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***Interactive comment on “Model evaluation of
NO₃ secondary organic aerosol
(SOA) source and heterogeneous organic aerosol
(OA) sink in the Western United States” by
J. L. Fry and K. Sackinger***

Anonymous Referee #2

Received and published: 11 April 2012

Model evaluation of NO₃ secondary organic aerosol (SOA) source and heterogeneous organic aerosol (OA) sink in the United States J.L. Fry and K. Sackinger Overall comments: This paper presents a scientific issue of atmospheric importance which requires thorough investigation and is relevant to scope of ACP. But the methodology of this analysis needs to be further clarified. Also, more importantly, since all relevant calculations are done offline, the authors should be careful of interpreting these calculations in terms of their atmospheric significance. I have several suggestions below which need to be addressed before the manuscript could be accepted for publication in ACP.

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1. Page 5194: Why was the model run for January May and August of 2008, but October 2004? How different were emissions during these 2 years?
2. Did SOA yield from NO₃ get added to WRF-Chem variables oc_a01, oc_a02, oc_a03 and oc_a04? This needs to be mentioned.
3. The yields in Table 1 for SOA from NO₃ are chosen for 10s of ppb of reactants which could be an upper bound. The calculations in Table 3 for mass yield of 5% may be relevant under most atmospheric conditions. However, the 95% mass yield is clearly unrealistic, not supported by measurements. There should be some discussions about atmospheric relevance of mass yields used in this study.
4. Page 5197: To determine products of heterogenous reaction sink of OA due to NO₃ radical, the authors assumed composition of starting aerosol material as 35% aliphatic. But in Table 3 97% aliphatic aerosol gives higher sink rate than their unmodified sink. Why?
5. Equation 7: I agree that using partitioning theory to determine the gas-particle distribution of products is much better and far less confusing to readers than using a 50% cutoff approach.
6. Page 5198: The reaction products are divided into bins with similar vapor pressures reported in Torr. It would be better to give the vapor pressure range in ug/m³. Also what was the bin spacing used?. On page 5201 there is an indication of using 1.5 orders of magnitude bins. But then this should be mentioned here.
7. Page 5198: It is also confusing that if the authors use very high carbon number of 27 carbon atoms and standard deviation of 9 carbons for all products. Is there any support for this assumption either from laboratory studies or from Master Chemical Mechanism? If so, it should be explicitly described.
8. Page 5202 and Table 3: OA sink rates are shown to be several orders of magnitude lower than formation rates. But given that all these calculations are done offline without

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consuming ROG, NO₃ radical or OA in the model at each time step, would this reasoning still hold? There has to be some discussion on the relevance of these calculations done offline.

9. Page 5204: The calculated peak night-time OA prediction from NO₃ are shown to be 23–46 ug/m³. The authors mention that it is an overestimate, due to the crude lifetime analysis, mainly because the model does not consume NO₃ radical, ROG or OA at each time step. But, at the beginning of page 5205, the authors suggest that large magnitude of SOA source due to reaction with NO₃ radical is likely to be significant both in terms of OA production and NO₃ radical sink. These 2 statements are contradictory.

If OA is overpredicted, how can it be a large magnitude of SOA source due to reaction with NO₃? The only way to assess this nitrate SOA source is through online WRF-Chem calculations. If the authors wish to make any statement about importance of NO₃ initiated SOA source during night-time, they should show it through online WRF-Chem predictions using relevant yields from literature.

Secondly, they have argued on page 5202 that NO₃ radical sink due to heterogeneous reaction with OA is insignificant, because NO₃ rapidly photolyzes after sunrise. Also, the homogenous sink of NO₃ radical due to reaction with gas phase organic species such as alkenes is already included in current photochemical mechanisms such as SAPRC. So their argument here needs to be revised and clarified.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 5189, 2012.

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