Interactive comment on “Aircraft-based observations of isoprene epoxydiol-derived secondary organic aerosol (IEPOX-SOA) in the tropical upper troposphere over the Amazon region” by Schulz et al

This paper reported an aircraft measurement in the tropical troposphere over the amazon region. The vertical profile of the main components in submicron aerosols were measured by a C-ToF-AMS. The authors focus their discussion on SOA formed through isoprene oxidation under low NO condition, i.e. isoprene epoxydiols-derived SOA (IEPOX-SOA) and organic nitrate formation. Vertical profiles of IEPOX-SOA and organic nitrate mass up to 14 km are new and very interesting. However, the analysis method of this paper has a very serious caveat which needs to be furtherly explored and addressed. I recommend a major revision due to the comments as below:

1) The entire analysis of this paper only depends on AMS measurement. To prove the deduction that the IEPOX-SOA can be formed in the upper troposphere. The authors should show evidences. E.g., what are the vertical profile of isoprene and NO, or even gas-phase IEPOX. The authors argued there is high organic nitrate formation due to NOx production of lighting. However, the high NO will inhibite IEPOX-SOA formation (Paulot, Crounse et al. 2009, de Sá, Palm et al. 2016, Liu, Brito et al. 2016). The addressed reason for organic nitrate formation conflicts with that for IEPOX-SOA formation. And the authors should show recalculate the PH after correcting the mass concentration of nitrate. See detailed information in the followed comment.
2) This paper reported the mass and ions measured with C-ToF-AMS, which gives an UMR spectrum. M/z 30 is mainly composed of CH_2O^+ and NO^+ ions, and m/z 46 is NO_2 and $CH_2O_2^+$. The contribution of CH_2O ions to m/z 30 (CH_2O_2 to m/z 46) is very high in areas strongly influenced by biogenic emissions. In the SE US, the aerosol composition of which is very similar to the Amazon area, UMR nitrate (with NO^+, NO_2, $CH_2O^+CH_2O_2$) is overestimated a factor of 2-3 than the high-resolution nitrate (true nitrate with NO^+ and NO_2) based on the default fragmentation table, due to underestimation of the organic (CH_2O and CH_2O_2) interferences (Hu, Campuzano-Jost et al. 2017) . Thus the nitrate mass concentration reported in this study are combination of organic and nitrates. As a result, the ratio of UMR m/z 30 and UMR 46 cannot be used to calculate organic nitrate mass concentration, unless the authors find a way to exclude the interference. A revised fragmentation table is suggested in (Allan, Morgan et al. 2014). However its suitability for Amazon area still needs to be evaluated. Similarly, the authors cannot really report the IEPOX-SOA increases with nitrate, which is probably organics. The statement in page 17 1-7 lines are wrong due to the reason mentioned here.
3) Page 20 line 15, tthere is no way that the calculated organic nitrate mass concentration based on Fry et al. (2013) can be higher than the total nitrate mass concentration measured from AMS. Because Fry et al. (2013) used a method base on splitting the

mass of total nitrate masses with differential NO/NO2 ratios from NH4NO3 and organic nitrate. Please check furtherly.

4) How are the vertical profiles of main components in submicron aerosols in this study compared to the amazon study reported in (Allan, Morgan et al. 2014). Also the It will be interesting if the authors can address the similarity and differences, especially for m/z 82 and f82. What is the new finding in this study compared to the previous ones.

Minor comments:

Page 7 line 12: please give details about the "a time-dependent cubic spline function was used to determine a detection limit for each data point"

Page 6 line 13: give full citation of Molleker et al. 2008.

Page 8 line 15-16: Hu et al. 2015 showed an f82 values from IEPOX-SOA and background sources in the appendix.

Page 8 equation 1, what is the f82 values of IEPOX-SOA used in this study to calculate the mass concentration of IEPOX-SOA.

References:

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