Interactive comment on "The role of low volatile organics on secondary organic aerosol formation" *by* H. Kokkola et al.

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We thank the referee for the time invested in reviewing our manuscript. We believe that revising the manuscript according to the referee comments will improve the quality of the manuscript. Here are the referee comments followed by our replies:

General comments:

The authors present measurements and simulations of aerosol formation from VOC ozonolysis in a teflon chamber, focussing on the loss of gas-phase compounds to the chamber walls. Modelled and measured size distributions are compared in order to

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infer the extent to which low volatility compounds are lost to the walls. They conclude that SOA yields reported in the literature are erroneously low because wall losses of these SOA forming compounds have not been taken into account - a factor of four underestimation was found in this study.

Very few details of the SALSA model set up are given in the manuscript which makes it very difficult to follow the results sufficiently well to be able to asses the scientific merits of the paper. I would be interested to see a revised submission of this manuscript which includes detailed descriptions of the model implementation. I have expanded on some key areas below.

- We will include a new section in the manuscript where we give a detailed description of the model.

Specific comments:

2.2 Wall losses Measuring the time evolution of the ammonium sulphate size distribution will give you a measure of the wall loss rate as a function of size. However, I find this sentence to be rather vague: "A functional form of the wall loss rates was obtained by minimizing the difference between the measured and modeled size distributions." Does this mean the wall loss rates were calculated from the measurements, but then tuned in the model to get the best reproduction of the size distribution? Please give further details as to how wall losses are implemented in the model.

- Yes, you have understood correctly the method for estimating particle wall losses. The loss rate was measured injecting polydisperse aerosol in the chamber then measuring the evolution of the size distribution. The loss rates were assumed to have a polynomial form as a function of size. However, since

coagulation is also a sink for particle numbers, we optimized the loss rates using the model so that the model was reproducing the measured evolution of particle numbers for all sizes. We will present the polynomial fit in the revised manuscript and explain the method better.

In 3.1/Fig 2a it is claimed that the injected compounds decreased in concentrations over a few minutes and then reached an equilibrium concentration without further decrease. To my eyes, the concentrations look constant within the noise, rather than decreasing over a few minutes to a stable level. I assume the initial spike at each level is what is being referred to, but the figure isn't very clear. I would suggest smoothed traces in addition to the raw data to clearly illustrate this point.

- We will add an example figure zoomed so that the injection peak and stabilization of concentration is visible.

3.2 "According to the measurements, when the nucleation mode appears in the size distribution, organic mass fraction in the seed aerosol rapidly increased to about 0.3 supporting the conclusion that the new particles were grown by LVOC's." The seed aerosol is ammonium nitrate. How can this have an organic mass fraction? Are you referring to the particles in this size range having an overall organic mass fraction of 0.3? The AMS can't measure nucleation mode particles, so this is presumably what was meant - this needs to be made clearer, because the implication in this paragraph is that the organic mass fraction and O:C ratios apply to the nucleation mode (too).

- Yes, we are refering to the overall mass fraction and will make this clearer in the revised manuscript.

3.3 This section would really benefit from more details regarding the implementation of the SALSA model. There are hardly any details of the model set up given so it's

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very hard to follow the analysis. For example, the SALSA paper describes optimising the number of bins and their spacing over the size distribution depending on the application of the model. How many bins were used for the simulations shown here and how were they distributed (i.e. what were the bin size cuts)? Why are the simulated and measured size distributions (fig. 3) not shown over the whole size range, instead of starting at 10nm? The SALSA paper details an expression for the nucleation rate, but the nucleation rate here is constrained by the measurements. This I would have thought severely undermines the optimisation performed for the stoichiometric coefficients for the lowest VBS class?

- We will include a detailed description of the model (see reply to Referee #1). SMPS smallest channel is for 8.82 nm particles, so the figure covers essentially the measured size range. The smallest sizes are an input to the model and are not included in the optimization and as such do not affect the optimization of the stoichiometric coefficients.

Technical corrections: Figures 2a and 2b need labelling a and b. 3.3 The text suggests that figure 2b was the intended figure instead of the first reference to fig 3b.

- This will be corrected in the revised manuscript.

The graphs in Fig3 are too small (especially the axis and colour scale labels), but even zoomed in, it's difficult to discern the magnitude of the differences between the plots. Some 2d snapshots at key times would be useful for ease of comparison.

- We will include 2d snapshots of the size distributions so that it is easier for the reader to distinguish differences between the measured and modeled size distributions.

Several sections of the manuscript are hard to read due to poor grammar. The manuscript would benefit from more thorough proof reading to improve clarity.

- We will improve the grammar in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 14613, 2013.