

Interactive comment on “Aerosol hygroscopicity at Ispra EMEP-GAW station” by M. Adam et al.

Anonymous Referee #1

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This paper presents the results of hygroscopic growth, light scattering and absorption measurements at the Ispra site. From these measurements a variety of derived parameters are estimated, including refractive index, the dependency of GF on RH, enhancement factors of optical parameters on RH, and the uncertainties of the measured and derived variables. Most of the focus is on derived parameters and relationships. While this may be desirable or required in the context of comparison with other stations in the GAW and other international monitoring activities, I don't see the scientific value of much of this beyond the measurements themselves. I believe a paper focused on the measurements could be much shorter and straightforward, i.e., I don't believe 16 figures are required to present and explain these measurements. The following specific issues demonstrate my concerns.

The Mie calculations are based on the measured PSDs and refractive indices (RIs) derived from an empirical inversion of the multispectral extinction data. All other derived

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optical properties are based on these RIs. Figure 8 shows astonishingly good agreement between measured and calculated Bsp, Bap, and Bep. Isn't this circular? The authors assume an internal mixture. What components of this mixture account for real RIs as high as 1.7 (Figure 9)? There is a considerable literature on aerosol chemical composition in this area. The paper should reconcile the derived RIs and measured single scatter albedo (SSA) and GFs with the available chemistry.

Indeed, the authors observe that the measured GF(90) and SSA are among the lowest observed at polluted locations. These are perhaps the most interesting observations in the paper but they are not well explained. Based on available chemical data, what accounts for the weaker hygroscopicity and low SSA at this site? Is it soot, organics, or other insoluble material? Why would emissions near this location be so different from other polluted areas?

The Aethalometer provides a consistent measure of adsorption which can be used to establish and compare spatial and temporal variability. However, I don't believe it is accurate considering its measurement artifacts (e.g., multiple scattering, shadowing). I'm not convinced that these artifacts were accounted for. The authors note that Aethalometer Bap was reliable because it compared well with MAAP Bap and stated that the MAAP was unbiased. I simply don't believe this. Chow et al. (2009, *Atmos. Res.*, 93, 874-887) reported large discrepancies between corrected MAAP and photoacoustic (PA) Bap. I wouldn't accept any Aethalometer measurements as accurate without comparison to a PA. Could Aethalometer measurement artifacts account for the low SSA during this study? Further, could the median RH enhancement factor for Bap at 90% RH be due to RH enhancement of multiple scattering in the Aethalometer measurement, noting that Schmid et al. (2006) found no Bap enhancement, at least up to 80% RH? While it is not always feasible, Schmid et al. recommended site-specific calibration of filter-based Bap against the PA. Since this paper bases important conclusions on a measurement (Bap) subject to considerable uncertainty, at least more discussion on this is warranted.

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GF was measured only at 90% RH and its relationship with RH was estimated using the exponential function in Equation 4. The results presented in Figures 10, 12, and 16 as well as the determination of the RI_s at instrument RH (through Equation 5) depend on the validity of this assumption. It is therefore important that the authors address the accuracy of GF(RH) estimated with Equation 4. The authors had ample opportunity to perform RH scans with the HTDMA. Can such results be presented and compared with Equation 4?

Minor

1) last line on p. 5299 "RH corresponds to the any conditions,..." 2) p. 5300, line 21, as noted in the comment by Otto – Bsp = N x scattering cross section, not efficiency.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 5293, 2012.

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