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Comment

***Interactive comment on “The sensitivity of Secondary Organic Aerosol component partitioning to the predictions of component properties: part 2; determination of particle hygroscopicity and its dependence on “apparent” volatility” by D. O. Topping et al.***

**Anonymous Referee #1**

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This manuscript raises an important issue that has largely been neglected when performing hygroscopic growth measurements, the change in partitioning of semi-volatile components between the gas and condensed phases with change in water activity. This is particularly important when the growth factor is reported relative to a dry particle size recorded under dry conditions. The sensitivity of the uncertainty associated with the first indirect effect to the re-equilibration of the semi-volatile organic components under dry conditions in hygroscopicity parameterizations is also considered. The

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authors have presented their arguments in a clear and concise way and I am sure that the manuscript will provoke further discussion. I recommend publication once they have been able to address the following concerns.

1. The impact of the manuscript would be more significant if the authors were to more closely examine the consequences of their claims for previous laboratory and field studies of hygroscopicity. The limit of discussion by the authors is on page 9030 when they state: “However, discrepancies in measurements of SOA particle hygroscopicity with multiple instruments have been reported in chamber experiments that could be attributable to effects such as those described here (Good et al., 2010; Duplissy et al., 2009), as has failure to achieve closure under nitrate rich conditions attributed to HTDMA evaporation of semi-volatiles”. Specifically, I think it is important that the authors expand on the phrase “that could be attributable to effects such as those described here”. Based on their description of the effect, it may be important to classify techniques according to how likely the equilibrium partitioning is to be maintained throughout the experiment. For example, measurements using a HTDMA approach might be expected to be susceptible to repartitioning to a different degree to EDB measurements. In the latter, the partitioning into a gas phase of continually flushed wet/dry nitrogen will be irreversible whereas measurements made using the former approach may, to some extent, allow the repartitioning to be reversible. In this case, the reported GFs could, presumably, just include an inherent correction factor for the greater partitioning by mass into the condensed phase at higher RH. Would this still not mean that using the reported low GFs is a satisfactory first order treatment if the complexity of dealing explicitly with the gas-condensed phase partitioning is to be avoided? By surveying some past measurements, can the authors be more specific about the evidence that already exists in the literature that their claims are important? As a follow up, it is not immediately clear how the increased partitioning to the condensed phase at high RH can lead to GF that should be larger than previously reported. If the partitioning of the organic into the condensed phase follows the partitioning of water, leading to a larger mass of organic in the condensed phase at high RH, should the real GF actually

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not be smaller than the reported one?

2. The authors mention that they are neglecting the effects of kinetically limited mass transfer and condensed phase chemistry in this study. They are also presumably neglecting the possibility that the SOA may contain residual water on drying etc.?

3. The authors make the following statement on page 9030 “If these predicted sensitivities are reasonable, it appears evident that semi-volatile organic components present in secondary aerosol in the atmosphere or smog chambers do not equilibrate in instruments designed to expose them to varying humidity.” Does it really “appear evident”?

4. Figure 1: This is largely a helpful figure, but the distinction between the light green and dark green volumes on the right side of Figure (b) is not made. I presume the light green represents the equivalent volume fraction that partitions into the gas phase on drying.

5. Figure 4: Line 3 of caption, ‘on’ should be ‘no’, presuming I understand the figure correctly. i.e. the sentence should read ‘...simulations assuming no re-equilibration...’

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 9019, 2011.

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