Response to comments by Referee #3 on “Humidity-dependent phase state of SOA particles from biogenic and anthropogenic precursors”

The authors thank Referee #3 for the positive comments and constructive points raised in the review. The points brought up are discussed below:

1. The uncertainty of RH at the point of impaction at LPI deserves considerably more discussion than there currently is in the manuscript. The authors state (on p. 4454) that the relative humidity values were corrected for the pressure drop in the impactor. However, RH is also significantly influenced by variations in temperature - through the exponential relationship between saturation vapour pressure and temperature. How well is the temperature in the different stages of the impactor constrained? At least some decrease in temperature is expected because of the expansion - what kind of uncertainty in RH would this correspond to?

> This is a valid concern for the measurement method used, and has been addressed in Saukko et al (2012). The validity of the relative humidity correction is verified with a reference compound, namely ammonium sulfate, for which deliquescence and efflorescence relative humidities are well documented. The measured phase transitions occur at predicted RHs within the measurement uncertainty. Following statement has been added to the manuscript: The validity of this RH scaling is shown in Saukko et al. (2012), where correct deliquescence and efflorescence relative humidities for ammonium sulfate are obtained to within 3% RH.

2. In general, but also related to the previous comment, the authors need to add error bars to their experimental points in Figs. 3-6 -- in particular to the RH axis. Also, please add some estimate on the experimental uncertainties in Table 1.

> The standard deviation for O/C and H/C are added to table 1. Error estimate for RH scale is given in the revised manuscript (see previous comment).

3. p. 4453, line 1: The oxidation rates in the study are quite a bit higher than the corresponding atmospheric rates. Is it possible that the oxidation rates influence the solidification of the aerosol? The authors should discuss this issue with respect to the atmospheric relevance of their results.

> At current, it is not even clear what exact role the oxidation (e.g. the O/C ratio) plays in the formation of glasses, hence an effect of oxidation rate is not known at present.

4. p. 4454, line 7: What exactly do the authors mean by the statement "The particles with high bounced fraction are more solid..."? The fraction of solid phase is larger? Their mechanical properties are different? This is a vague sentence and should be revised.

Semi-solid / solid amorphous materials have very wide viscosity values. The viscosity of the semisolids varies from ~10^2 Pas up to 10^{12} Pas. Material having viscosity > 10^{12} Pas can be considered as “glass” (e.g. Koop et al., 2011). Thus “more solid” refers to the (amorphous) material having higher viscosity than some other (amorphous) material.

The statement is revised to: Amorphous particles with high viscosity have higher bounced fraction than other amorphous particles with lower viscosity and are said to be more solid than those with lower viscosity and bounced fraction.

5. Fig. 8. Why is the slope reported as a function of the inverted mass of the precursor? In my opinion the results would be much easier to put into context if the actual mass would be given instead of the inverse.

The Fig. 8 x-axis changed to mass of precursor in revised manuscript.

References

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