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Atmospheric
Chemistry
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Discussions

Interactive comment on "Global modeling of organic aerosol: the importance of reactive nitrogen" *by* H. O. T. Pye et al.

Anonymous Referee #1

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This manuscript presents novel global modeling results (GEOS-Chem) to assess the global and North American effects of NOx and NO3 on secondary organic aerosol formation. The incorporation of more detailed RO2/NO vs. RO2/HO2 competitive chemistry and nitrate radical (NO3) initiated oxidation is an important improvement to global SOA modeling and this benchmark study provides a useful orientation to the magnitude and regional variation in the contribution of these mechanisms to aerosol suppression or enhancement. The model experiments are well conceived and clearly described. I highly recommend the paper be published in ACP after these minor comments are addressed.

Specific comments:

Suggest calling out NOx and NO3 specifically in title, e.g., "…reactive nitrogen (NOx

and NO3)"

End of section 2.2 (p. 21265 lines 6-11): Would be informative to cite approximate aerosol yields of various compounds here to give reader a sense of relative importance.

SOA parameterization (section 2.3): General comment: Your two-way arrows emphasize that gas/aerosol partitioning of all these species responds instantaneously to changes in overall aerosol loading. Yet some recent chamber studies (e.g. Leungsakul, et al. 2005) show that aerosol-phase species must have significantly (2 orders of magnitude!) lower effective vapor pressure than would be predicted for those structures. This seems to argue for oligomerization/polymerization. Is there any way to parameterize this in your model, or is it not well enough experimentally constrained? Perhaps just a comment on how this might affect ultimate SOA loading estimates.

p. 21267 line 5 "ozonolysis"

Section 2.3.1: I don't find text in here describing how NO3+terpenes yield (seemingly a pivotal parameter in your modeling) is fit using a single b-pinene measurement from Griffin 1999. Please discuss. For example, another recent measurement of NO3 b-pinene yield was much higher (Fry et al 2009, 50% at < 10 ug m-3) … how uncertain is this yield parameter? Are there other measurements in the literature suggesting it might be higher/lower?

p. 21268 lines 10-12. Is this because the "high-NOx" limit was completely unrealistically high?

Fig 2: (discussed section 3.2). Could you redo this figure so that rather than each hydrocarbon summing to 1, the entire plot sums to 1? Or something similar to ease comparison of relative importance of different HC classes as well as oxidants?

p. 21271, line 5: You use a global avg OA loading of 1.5 ug m-3. Might be worth re-emphasizing here that this is highly regionally variable (maybe mention something about how different this would look if you took a high or low background OA number?)

More generally, there are some interesting “hot spots” on the August 2000 plots (e.g., Idaho?!) Please comment briefly on this in the text. Is this forest fires?

p. 21271, lines 15-17: This is completely dependent on your assumptions about the volatility of nitrate products. If e.g. oligomerization is occurring, this could look dramatically different. How would this conclusion change if you lowered the volatility a couple orders of magnitude?

p 21275, lines 16-21. Again, some caveats might be worth mentioning here. How well-constrained do you consider the 4% / 26% yield numbers to be?

p. 21278 line 27: “levels, even”

Fig 1 caption: “Schematic”

Fig 2: please see comment above for a suggested alternative presentation of this data

Fig 4: cite Tref in caption?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 21259, 2010.