General Comments

This paper describes the characterization and application of a mobile apparatus for investigating particle formation and transformation in the atmosphere. The apparatus consists of two identical chambers, operated in dynamic mode, coupled to a range of instruments for measurement of particle number and size, ozone, volatile organic compounds etc. The twin-chamber design is novel and enables the influence of specific parameters, e.g. ozone concentration and light intensity, to be studied. Some characteristics of the chamber such as residence time and wall losses are characterized and the apparatus is tested in both urban and remote environments. This work fits nicely within the scope of Atmospheric Measurement Techniques, however, significant improvements, in line with the comments provided below, are needed before this paper is suitable for publication.

Major Comments

1. The paper can be hard to follow at times. The language is often awkward and leads to confusion in many places. I strongly suggest that the authors enlist the services of a native English speaker to help improve this aspect.

2. It is difficult to place the work in context because other or existing approaches, e.g. simulation chamber studies, are not covered in sufficient detail. The authors need to outline some examples of alternative set-ups and their advantages/disadvantages in the Introduction section. In this respect the authors also need to make it clear that they have built a portable apparatus for investigating the formation and transformation of ambient particles. I think the use of "simulation chamber" in the title and in parts of the text is misleading, since simulation chamber experiments normally involve laboratory-generated particles and not ambient atmospheric particles.

3. The characteristics of the chambers are not reported adequately. Although residence time and wall losses have been measured, other key characteristics normally reported for new simulation chambers include light intensity inside the chamber and leak rate (e.g. Wang et al., 2011). Indeed, ETFE Texlon is not a typical material for simulation chambers, and a spectrum of the transmitted light intensity, or an estimate of the photolysis frequency of NO_2 and O_3, would be very useful.

4. There are a number of concerns about particle losses in the chambers (section 3.2.2) that need to be addressed and explained.
    (i)   Why is the particle loss rate so high (up to 100 times that observed in large simulation chambers)? Does the loss rate change significantly if the flow rate is changed? Some comments are also required to compare the performance with other apparatus.
    (ii)   Around 50% of the particles are deposited in the chamber. During the tests, the twin-chambers were operated in the field for many days and substantial amounts of particulates will be deposited on the walls of the chamber. How does this affect the measurements?
    (iii) Finally, and importantly, the fit provided in figure 5 seems to be surprisingly poor. Why? Can it be improved?

5. There are a number of concerns about the measurements performed under enhanced ozone concentrations.
   (i)   A pen ray lamp is used to provide the UV radiation necessary for production of O atoms and hence O_3. However in section 4.1, it is stated that the lamp is located in the chamber, while in section 4.2.1 the lamp is mounted at the inlet to the chamber. Please clarify.
   (ii)   It is not clear whether the chamber and its contents are exposed to the UV radiation. If it is, what about the effect of UV light on the particles and gaseous components? Indeed, the combination of ozone and UV radiation from mercury lamps is often used to clean simulation chambers and photoreactors. The process results in the generation of particles from processes occurring at the surfaces of the chamber. How can the authors be sure that the observed increases in particle number (Figures 7, 11, 12) are not due to release of particles that have been deposited at the chamber walls?
   (iii) Addition of ozone increases particle number. But how does the size distribution change? Does the mode of the original/ambient particles change?
   (iv)   Why was such a high concentration of ozone (700 ppbv) used in the first ozone enhancement experiment (section 4.1.1)? The authors use the result of this experiment to state that a 10% increase in ozone mixing ratio would result in a 30% rise in particle number (lines 17-26, page 5972). How can the authors justify giving such a definitive statement from just one experiment performed using unrealistic levels of O_3?
   (v)   The ozone mixing ratio is quite erratic in figure 11, but much more stable in the dark, Figure 12. Why is this?
6. In section 4.1.2, it is mentioned that the temperature of the chambers can reach 50°C during the day. Surely this would have an effect on the gases and particles as they pass through the chamber. Did the authors try to measure the particles and gases before and after passing through the chamber? Was any evaporation of the particles observed? Did VOC concentrations change?
7. There are very few references to the literature throughout the whole article, especially in the Results and Discussion sections.

Minor Comments
1. Page 5959. I think that the title does not make it clear that the work is about a portable twin-chamber apparatus for investigating particle production in the atmosphere and should be changed.
2. Page 5960. The abstract needs improvement. The first three sentences are not needed. The quoted deposition rate is for gases, but this is not stated. The deposition rate for particles in not mentioned. Information on the some of the tests, e.g., effect of ozone, solar radiation and VOC addition could be explained more clearly.
3. Page 5961. IPCC references are not provided in the list at the end of the paper.
4. Page 5962. More references should be made to existing efforts in the area, see Major comment 2.

5. Page 5965. Bourtsoukidis et al (2012) is not in the list of references.
6. Page 5966-5977, Section 3.1. The residence time is determined using flow rates between 10 and 20 L min^{-1}. Why choose these flow rates? The residence time is 26 minutes. Is this long enough to adequately study atmospheric processes? If not, what are the restrictions or disadvantages caused by this residence time?
7. Page 5967. Seinfeld and Pandis (2006) is not in the list of references.
8. Page 5968 and 5969: This section on particle deposition rate, and particularly the fitting procedure, is poorly explained and needs clarification. See also Major Comment 4.
9. Page 5969: The particle loss in COMPASS 1 and COMPASS 2 is slightly different, but was expected to be the same, as observed for gases. This is partly attributed to "non-stable initial aerosol". Was the measurement repeated with "stable aerosol"?
10. Page 5970: not September 2013, unless the authors are time travellers!
11. Page 5970: The particle formation process (figure 6, line 10) is not very clear and it does not look like a "banana plot". The possible role of traffic emissions is mentioned, but this is speculation. PAKs= PAHs?
12. Page 5972. There is no reference to Figure 8 in the text.
13. Page 5972. Reference is made to a scatter plot (line 22), but this is not shown in the manuscript.
14. Page 5973. Reference is made to the possible of role of OH radicals in particle formation/growth, but what is the evidence for this?
15. Page 5974, lines 4-5. Erroneous statement. The ozone mixing ratio was not maintained above 100 ppbv. Why is the ozone signal unstable, especially compared to that observed in the dark experiment (Figure 12)?
16. Page 5974, lines 23-25. Erroneous statement. The particle number concentration in Figure 11b does not exceed 10^5 (line 25).
17. Page 5975 and 5976. The section titled "Phase II" is somewhat hard to follow and should be made more clear.
18. Page 5979, lines 23-25. It is stated that deposition is a minor process. But the deposition rate is around 50% for small particles (50 nm diameter)!
19. Page 5980, lines 9-11. This is overstating the importance of findings from one or two test experiments.
20. Page 5987, Table 3. The significant changes that are highlighted in bold are somewhat selective. E.g., why is acetaldehyde highlighted, but methyl salicylate is not?
21. Page 5988, Table 4. The significant changes that are highlighted in bold are somewhat selective. E.g., why is formaldehyde highlighted, but methyl salicylate is not?

References

Wang, J., Doussin, J. F., Perrier, S., Perraudin, E., Katrib, Y., Pangui, E., and Picquet-Varrault, B.: Design of a new multi-phase experimental simulation chamber for atmospheric photosmog, aerosol and cloud chemistry research, Atmos. Meas. Tech., 4, 2465–2494, doi:10.5194/amt-4-2465-2011, 2011.