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Interactive comment on "Chamber simulation on the formation of secondary organic aerosols (SOA) from diesel vehicle exhaust in China" *by* Wei Deng et al.

Anonymous Referee #2

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Deng et al present measurements of secondary organic aerosol (SOA) formation from dilute diesel exhaust in a smog chamber. The manuscript is topically relevant to ACP, and the journal has published several similar papers in the past (e.g., Chirico et al 2010, Gordon et al 2014a and b, Nordin et al 2013). However it is not ready for publication in its current form, and needs significant revision.

General comments: My primary criticism is that this manuscript does little to differentiate itself from previous similar works (e.g., Chirico et al 2010, Gordon et al 2014a and b, Nordin et al 2013, and others), and does not seem to offer much in the way of new information or scientific insight. The authors argue that the value of this manuscript lies in the fact that this is the first such study using Chinese diesel engines, which have

fewer pollution controls than modern US and European diesels. I do not find this a compelling argument for publication, as both Gordon et al and Chirico et al conducted nearly identical experiments with diesel vehicles without after treatments such as DOC or DPF.

The authors also argue that the higher SOA production observed from their engines compared to Gordon et al and Chirico et al is a significant result. However, this could merely be a result of less dilution of the exhaust in the smog chamber and resultant partitioning during oxidation. Figure 2 shows that the OA concentration in the chamber after exhaust injection was 50 ug/mˆ3. This means that the chamber had ample POA for SOA to partition into, and oodles of vapor available to oxidize and form SOA. My impression is that previous similar studies worked with much lower POA concentrations, and therefore produced less SOA. The authors have done nothing to convince me that the excess SOA formed in their experiments is the result of higher SOA formation potential of the exhaust rather than higher initial concentrations.

The results presented in the manuscript show that a significant amount of SOA is formed during photo-oxidation of dilute exhaust, and that the OA becomes more oxidized during oxidation. This is not new news - Sage et al showed this exact behavior in ACP in 2008 (albeit with a laboratory-scale diesel engine), and the result has been repeated multiple times with emissions from many sources (gasoline engines, diesel engines, aircraft, etc).

While I think the underlying data are sound, and the methods appropriate, simply regurgitating previously-published experiments should not pass muster for publication in ACP. The authors need to show how their study adds substantially to the existing state of knowledge.

Specific comments:

- The manuscript needs a thorough English grammar check.

- Is Figure 1 comparing apples to apples? E.g., does it compare the w=0 case from the present study to w=0 from previous work (or w=1 to w=1)?

- Line 330-333: Increase in m/z 59 during oxidation could also be acetone

- Figure 4 - which line goes with which axis?

- The authors only seem to use experiment 8 for representative data. This makes me wary about results from the other experiments.

- Figure 8a - what does SOA mean? Is this the total OA at the end of the experiment or was SOA separated from POA somehow?

- Fig 8b would be better if it showed VK plots for more than one experiment.

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