

Interactive comment on “Design and characterization of a smog chamber for studying gas-phase chemical mechanisms and aerosol chemistry” by X. Wang et al.

Anonymous Referee #2

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The authors attempt to describe the design and characterisation of a new smog chamber for studying gas-phase chemical mechanisms and aerosol chemistry. The manuscript suffers from a serious lack of details and discussion in several places. It can be considered for publication in AMT, if the authors address the following comments and concerns.

Main comments:

Page 7737–7739: Generally, the introduction is too short and does not provide a good enough overview of the range of existing chambers and their main features. This is

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not to ask for a literature review of existing chambers, but to provide an overview of the main design factors that the authors learnt from the literature and attempted to implement or avoid in their new design.

Page 7738, lines 9 – 20: this part of the introduction does not really explain the problem or the solutions and needs to be elaborated up on.

Page 7745: no discussion was provided at all for the implications of the data presented in Figure 2a for experiments at low/high temperatures? Does this mean that for experiments of this type, injection of chemicals has to be delayed until the target temperature is reached? If not, how would you deal with the data during the period of temperature stabilisation?

Page 7745: the discussion of “dilution” is very abstract. Did the authors account for wall loss rate of the tracer compounds in your evaluation of the dilution effect? How would high volume sampling lead to dilution if you are not supplementing the chamber with any additional air?

Page 7746: The discussion of the light spectrum and its suitability to the application of simulating atmospheric chemistry is currently absent from the manuscript. This needs to be included. The chosen lights account for part of the solar spectrum. What potential implication could this have on the gas phase and aerosol composition compared to the atmosphere?

Page 7747: Although the discussion mentions the size dependence of the particle wall loss, the characterisation was only provided using the total particle number ignoring the size dependence! No discussion was provided!

Page 7749: No discussion is provided for the clear model over-estimation of NO₂. This should be provided in the revised manuscript.

Page 7750: again the discussion of the results is very brief and does not provide enough information about the other datasets it is being compared to. Were they all

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also ozonolysis under dry conditions? did any of the studies involve photooxidation? did any of the studies use OH scavenger or use seed particles?

Page 7750, line 12: SOA particle density is quite often assumed to be in the range of 1.2 to 1.4 g/cm³. A density of 1.3 g/cm³ was determined for α -pinene SOA in previous studies (e.g. Bahreini et al., 2005, Alfarra et al., 2006). In fact, the manuscript already states that the Saathoff et al., study applied a factor of 1.25 g/cm³. This has an effect on the yield calculations and should be included. Alternatively, the mass measured by the AMS (if available) should be used for the yield calculation.

Page 7751, lines 4-5: The claim made here, and in other parts of the manuscript, that the facility can provide valuable data on aerosol chemistry has not been supported by any data in the current version of the manuscript. Only demonstration of gas phase chemistry and aerosol particle formation were illustrated. This has to either be supported by data or the text should be modified to reflect this fact.

Other comments and minor corrections:

Page 7738, line 26: change “for long duration” to “for a long duration” or “for long durations”.

Page 7741: clean air at flow rates of 45 and 200 lpm is mentioned; a blower with flow rate of 1 m³/min is also mentioned. Please clarify these and how they are used!

Page 7744, line 17: add Aiken et al., (2007, 2008) as the correct reference for elemental ratio measurements by HR-TOF-AMS

Page 7744, line 19: change “ammonia” to “ammonium”

Page 7747: why did propene was not lost to the walls, while NO , NO₂ and O₃ did?! Explain or discuss.

Page 7747: How did the seed particles mode increase from 150nm to 200nm in the absence of condensation? Please provide an explanation or discuss.

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Page 7747: did you mean “median” diameter or “mode”? not “medium”?

Page 7750, line 10: did you mean in the presence or absence of seed particles? Please clarify. If it was in the presence of seed, then the details should be included in Table 6.

Page 7751, Line 2: Change “We have built an indoor smog chamber ever largest in China..” to “We have built the largest indoor smog chamber in China. . .

Table 1: DL for HR-TOF-AMS have been published and documented for the different chemical species that can be measured by this instrument (see DeCarlo et al., (2006) for details). I suggest that DL for organics is quoted here.

Table 3: The volume of the GIG-CAS chamber should be changed from “3” to “30”

References:

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DeCarlo, P. F., Kimmel, J. R., Trimborn, A., Northway, M. J., Jayne, J. T., Aiken, A. C., Gonin, M., Fuhrer, K., Horvath, T., Docherty, K. S., Worsnop, D. R., and Jimenez, J. L.: Field-deployable, high-resolution, time-of-flight aerosol mass spectrometer, *Analytical Chemistry*, 78, 8281-8289, 2006.

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