

## ***Interactive comment on “Airborne observations of IEPOX-derived isoprene SOA in the Amazon during SAMBBA” by J. D. Allan et al.***

**Anonymous Referee #1**

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The paper by Allan et al. presents in flight measurements of aerosol composition in the Amazon during 5 flights performed within SAMBBA. Aerosol composition is evaluated as function of flight altitude with a focus on potential markers of isoprene SOA formation. The paper is well written of interest to the readers of ACP. Before publication the following two major concerns need to be addressed.

Major comments: The analysis of the contribution of isoprene as SOA precursor is based on a single  $m/z$  peak ( $m/z$  82). As the authors correctly state this mass peak is observed from organic aerosol in a variety of laboratory and field measurements and not by itself specific. In particular it is also found in SOA from biogenic monoterpene emissions (e.g. Kiendler-Scharr et al., EST 2009). An unambiguous identification of isoprene SOA is thus not provided by the presence of  $m/z$  82 alone, even with the  
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fractional contribution of  $m/z$  82 exceeding typical values of 4%. Although the authors state that factor analysis of the in flight data did not lead to conclusive isoprene-SOA factors, they should attempt to strengthen their analysis by including other mass peaks found in SOA from isoprene. Experiments using fully deuterated isoprene as precursor have identified a number of more prominent ions in the SOA from isoprene (Kiendler-Scharr et al., 2012). Surely there is more information on AMS spectra in the literature from laboratory experiments producing SOA from isoprene at low NO<sub>x</sub> conditions that can be used in this context. This could also support the interpretation of the mass spectral differences between “fresh” and “aged” organic aerosol (Figure 6).

I am concerned with the interpretation that a higher contribution of  $m/z$  30 in some flights can be taken as evidence that organic nitrates play a role during these flights. If I understand correctly, the C-TOF instrument used here does not provide the mass resolution required to distinguish individual peaks on nominal mass  $m/z$  30? How can a contribution from organic ions ( $\text{CH}_2\text{O}^+$ ) be excluded?

Minor comments (in order of appearance in the manuscript):

Refer to table 1 when first mentioning the different flights discussed.

Using data from a flight where the orifice was partially clogged needs more caveats to be mentioned. How can one be sure that ratios are unchanged by this? Is there evidence that ratios discussed are independent of particle size and losses through the clogged orifice are independent of size?

The exclusion of the possibility to observe “excess ammonium” is too strong in my opinion. It was shown previously that at least oxalic acid is readily partially neutralized by  $\text{NH}_4$  in the particle phase (Mensah et al., 2011). Also recent discussions point towards a more complex role of  $\text{NH}_4$  in particle chemistry (e.g. Nguyen et al., 2013). In a forested region ammonia and amines may be crucial.

The discussion that inorganic matter during B749 and B750 may have been present as

solids seems unlikely in the context of a CE of 1 for all flights.

The discussion on the role of nitrogen and sulphur in organic aerosol is purely speculative and should be skipped.

In the discussion on vertical boundary layer profiles of Org82 gas-phase photochemistry is excluded as potential reason for the strong gradient based on the observation of constant actinic flux. Yet the temperature did change in the discussed region. Can T-dependent lifetimes of intermediate species play a role here?

References: Kiendler-Scharr et al., *Environ. Sci. Technol.* 2009, 43, 8166–8172; Kiendler-Scharr et al., *Atmos. Chem. Phys.*, 12, 1021–1030, 2012; Mensah et al., *Journal of Aerosol Science*, 42, 11-19, 2011; Nguyen et al., *Faraday Discussions*, p473, 2013

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