Smith et al. describe the construction and characterization of an indoor chamber for aging studies of biomass burning aerosols native to sub-Saharan Africa. Given the importance of biomass burning aerosols in regional and global environmental issues, the subject matter is somewhat appropriate for AMT. However, the paper cannot be accepted in its current form given its lack of novelty, insufficient results and ambiguous writing. The paper should only be accepted if these issues are addressed.

General comments:

1. I fail to see what makes this indoor chamber novel, and how this chamber is different from the numerous indoor smog chambers already constructed and are currently being used. The authors need to be explicit when describing the novelty of their chamber.

2. The authors claim that that this chamber is constructed specifically for aging studies of biomass burning aerosols native to sub-Saharan Africa. However, it seems like this chamber can be used to study any biomass burning aerosol system, not necessarily those native to sub-Saharan Africa. Does the combustion of biomass burning aerosols native to sub-Saharan Africa require special experimental apparatus or setup that is different from those currently being used in other chamber facilities? This is not clear from the manuscript. This also makes the title of the manuscript very deceiving. The title “Construction and characterization of an indoor chamber for the measurement of the optical and physicochemical properties of aging biomass burning aerosols” seems more appropriate.

3. The amount of results related to the characterization of the indoor chamber presented in this manuscript is insufficient, especially those related to particle wall loss. This makes it hard to judge if the chamber is truly suitable for biomass burning aerosol aging studies as claimed by the authors.

4. I find the flow of the manuscript meandering and somewhat confusing. There are parts of the manuscript (specifically the results section) where I fail to see how the paragraph(s) is related to the point the authors are trying to make.

Specific major comments:

1. Line 112: How does the lab’s temp and RH affect the chamber’s temp and RH (when the chamber lights are off)? It would be useful to have a figure showing this. This figure will also help

convince readers that the lab’s large RH range (30 to 60 %) will not affect the experimental conditions in the chamber.

2. Line 232: Is the chamber operated as a “batch reactor” or as a “continuous flow reactor”? This should be stated in the manuscript.

3. Line 254: What are the concentrations of NO and NO2 added into the chamber based on the flow rates and concentrations of certified NO/NO2 gas cylinders? Providing these numbers will make it easier to compare these concentrations to those measured by NO or NOx monitors shown in Fig. 3. On a related note, does “elapsed time” in the x-axis of Fig. 3 refer to the time passed since the first gas injection? This should be stated explicitly in the text. Also, please indicate in Fig. 3. when the additional gas injections were made.

4. Line 262: The authors state that particles only become well-mixed in their chamber after 60 to 90 min. A figure showing how the aerosol number/volume concentration changes as a function of elapsed time after aerosol injection into the chamber should be used to back up their claim.

5. Line 278: The authors state that O3 injections took over 3 days. This sentence does not make sense. Assuming that the chamber was run as a “batch reactor” in this experiment, I would expect their 9 m3 chamber to be completely deflated by day 3, even with minimal chamber leaks. If bag deflation did not take place, then what is the volume of the chamber on day 3? On a related note, the initial O3 concentration injected into the chamber can be estimated from the O3 injection time into the chamber and the O3 production rate of the lamp.

6. Line 303: The authors claim that biomass burning aerosols from white pine wood was used to enable comparison with other chambers. It is difficult to make direct comparisons when all the chambers used in this comparison use different aerosols to characterize their chambers. For example, the Cocker and Wang papers use ammonium sulfate seed aerosols to characterize particle wall loss rates in the Caltech and Guangzhou chambers, respectively. On a related note, it will be easier for the authors to characterize particle wall loss rates in their chamber if they used inert aerosols such as ammonium sulfate (similar to the Cocker and Wang papers) instead of biomass burning aerosols. This is because inert aerosols will not be subjected to changes in particle size as a result of gas-particle partitioning processes (unlike the biomass burning aerosols used by the

authors), thus making the calculation of particle wall loss rates for the different particle size bins easier.

