

Interactive comment on “A comprehensive organic nitrate chemistry: insights into the lifetime of atmospheric organic nitrates” by Azimeh Zare et al.

Anonymous Referee #2

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This is a nice paper that looks into details of organic nitrate chemistry, with recent new understanding on this topic. The authors develop a new mechanism in WRF-Chem model and compare model simulations to observations in Southeast US during SOAS 2013. They find that their model is generally in good agreement with observations, assuming organic nitrates is short lived with a lifetime of 2-3h. The paper is well written. I would recommend publication on ACP after the following comments are addressed:

1. As organic nitrates are largely driven by biogenic VOCs, it is important for authors to evaluate isoprene and monoterpene concentrations in their model. Isoprene and monoterpene measurements have been shown in Fisher et al. [2016]. I assume that

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they are available for comparison.

2. The authors have done a detailed comparison with Fisher et al. [2016]. It is important to point out that Fisher et al. [2016] assumes a 9% yield for first generation isoprene nitrates, while it is assumed 11.7% in this paper. Given the higher yield and slower aerosol hydrolysis in this study, could authors comment on why these two studies show similar amount of total organic nitrates in their models?

3. The authors appear to have ignored another model study on this topic, Li et al. [2018]. It seems that Li et al. [2018] also did a detailed analysis on first- and second-generation isoprene nitrates using data collected in Southeast US. It might be worthwhile to compare this model to their results in details.

4. I would suggest that the authors include two review papers on this topic in the Introduction part, Carlton et al. [2018] and Mao et al. [2018].

5. It might be useful to mention vertical resolution of WRF-Chem, to help reader understand how well the model is representing nighttime boundary layer emission and chemistry.

6. Page 10, Line 25, “They showed total particle organic nitrates have a dominant contribution from highly functionalized isoprene nitrates containing between six and eight oxygen atoms.” Is this correct about the isoprene nitrates dominating particle organic nitrates? If not, then this should not be the reason for “the difference between the modeled and observed contribution of isoprene nitrates to total organic nitrates”.

Reference

Carlton, A. G., et al. (2018), Synthesis of the Southeast Atmosphere Studies: Investigating Fundamental Atmospheric Chemistry Questions, Bull. Amer. Meteorol. Soc., 99(3), 547-567, doi:10.1175/bams-d-16-0048.1.

Fisher, J. A., et al. (2016), Organic nitrate chemistry and its implications for nitrogen budgets in an isoprene- and monoterpene-rich atmosphere: constraints from air-

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craft (SEAC4RS) and ground-based (SOAS) observations in the Southeast US, Atmos. Chem. Phys., 16(9), 5969-5991, doi:10.5194/acp-16-5969-2016.

Li, J., et al. (2018), Decadal changes in summertime reactive oxidized nitrogen and surface ozone over the Southeast United States, Atmos. Chem. Phys., 18(3), 2341-2361, doi:10.5194/acp-18-2341-2018.

Mao, J., et al. (2018), Southeast Atmosphere Studies: learning from model-observation syntheses, Atmos. Chem. Phys., 18(4), 2615-2651, doi:10.5194/acp-18-2615-2018.

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