This manuscript reported the measurements of organic nitrates and the NOy budget during the SOAS campaign. The authors estimated the NO3 radical concentration and then calculated the cumulative losses of NO3 radical. Based on the correlation between the cumulative losses of NO3 radical to terpenes and the measured particle-phase organic nitrate (ON), the authors concluded that the molar yield of aerosol phase monoterpene nitrate ranges from 23 - 44%. While the writing is clear, the conclusions are not very novel and not well justified. I recommend accepting manuscript after major revisions.

Major comments:

Firstly, regarding the organic nitrates, Xu et al. (2015a) already demonstrated the importance of biogenic VOCs (especially monoterpenes) + NO3 in the SE US. Furthermore, Xu et al. (2015b) systematically evaluated the contribution of ON to ambient organic aerosol at multiple sites and in all seasons in the SE US. These two studies are clearly relevant and should be discussed.

Secondly, the authors used the organic nitrate measured by AMS extensively in the analysis. However, how the ON is measured by AMS is not discussed in the text. It is a little misleading to say that "HR-ToF-AMS was used to measure submicron organic and inorganic nitrate aerosol composition" (page 16241 line 15 – 17) without further justification. If the concentration of ON is calculated based on the NO+/NO2+ ratio method proposed by Farmer et al. (2010), more details should be definitely included in the text. Specifically, the NO+/NO2+ ratio for organic nitrates appear to be dependent on the specific systems (e.g., it is about 5 for isoprene ON, but about 10 for monoterpene ON). Even for ammonium nitrate, the NO+/NO2+ ratio can also vary. What NO+/NO2+ values did the authors use for organic nitrates and inorganic nitrates specifically in the equation proposed by Farmer et al.? Xu et al. (2015b) used NO+/NO2+ ratio of 5 and 10 for estimating organic nitrate contributions in the SE US and the uncertainties associated with this method were discussed. Regardless of the choice of the NO+/NO2+ ratio for organic nitrates, when using AMS data along to estimate organic nitrate concentrations, the authors should explain their methods clearly and justify their approaches.

Thirdly, the authors estimated the molar yield of aerosol phase monoterpene nitrates by correlating the cumulative losses of NO3 radical to monoterpene to total aerosol ON. However, I have some concerns regarding the authors' interpretation.

(1) While the x-axis is the predicted losses of NO3 radical to monoterpene, the y-axis is total aerosol ON, which also includes isoprene ON. Thus, the slope cannot be interpreted as yield of monoterpene ON.

(2) In page 16247 line 26-28, the authors cited Lee et al. 2015 (not published) to argue that isoprene ON accounts for < 0.5% of total particle-phase ON. However, according to the abstract (page 16237 line 13-16), it seems like that 0.5% is only the fraction of C5H9NO5 in total aerosol ON, instead of the fraction of all isoprene ON in total aerosol ON. This discrepancy needs to be reconciled. In fact, how are isoprene ON identified from the CIMS measurements? Since Lee et

al. 2015 is not published, the authors need to provide more justifications about Lee et al. to help readers understand.

(3) The calculated yields (both ON yield and SOA yield) should be compared to the values in the literature. For example, Boyd et al. (2015) reported the ON yield from b-pinene + NO3. Fry et al. (2011) reported the yields from limonene + NO3.

Fourthly, section 3.3 is highly speculative. The authors totally ignored the BVOC concentrations. Centreville site is located in a forest, where BVOC concentrations are much higher than other locations.

Specific Comments:

1. Page 16241, line 1-2. Some discussions need to be better organized. For example, it is better to move this sentence after introducing GC-MS (page 16241 line 26-29). Another example is page 16243 line 7-9. The heterogeneous uptake of N2O5 is better discussed together with page 16245 line 1-10.
2. Page 16242 line 1-9. It would be helpful to include the size cuts of all instruments.
3. Page 16242 line 23-24. It would be helpful to show the detection limit of cavity ringdown instrument.
4. Page 16244 line 23-25. The authors concluded that half of the daytime NO3 losses are due to reaction with BVOCs. However, this conclusion is highly dependent on the jNO3 value. The description of jNO3 calculation is not clear. What's the uncertainty of this value? How sensitive is the fate of NO3 to jNO3? In addition, this conclusion is a little misleading. Even if half of NO3 radical reacts with BVOC in the day, what's the concentration of NO3 concentration in the day? Is BVOCs+ NO3 an important pathway for SOA formation during daytime?
5. Page 16246 line 12 - 25. This part needs to be better organized, since the authors jump from figure 6 to figure 7 and back to figure 6 again.
6. Page 16246 line 17 - 19. It is not clear how the conclusion is drawn. Do the authors have any evidence that the compounds shown in figure 7 are first-generation products?
7. Page 16247 line 7. Delete Lee et al.
8. Page 16247 line 26 - 28. How the isoprene oxidation products are differentiated from monoterpene oxidation products from the CIMS measurements? In addition, one cannot reach the conclusion that most of the isoprene ON remains in the gas phase just based on that observation that isoprene ON accounts for <0.5 of total aerosol ON. Both gas and particle phase isoprene ON data are required to investigate the partitioning.
9. Figure 5. How are the start and stop points selected?
10. Figure 8. Why do the authors use maxima in this plot?
11. Figure S2 is not optimal. Firstly, both y-axes should have the same scale to facilitate comparison. Secondly, a scatter plot would be helpful.

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