

## ***Interactive comment on “Bounce behavior of freshly nucleated biogenic secondary organic aerosol particles” by A. Virtanen et al.***

**Anonymous Referee #2**

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The physical state of atmospheric particles is important to assess gas-particle partitioning and many other processes. Virtanen et al. uses bounce artefacts in an Electrical Low Pressure Impactor to draw conclusions about the physical state of biogenic SOA particles formed in two smog chamber experiments. The methods are sound and the techniques are innovative. However, the paper presents only limited additional information compared to previously published data by Virtanen et al. (Nature, 467, 824-827, 2010).

I agree with reviewer 1 that this manuscript presents too little new data compared to the Virtanen et al. 2010 paper to merit publication in its present state. After including calibrations with solid calibration particles down to 30 nm and preferentially also the sophisticated impactor model described on p 9324 and also a few more smog chamber

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experiments at higher RH I believe this will be a fine paper that will improve the understanding of the physical state of atmospheric SOA particles (given that my comments below are taken into account).

Particle densities derived from AMS and SMPS data (1.0-1.1 g/cm<sup>3</sup>) are slightly lower than many other smog chamber studies that typically found 1.2-1.5 g/cm<sup>3</sup>. Could the authors comment on the likely cause for this difference. How was the SMPS and AMS size-calibrated? Given that density measurements for small particles was problematic for the SMP-AMS method, I am somewhat surprised that the SMPS-ELPI method (Ristimäki et al. 2002) was not used for density measurements. The SMPS-ELPI method was introduced by the same research group and is already in the reference list of the manuscript. In my opinion the SMPS ELPI method when using porous substrates should be a more powerful method to determine density compared to the SMPS-AMS method. It would be of interest to get results from density from both methods as density is a key parameter in interpreting the data (e.g. fig 3).

In low-pressure jet impactors adiabatic cooling of the air occurs which could lead either to a reduction of the particle temperature thus promote solidification and thereby increase the detected degree of bounce or it could result in an increase in RH, which if the SOA is hygroscopic could promote a transition from solid to liquid particles. Such effects could be potentially important for the interpretation of the results of this study. Such effects should be discussed in the context of the available literature.

Another question relates to the time resolution. Is the new point below 30 nm calculated for a single SMPS scan? If so, how much did the size distribution vary over the course of this scan? If the limited time resolution of the SMPS may affect the results, then the authors should comment on the uncertainties this impose on the calculated bounce factor.

It is well known that bounce from the vaporization oven in the AMS is a critical parameter in quantitative AMS measurements. Bounce is one contributing (often dominating)

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factor when a collection efficiency (CE) less than unity is found for the AMS. Was the CE of the AMS quantified? If not I strongly suggest to compare the time dependent CE of the AMS between the different experiments as it should give complementary information to the bounce factor in ELPI.

RH of 30% was used in the smog chamber. How does this compare to typical RHs in and downwind boreal forests? I would suggest future chamber experiments be performed with systematically varied RHs or with or without diffusion dryers preceding the ELPI.

Minor comments:

P9318, L22: "flow of" should be "flow rate of"

Fig 1-2: Please add which experiment is used for the data in the figure captions.

References:

Ristimäki, J., Virtanen, A., Rostedt, A., and Keskinen, J.: On-line measurement of size distribution and effective density of submicron aerosol particles, *J. Aerosol Sci.*, 33, 1541–1557, 2002

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 11, 9313, 2011.