The authors present a method to perform long term smog chamber experiments. To avoid deflation of the bag only few instruments measure during the whole experiment while another set of instruments operates only the final eight hours of a 24, 30 and 36 hour experiment, respectively. During an 18 hour experiment all instrument are running simultaneously. The virtual long term experiment (36h) is then composed of the 18 hour experiment and the measured intervals of all the other experiments. Two methods are used to determine the aerosol yield of m-xylene; 1) a lower bound estimate assuming no further gas phase partitioning of the particles lost to the walls, 2) an upper bound assuming full gas/particle phase partitioning for suspended and wall deposited particles. The time trend of the upper bound aerosol mass reaches a maximum after about 12 hours and decreases thereafter. After about 5 hours the O:C ratio is steadily increasing. This second phase is interpreted as aging of the SOA whereby semi-volatile compounds are further oxidized producing higher generation products. Some are more functionalized leading to low volatility products condensing onto the particles while others are fragmenting leading to evaporation and loss of particle mass. These processes are further illustrated with time trends of certain mass fragments of AMS and first and higher generation high molecular weight gas phase species measured by CIMS. Switching off light after 18h provides evidence that wall losses of these semi-volatile species are small or negligible. An effect of particle acidity on aerosol yield was not observed.

The aging of aerosols is an important question and smog chamber studies have so far failed to generate such highly oxidized aerosols as observed in the atmosphere. Therefore, the presented protocol performing longer term chamber experiments leading to more aging is timely and of great relevance. The paper presents interesting results which are well presented. I recommend publication of this manuscript in ACP after my comments below have been addressed.

The composition of several experiments to one long term experiments relies on the assumption that the reproducibility is very good. The authors show indeed a good agreement between two 18h experiments. However, looking at the lower bound mass trace in Figure 2 I have some doubts on this. The 18h experiment starts already to decrease towards the end while after 22 hours mass concentration is slightly higher with a decreasing trend. This occurs again at hour 30. The measurements of the 24h experiment are even missing. Why? Either the repeatability is not as good as stated or the wall loss correction does not work well. Regarding the latter I wonder if coagulation does not play a role when the wall loss rate of particle number concentration 2 is determined. Now, it seems from Figure that individual experiments show particle mass loss due to aging while the composed mass trace does not. Another issue with this is that the higher bound mass trace implies a mass loss while the lower bound estimate does not. However, most of the discussion and conclusion is on the mass loss of the aerosol as observed from the upper bound approximation. Somehow the authors assume that this is the reality. This deserves some discussion. An implication of this is that the lower bound limit can become higher than the upper bound limit over a long time.
If evaporation makes up almost 30% of aerosol mass (in Figure 1 mass decreases from 30 to 20 $\mu g/m^3$) I would expect to see that also in the size distribution. Figure 5 bottom

panel shows a constant maximum diameter. I agree with the authors that evaporation and wall loss effects can have opposing effects on the maximum but not on the size distribution (page 24983 line 18). I would expect a clear narrowing of the size distribution from both sides. Taking the maximum diameter might not be the appropriate parameter to look at evaporation effects.

Page 24980 line 8: The high O:C ratios in the beginning of the experiment seem to have a large scatter. The 30h and 36h experiment do not show these high O:C ratios in the beginning. Why? How reliable are these high numbers?

Page 24981 line 17: The statement “the total amount of each ion” relating to Figure 5 top panel is wrong. What is shown in Figure 5 is a relative normalized value.

Page 24983 line 25: Why does the lower bound mass still increase after lights off?

Page 24984 line 2ff: It states “After irradiation stops, **no** change in the amount and composition of the particles is observed”. I agree that the changes are much lower but there still seem to be some changes. f_{43} and f_{44} seem to evolve slowly and the upper bound mass estimate slightly decreases. Why is the organic to sulfate ratio decreasing? Has this to do with the fact that organics condense faster on smaller particles, thus r_{OS} is higher, but are lost at a higher rate to the walls? How large is such an effect?

Page 24984 line 18ff: please indicate in the text where the notation of compounds and rate constants can be found.

Page 24985 line 20 and Figure 9: To me the time trends shown in the top and middle panel seem to be similar. Both start to increase immediately. Why should the middle panel present a second-generation oxidation product except that the m/z fits with a possible reaction product?