

## ***Interactive comment on “Limited effect of anthropogenic nitrogen oxides on Secondary Organic Aerosol formation” by Y. Zheng et al.***

**Anonymous Referee #1**

Received and published: 28 September 2015

In this manuscript, the authors use a global chemical transport model to study the effect of changing NO<sub>x</sub> emissions to SOA formation, using an updated volatility basis set scheme. The predicted SOA concentrations showed good agreement with observations by aerosol mass spectrometers, both in the temporal and spatial profiles. The global SOA burden was shown to be quite insensitive to NO<sub>x</sub> changes, implying that future emission control policies on NO<sub>x</sub> may not be effective in controlling global SOA. The manuscript is clear and well written, and fits within the scope of ACP. The comparison with observations demonstrates that the model results are robust and is a strength of this manuscript. I have some questions about the interpretation of the results, but they can be easily addressed with some minor revisions. I recommend publication of this manuscript in ACP.

C7326

Main comment:

My only major comment is the interpretation of why changes in NO<sub>x</sub> do not lead to significant changes in global SOA. From Figs. 8 and S4, it seems to me that there simply is no significant change in beta. Since SOA yield depends critically on this branching ratio, if there is no change in beta, there is no change in relative SOA formation. To me, that seems to be the most straightforward explanation of the model results. As I understand, most areas are not NO<sub>x</sub>-limited in the context of SOA formation, since NO<sub>x</sub> → HO<sub>2</sub>. So a 50% reduction in NO<sub>x</sub> (or NO) would translate to a very change in beta. This point is further highlighted by Fig. 10, which shows no changes in SOA contributions for essentially all of the pathways. Therefore, the proposed reasons detailed in Section 5 are not as important as the lack of change in beta. I believe that if you go to greater reductions in NO<sub>x</sub> (~90%), one might start to see changes in relative SOA formation. This does not change the conclusions of the manuscript (50% reduction in NO<sub>x</sub> leads to no change in SOA), but I believe the reason for this is simply current NO<sub>x</sub> is too high for HO<sub>2</sub> pathways to compete, even with a 50% reduction in NO<sub>x</sub>.

Other comments:

- Table S1 and S2 show that the enthalpies of vaporization are different between the 2-product scheme and the VBS scheme. How much the improved agreement is due to the changes in enthalpies of vaporization?
- What is the fossil content in SOA? Can this be used as an additional constraint for the model?
- How good is the assumption that POA == HOA and SOA == OOA? Could that lead to biases in predicting primary/secondary split and errors in POC estimation as described in Section 4.2.1?
- Equation (2): the ratio beta should include NO and HO<sub>2</sub> concentrations, and it is only because we expect  $k_{\text{RO}_2+\text{NO}}$  to be similar to  $k_{\text{RO}_2+\text{HO}_2}$  that it can be simplified to

C7327

equation (1)

- Pg. 23234 Line 25: add comma between Bakersfield and California, and after California

- Pg. 23240 Line 17: How is O:C ratio important in this study? I don't see any comparison of modeled O:C to observed O:C. Is it used to calculate OA:OC?

- Pg. 23243 Line 4 and Tables S1 and S2: it seems that the average concentrations of OA is 0.3 – 5  $\mu\text{g m}^{-3}$  (Table 6). I suggest using yields at a lower OA concentration instead of 10  $\mu\text{g m}^{-3}$

- Table 4: remove f from fVBS\_agHigh

- I suggest including a table of abbreviations because there are many of them used in this manuscript and a reader outside of the field will be easily confused

---

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 23231, 2015.

C7328