% do not necessarily suggest that most particles activate at 0.8%. The high activation fraction at high supersaturation is more convincing.

This is true. We have revised the text to reflect this.

Page 10314, line 20-23: Please also show size distribution and values for period D.

Unfortunately, the SMPS was not operational during this period. Only the CCNc and a CPC (for measurement of total aerosol number concentration) were available.

Page 10315, line 1-2: Similar PM1/PM2.5 ratios for organics and sulfate do not necessarily mean internally mixed aerosols. Maybe it is more appropriate to change "suggest" to "are consistent with".

Done.

Page 10315, line 21: Please clarify what "gregs" stands for.

"Greqs" stands for "gram equivalents" or "equivalent weight" (the amount of an element that reacts, or is involved in reaction with, 1 mole of electrons).

Page 10315, line 23: Ammonium sulfate accounted for 75% of the total inorganic mass concentration. Therefore the other inorganic components accounted for about 25% of the total mass concentration, and might not be negligible. Does it improve the closure agreement by taking other inorganic species into consideration?

For the total aerosol (coarse and fine) ammonium sulfate accounted for 62.4$\pm$12% of the total inorganic mass concentration. In the fine mode, ammonium sulfate accounts

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for 80±10%, on average, of the inorganic mass (the original manuscript contained a typo which is now corrected). The potential effects of these ions can be assessed through the sensitivity test in section 3.4, where the ammonium sulfate mass fraction was varied; the average sulfate fraction needs to increase by 7% to account for the underpredictions bias at low supersaturations (but will also lead to an overprediction at high supersaturations). We have included this discussion in the text.

Page 10316, line 19: Should Vs be Vo?

Indeed. We apologize for the typo.

Page 10317, first paragraph: what is the value for Vo?

In this study, ν_o = 1

Page 10317, line 17: PM10 or PM2.5?

It is PM10. Now corrected in text.

Page 10317, line 13-25: The authors suggest that the underprediction at low supersaturation is most likely due to neglecting the soluble organics instead of different composition at Dp50 from bulk measurements. However, the evidence presented are not very strong. Similar compositions for PM1 and PM10 do not necessarily suggest same composition at 100 nm, as 100 nm particles might only contribute a small fraction of total mass. Also Fig.6 shows substantial contributions at 100 nm from both lognormal modes (Sept 21 and Sept 25), which likely had different chemical compositions. The deviation of particle composition at Dp50 from bulk likely contributed (at least partially) to the underprediction. In addition, the deviation of particle chemical composition at small sizes (30-100 nm) from that of larger (100 nm and above) is suggested as the reason for the overprediction at higher supersaturation.

We have stated that the underprediction at the low supersaturation could be from size-dependant variation in composition. However, that alone cannot contribute to the bias, as water-soluble organic carbon is known to affect CCN activity, and constitutes an

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important fraction of the aerosol sampled. This is clear by the marked improvement in the closure, once WSOC is accounted for. The arguments from the observed size invariant composition are presented as indications and not proof (and this is stated as well in the text). We did rewrite this section however to bring the points across more clearly: that composition may vary (historically) by 5-10% (which is sufficient to account for the bias and scatter), but not much beyond that.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 10303, 2009.