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Comment

***Interactive comment on* “Chemistry of new particle growth in mixed urban and biogenic emissions – insights from CARES” by A. Setyan et al.**

Anonymous Referee #2

Received and published: 22 March 2014

This paper discusses aerosol observations during June 2010 at two Californian sites: an urban site in the Sacramento, CA and a rural site about 40 km northeast of Sacramento. The observations focus on the growth of newly formed particles observed on several days at both sites, the connections between the sites and the chemistry of the particles. The new particle growth events are found to be associated with the transport of the aerosol from the urban area towards the rural site. The authors suggest that most of the growth was due to anthropogenic SOA, and there is a link between the growth event and the presence of amines in the particles. The consistency between the AMS mass concentrations and MPSS volume concentrations down to near 30 nm VAD is remarkable (Fig. 8). The presentation is well done. I have a few minor comments for the authors.

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Specific comments:

1) I believe that SMPS is a trade mark of TSI, and thus the authors may wish to avoid its generic use here. Why not use MPSS?

2) Was the AMS lens a standard lens or is it a new design? It is unclear from Setyan et al (2012). What is the reference for its characterization?

3) Page 2015 – Are all the distributions for which you are estimating the mode diameters log normal?

4) Page 2052, line 25 - Page 2053, line 5 – How do your growth rates depend on your somewhat arbitrary definition of “when the growth significantly slows down”? Increased biogenic precursor concentrations could result in larger growth rates. The growth rate in a biogenic environment also depends on the volatility of the condensing material, a point discussed by Riipinen et al (ACP, 2011), Pierce et al (ACP, 2011), Pierce et al (ACP, 2012) and most recently in a Nature publication. These additional points should be mentioned here.

5) Page 2053-2054 and Figures 2-4 – It is worth pointing out here that the delay between the two sites and the absence of particles smaller than 20 nm compared with T0, suggests that the particle nucleation occurred much near and upwind of T0 and not close to T1. In other words, the banana observed at T1 was likely independent of the emissions in the T1 area and mostly dependent on the emissions near T0 and upwind of T0. That is of course consistent with your general conclusion that the growth was dominated by anthropogenic precursors.

6) What about the seemingly independent mode bounded by about 11am and 5 pm and 60-100 nm in Figure 2? Was that common during the NPE days, and how did that mode influence your estimate of the composition of the 40-120 nm particles?

7) Page 2057, lines 12-13 – How were the cases of “dominate biogenic influence” derived?

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8) Page 2057, lines 21-23 and Fig 7c – Despite the highest temperatures from 10am to 4pm, a 10% increase in biogenic SOA across that time is sensible compared with previous observations of BSOA (e.g. Slowik et al., 2010; Pierce et al., 2012). The spatial scales of anthropogenic and biogenic emissions are so much different, and your results seem to be an excellent demonstration that the anthropogenic components dominate on smaller scales.

9) Section 3.3 and Figure 10 – Was SO₂ measured? Figure 10 appears to need an “(a)” caption.

10) Page 2060, lines 6-9 – I am confused by your apparent conclusion in this sentence. Are not sunny days those that would have the solar radiation peak about noon?

11) Page 2061, lines 5-15 and page 2062, lines 14-16 – If your results show that biogenic SOA was a small contributor to the growth, what is the basis for saying that growth was promoted by the interaction of urban and biogenic emissions?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 2043, 2014.

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