

Interactive comment on “Aircraft observations of water-soluble dicarboxylic acids in the aerosols over China” by Yan-Lin Zhang et al.

Anonymous Referee #1

Received and published: 15 February 2016

The authors present a summary of airborne filter measurement data with a focus on dicarboxylic acids in aerosols over China. The topic of organic acid composition in aerosols is of interest to the atmospheric chemistry community since the organic fraction of particles is complex and uncertain, and requires improvements to be able to model their impacts better. The paper is written fairly well but requires some minor English editing (I point out a few suggested changes below for improving the language). The tables and figures are appropriate. The title and abstract are also appropriate in terms of representing the contents of the manuscript.

The methods used are fine, and the results are informative and rich. The strength of the work includes the detection of so many organic acids that exceed the range of acids detected in some past studies relying on airborne measurements. The overall conclusions mostly repeat those in other studies though, which I hope the authors

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can address in their revisions. This study confirms that organic acids are generated by secondary production mechanisms and that aqueous processing can be important, especially aloft. To strengthen the paper, I would suggest that the authors try to find something new in their data to push the state of understanding of organic acids forward as compared to repeating what has already been documented.

The data generated are of high value as airborne diacid data are scarce, so it would be useful to have this work published. But before that happens, as noted above, it is requested that the authors narrow in better on what is novel in their dataset to highlight better in their text. This may be assisted by better consideration of what past work has shown and how this dataset can extend upon those past papers, some of which are highlighted below.

Major Comments:

The authors should expand discussion on the potential influence of biomass burning on their measurements. When was biomass burning evident and how did this impact the organic acid data? Presumably the authors have reasonable tracers for biomass burning plumes.

In various places in the manuscript the authors refer to Free Troposphere (e.g., Line 285). They should make it clear what data and criteria they used to distinguish between FT and the lower mixing layer.

Since denuders are not mentioned in the instrument description, the authors should comment on what impact the lack of using denuders have on the data.

The authors should make note of what affect temperature effects would have on their data as during aircraft sampling there likely are differences in temperature between ambient air and their filters.

Specific Comments: Page 1, Line 4: change 'organics' to 'organic'

Page 2, Line 38-41: Diacids have also been measured in other areas such as deserts

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and this should be noted for the sake of completeness (example provided here): Sorooshian, A., et al. (2012). Hygroscopic and chemical properties of aerosols collected near a copper smelter: Implications for public and environmental health, *Environ. Sci. Technol.*, 46, 9473-9480.

Page 2, Line 41: 'play an important role in...'

Page 3, Line 66-69: For completeness the authors should refer to the following other airborne studies: For 'Coastal marine stratocumulus and cumulus clouds over USA', add: Wonaschuetz, A., et al. (2012). Aerosol and gas re-distribution by shallow cumulus clouds: an investigation using airborne measurements, *J. Geophys. Res.*, 117, D17202, doi:10.1029/2012JD018089.

Prabhakar, G., et al. (2014). Sources of nitrate in stratocumulus cloud water: Airborne measurements during the 2011 E-PEACE and 2013 NiCE studies, *Atmos. Environ.*, 97, 166-173, doi:10.1016/j.atmosenv.2014.08.019.

Measurements have been conducted over inland agricultural and urban areas in the western United States: Sorooshian, A., et al. (2015). Surface and airborne measurements of organosulfur and methanesulfonate over the western United States and coastal areas, *J. Geophys. Res.*, 120, doi:10.1002/2015JD023822.

Line 98: 'for a series...'

Line 325-327: The discussion here can benefit from inclusion of past work showing how the relative amount of oxalate (versus total organic mass) increases with relative humidity based on airborne measurements [Sorooshian, A., et al. (2010). Constraining the contribution of organic acids and AMS m/z 44 to the organic aerosol budget: On the importance of meteorology, aerosol hygroscopicity, and region, *Geophys. Res. Lett.*, 37, L21807, doi:10.1029/2010GL044951] and how water-soluble organics have been shown to be enhanced relative to the surface when humid conditions aloft [Duong, H. T., et al. (2011). Water-soluble organic aerosol in the Los Angeles Basin and outflow

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regions: Airborne and ground measurements during the 2010 CalNex field campaign, J. Geophys. Res., 116, D00V04, doi:10.1029/2011JD016674.].

Supplement Table 1: For altitude, the authors should include a plus/minus standard deviation since it seems unlikely that the aircraft was level for that long of a period for each sample.

Line 356: What do the authors mean by the word 'control'? This seems like a very strong word that may not be warranted here. Are these diacids the majority of the OA mass and, if so, can this be shown in the manuscript in a revision?

Table 2: The authors should make mention of the sample number involved with these correlation calculations somewhere in the table or caption.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-5, 2016.

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