7. Line 308: What are the initial aerosol volume and surface area concentrations used in these experiments?

8. Line 317: The sentence “While the rate constant varied over time, it was consistent during the beginning, middle and end of the experiment” does not make sense. I would expect a single rate constant for each particle size bin.

9. Line 319: The observation that “The particle wall loss rates decreased with increasing particle diameter…” should be demonstrated using a figure showing how the particle wall loss rates change with particle size (similar to the one shown in Fig. 1 of Loza et al. (2012)).

10. How does the particle wall loss rates change with chamber RH and temperature? It will be worth knowing this, especially in the case of temperature since the authors showed in Fig. 8 that the chamber temperature can increase by ~9 C during a 5 h experiment when the chamber lights are switched on.

11. Line 321: It seems more appropriate to compare particle wall loss rates at different particle sizes for the different chambers, as opposed to at just one particle size (100 nm).

12. Were routine particle wall loss experiments performed (e.g., weekly, monthly experiments)? How reproducible are the particle wall loss rates for the different particle sizes?

13. Lines 329 to 334: The discussion about modeling particle wall as a second order process should be supported with figures showing the fits.

14. Line 336: Previous papers have already determined that coagulation plays an important role when large aerosol concentrations are used. See Nah et al. (2017).

15. There should be some discussion on how the authors plan to correct for particle wall loss in their experiments. For example, do they plan to use size-dependent particle wall loss rates (see Loza et al. (2012), Nah et al. (2017)), or the average loss rate of the total aerosol mass or number concentration (see Carter et al. (2005), Pathak et al. (2007), Pierce et al. (2008))?

16. Lines 339 to 356: These two paragraphs don't add any value to the discussion. They seem to belong in the intro.

17. Line 358: Pierce et al. (2008) is not the only paper that studied particle wall loss rates (see references in point 13). Please cite accordingly.

16. Line 401: No results were presented in section 3.5 (Soot generation and characterization). I don't see the point of this section.

Minor comments:

1. Line 86: Should be "As opposed to field measurements"

2. Line 102: Should be "white pine wood"

3. Line 389: There should be a space after (Burkholder, 2015). Also, it should be "Burkholder et al."

4. Fig. 2: The legend is ambiguous. What do "Chamber1, Chamber2, Chamber3 and Chamber4" in the legend refer to?

References:

Carter, W. P. L., Cocker, D. R., Fitz, D. R., Malkina, I. L., Bumiller, K., Sauer, C. G., Pisano, J. T., Bufalino, C., and Song, C.: A new environmental chamber for evaluation of gas-phase chemical mechanisms and secondary aerosol formation, Atmos. Environ., 39, 7768–7788, doi:10.1016/j.atmosenv.2005.08.040, 2005.

Loza, C. L., Chhabra, P. S., Yee, L. D., Craven, J. S., Flagan, R. C., and Seinfeld, J. H.: Chemical aging of m-xylene secondary organic aerosol: laboratory chamber study, Atmos. Chem. Phys., 12, 151–167, doi:10.5194/acp-12-151-2012, 2012

Nah, T., McVay, R. C., Pierce, J. R., Seinfeld, J. H., and Ng, N. L.: Constraining uncertainties in particle-wall deposition correction during SOA formation in chamber experiments, Atmos. Chem. Phys., 17, 2297–2310, https://doi.org/10.5194/acp17-2297-2017, 2017.

Pathak, R. K., Stanier, C. O., Donahue, N. M., and Pandis, S. N.: Ozonolysis of alpha-pinene at atmospherically rele- vant concentrations: Temperature dependence of aerosol mass fractions (yields), J. Geophys. Res.-Atmos., 112, D03201, doi:10.1029/2006jd007436, 2007.

Pierce, J. R., Engelhart, G. J., Hildebrandt, L., Weitkamp, E. A., Pathak, R. K., Donahue, N. M., Robinson, A. L., Adams, P. J., and Pandis, S. N.: Constraining particle evolution from wall losses, coagulation, and condensation-evaporation in smog- chamber experiments: Optimal estimation based on size distribution measurements, Aerosol Sci. Tech., 42, 1001–1015, doi:10.1080/02786820802389251, 2008.