

Interactive comment on “Secondary Organic Aerosol (SOA) yields from NO₃ radical + isoprene based on nighttime aircraft power plant plume transects” by Juliane L. Fry et al.

Anonymous Referee #1

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The manuscript is original and very interesting to read. The authors tried to get the optimum out of the data, but still addressed openly the limitations of their approach.

From the viewpoint of raising interesting questions regarding the role of isoprene chemistry and isoprene NO₃ chemistry for SOA formation and interesting approaches to address these questions, the paper could be published after some minor revisions (most of it formal character, e.g. references in text and supplement, see below).

However the manuscript fails clearly short behind its title claim and from this point of view, I suggest to reject the manuscript, due to the major concerns following below. Since the authors have already done the best with their data in a positive sense, I

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guess major revisions would not make sense.

A way out could be a reformulation of the title of the manuscript away from “providing yields” (reliable numbers) to a more procedural character of “addressing an important issue with interesting approaches and possibly important findings”.

Major:

The authors convinced me that pRONO₂ and thus organic nitrate in plumes is enhanced and that may indeed relate to enhanced NO₃ concentrations (Figure 6). However, the paper does not really show that that increase of pRONO₂ is related to isoprene oxidation alone (Figure 5). While the reasoning of a single -ONO₂ group per organic nitrate molecules is an acceptable approach to derive molar yields, the scatter in Figure 5 casts doubts, if the increase of pRONO₂ is really related solely to isoprene oxidation. Herein the weak point is the limited number of data points. I don't say the authors are wrong, but one would need more observations to strengthen the case. I concede that the authors revealed an interesting phenomenon, interesting enough to pursue the ideas and go out and get more/better proof.

L: 651: In going from the molar yield to the mass yield the uncertainty - and speculations clearly indicated as such, though - become even larger. On one hand obviously two oxidation steps are needed to achieve condensable isoprene oxidation products, on the other hand NO₃ seems to be the only available oxidant. Oxidation of both double bonds should thus lead to dinitrates. If pOrgNO₃ would really isoprene dinitrates the estimated yield would drop from 27% to 18%, not so far away from the referenced value by Rollins of 14%. I can follow the authors that it is likely that pRONO₂ dinitrates could be hydrolysed, but why should hydrolysis stop after one group, why not hydrolysing every second -ONO₂ group or even both ONO₂ groups? Moreover, as far as I understand, Rollin's value is based in parts on observations of several hours in a large chamber. So reaction time cannot be an issue?! I agree that wall losses could be an issue, though, but wall losses are also less important in large chambers. There-

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for with the same right I could argue that Rollin's yield of 14% is correct and then ask where could the rest of organic nitrate come from. I follow the authors that inorganic nitrate can be excluded as source. Could it be that the organic nitrates arise from liquid phase or heterogeneous processes via NO₂, NO₃? Is anything known about such heterogeneous nitration processes? NO_X and NO₃ were by definition high in the plumes.

Actually if I really think about it the mass yield analysis adds not much beyond the molar yield considerations and an analogous plot would just reproduce Figure5 with slopes of 18% or 27%, depending only on the assumption if the isoprene nitrates bring in two or three times the molecular weight of isoprene itself.

L483/ Fig. 2: What also concerns me and this is again related to small number of cases: There are indeed correlations between PNO₃ and pRONO₂ and anti-correlation with isoprene, but pOrg NO₃ sometimes increases by the same amount in the absence of plumes (2:17.30AM, 2:22.00AM) and some plumes do not create OrgNO₃ despite lower isoprene (ca. 2:21.20AM).

Minor:

I315, Fig.S1: If I compare the SENEX data with actual data in the range of plumes and background the difference is more a factor of 2 than 1.6. Moreover, two of the plumes fall off line while all the background measurements correlate as all other data. Unfortunately, the exception of AMS performance(?) or UHSAS performance(?) for "just that flight" in addition weakens the case.

I suggest listing also PNO₃ in Table 1; that would help to link quickly oxidation strength and observed effect

Main text: references not in ACP format Replace "author et al. (author et al., year)" by "author et al. (year)"

Supplement: the literature is not assessable and given in bracketed format

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I59: review

I172: Xu et al. (2015)

I339: Fry et al. [47]

I657: no “N” in the formula, it is not clear that refer only to organic rest of the tri-hydroxynitrate

I418: I suggest to replace “number densities” with “concentration” in context of gases

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