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

Anonymous Referee #1

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In this manuscript, the authors uses a global chemical transport model to study the effect of changing NOx 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 NOx changes, implying that future emission control policies on NOx 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.

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Main comment:

My only major comment is the interpretation of why changes in NOx 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 NOx-limited in the context of SOA formation, since $\text{NO} \gg \text{HO}_2$. So a 50% reduction in NOx (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 NOx ($\sim 90\%$), one might start to see changes in relative SOA formation. This does not change the conclusions of the manuscript (50% reduction in NOx leads to no change in SOA), but I believe the reason for this is simply current NOx is too high for HO₂ pathways to compete, even with a 50% reduction in NOx.

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 HO₂ 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 simplified to

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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 ug m⁻³ (Table 6). I suggest using yields at a lower OA concentration instead of 10 ug 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.

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