Atmos. Chem. Phys. Discuss., 10, C11710–C11713, 2010
www.atmos-chem-phys-discuss.net/10/C11710/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Hygroscopic properties of atmospheric aerosol particles over the Eastern Mediterranean: implications for regional direct radiative forcing under clean and polluted conditions" *by* M. Stock et al.

Anonymous Referee #2

Received and published: 28 December 2010

General Comments:

This work is a large effort by a talented group and deserves publication in ACP. Results like these that are carefully done and incorporate the relationships between aerosol chemical, microphysical and optical properties, and their net effects on aerosol radiative forcing of climate are exactly what modelers need to see. We need more of these studies conducted at different locations around the globe and for longer periods of time!

The other anonymous referee raised several of the same points that I had, so I will not repeat them here. These points should, however, be addressed by the authors before the paper is accepted. I also have a number of relatively minor comments that should also be addressed.

Specific Comments:

P25995, L16: You should reference the name of the manufacturer (PermaPure, LLC) rather than the local distributor.

P25997, L6: Replace "instable with "unstable".

P25997, L1-9: How accurately can you determine the RH at 90% (i.e., what is the uncertainty in this measurement)? I understand that you calibrate using salt solutions, but you can get the right answer for the wrong reasons. For example, the salts or water used might not be perfectly pure, and the RH sensor could be off, and yet the calibration might yield an answer that implies both are fine. This is of course a problem that other investigators have... I am just interested to see what precautions you have taken to ensure the best possible measurements. Related question: What RH sensors do you use in your H-TDMA and H-DMA-APS instruments, and what are the manufacturer's stated uncertainties at 90%.

P26005, L17-22: "However, we admit that uncertainties introduced by assumptions in the model - including spherical shape... were not taken into account (and) consideration of any particle morphology other than a homogeneous mixture is outside the scope of this work." I agree with this assertion. However, one simple test could have been performed that would have told the authors if non-spherical, externally-mixed particles were a major or minor contributor to the Aegean aerosol. This of course is some form of electron microscopy. Do you have archived samples that could be analyzed by SEM or TEM?

These EM analyses also would be useful to answer other questions raised in the

ACPD

10, C11710–C11713, 2010

manuscript; specifically the following two points...

P26008, L5: "...most likely dust particles". EM analysis of a sample or two would easily show if dust was a major contributor.

P26012, L23-26: "This may indicate a relatively significant influence of marine aerosols during the continentally-polluted period." Marine aerosols are also easily distinguished by EM analysis.

P26009, L13-14: "The linear fitting was done to match zero." Rephrase this to something like "The linear fits were forced through zero."

P26010, L16-19: The refractive index entries in Table 5 are transposed.

P26011, L12-15: The choice of 80% RH for growth factors is not a good one if, as you state, much of the Finokalia aerosol is composed of ammonium sulfate. Ammonium sulfate deliquesces at ~81%RH, and if you are off by only a little bit in your RH measurement you may see none, some, or all of the deliquescent growth of the salt at 80%RH. This may explain why you have higher growth factors than observed in the literature. I would do the analysis at 85% if you have an aerosol rich by ammonium sulfate... at least you know the salt has completely deliquesced at that point.

P26016, L1-10: Part of the effect is simply due to particle size and the increased forward scattering (i.e., decreased upscatter –> smaller negative forcing) of the larger marine aerosol particles.

P26024: Literature review looks adequate.

P26025, T1: Should state explicitly that fsoot is derived from analysis of the SDI stages < 1um. Some readers may think the MAAP was used to generate this.

P26026-P26027: State clearly in the text that these are diameter growth factors. You also discuss scattering growth in the manuscript.

P26037, F8: The lowest two points are essentially the same (i.e., well inside the enve-

ACPD
10, C11710–C11713, 2010

lope of their uncertainties). You really should have another point at a smaller particle size to confirm that you have found the peak of this number size distribution.

P26040, F11: Not explicitly stated, but I assume that the measured neph scattering was corrected for angular nonidealities. Is that correct? If so, what method was used?

P26041, F12: Suggests a jump in the lower envelope of data points in the continental A4 aerosol at 80%. Refer to point above about 80%RH and ammonium sulfate.

P26042, F13: Can't see the letters (a) and (b) in the figures. May want to consider white letters on the dark background.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 25991, 2010.