Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-52-RC2, 2016
© Author(s) 2016. CC-BY 3.0 License.

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Organic nitrate chemistry and its implications for nitrogen budgets in an isoprene- and monoterpene-rich atmosphere: constraints from aircraft (SEAC^{4}RS) and ground-based (SOAS) observations in the Southeast US" *by* J. A. Fisher et al.

Anonymous Referee #1

Received and published: 8 March 2016

This paper advances our understanding of isoprene and monoterpene degradation chemistry in a regional modelling framework. It specifically implements a range of updates to the isoprene and monoterpenes derived nitrates and compares the model results to those observed by field campaigns in the southern US. These observational datasets provide a uniquely comprehensive assessment of the species produced through this chemistry. The paper shows the role of a range of new processes notably the heterogeneous processing of RONO2 species.

Printer-friendly version

Discussion paper

This paper is comprehensive, well written and advances our understanding of this important area of atmospheric chemistry. My suggestion is that the paper is published but have one potential addition to the paper and a couple of minor comments below.

My major difficulty reading the paper is that there is little discussion of the impact of the addition of all of this chemistry on the wider composition of the atmosphere. How different are the North American concentrations of NOx, O3, PM, OH etc after all of this new chemistry has been added? How much difference does this make overall to the chemistry of the atmosphere? Should this chemistry be included in all modelling or is it of niche interest? This final summing up seems to be missing from the paper which makes it a little dissatisfying overall. The authors presumably have simulations with the old chemistry and the new chemistry or could readily perform a calculation with the isoprene / monoterpene nitrate chemistry switched off etc and a new section which provides some details of the overall impact would give the reader some sense of how important this is and whether they should care or not about the chemical detail described in the paper.

The authors gloss over a few of their mechanistic choices. They should put the chemical mechanism used in the work into supplementary material for both the gas, heterogeneous and aerosol phase chemistry and include more details of how they have changed absorption cross-sections from the IUPAC recommendation (presumably the quantum yield is 1?).

In a couple of places we are re-assured that the kinetic choices provide the best fit to the observational data ("Although simplified, we find this parameterization improves the model fit relative to the SEAC4RS and SOAS . . ...", "In any case, the choice of hydrolysis lifetime does not affect the concentration of gas-phase RONO2 species (because pRONO2 cannot re-partition to the gas phase in the model), and we find this value provides a reasonable match to AMS measurements of total pRONO2 at the surface during SOAS and SEAC4RS"). Could these results in the supplementary material so that the readers can judge these for themselves.

CC BY

I found sections 3,4,5 rather long for my taste. Would it be possible to put in some sub-section headings to help split those up and allow the reader to find specific bits of information more easily?

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-52, 2016.

Printer-friendly version

Discussion paper

CC BY