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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Molecular distributions of dicarboxylic acids, oxocarboxylic acids, and α-dicarbonyls in $PM_{2.5}$ collected at Mt. Tai, in North China in 2014" *by* Yanhong Zhu et al.

Anonymous Referee #1

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The authors present data from measurements at Mt Tai focusing on dicarboxylic acids and related compounds. Based on back trajectory and model analysis they conclude that aerosol arriving on Mt Tai has undergone long range transport and has a variety of sources, including anthropogenic emissions and biomass burning. This study is very similar to previous studies from some of the same authors (27% similarity rate), with some new features (backtrajectories, WRF model) which, however, have not been really made use of. Overall, it may be an interesting data set, in particular as it is discussed in the context of previous measurements of the same compounds at many different locations and also at Mt Tai in 2006. However, I think the discussion is quite confusing and needs major revision.

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Major comments

1) Day vs night time samples The authors find that day and night time samples show almost identical concentrations. However, I am not sure that distinguishing day- and night-time-samples is really meaningful here: If a sample was collected at nighttime (i.e. 6 pm - 6 am), it was likely processed during the day(s) before. The same might be true for day time samples that travelled to the sample location for several days. Thus, I am not surprised that samples collected during day and night show very similar composition and loadings. Unless I misunderstood the sampling and nomenclature of day/night samples, I suggest removing the discussion of day- versus night-samples. That way, hypotheses such as on night time oxidation (p. 2, l. 12) or less effective loss during night (p. 10, l. 24) could be removed as they do not seem supported.

2) Trajectories a) In Figure 1, the authors show 72 h-back trajectories of air masses arriving at Mt Tai. These trajectories are briefly discussed in Section 3.1. However, the authors do not link their later discussion to these trajectories. For example, there seems to be change in conditions (meteorology, emissions, air mass?) after the first half of the sampling period that leads to lower diacid loadings. Could that be linked to different trajectories? I suggest adding somehow the dates to the trajectories in Figure 1 or adding discussion in the discussion section.

b) The average RH on Mt Tai was low (17%). If indeed aqueous phase processing was a major contributor to the target compounds, RH would have needed to be high during the transport. Can the trajectories tell anything about clouds and/or high RH fields the air masses experienced during transport? Could any precipitation during transport explain an observed decrease in concentrations?

c) The discussion of the concentrations of diacids in the various clusters is not clear. How was the number of 73% of total dicarboxylic acids and related compounds in clusters 2 and 4 determined (p. 7, l. 8)?

3) Mass fraction of total and individual dicarboxylic acids etc a) The authors point out

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in the introduction that dicarboxylic acids are 'significant constituents in PM2.5' and '...impact on air quality' (p. 3, l. 4ff). However, later in the text the author quantify that these compounds contribute on average < 3% of OC (p. 7, l. 26), and related compounds even less (< 1%). What is the overall fraction of these compounds in the aerosol, i.e. not only related to organic mass but to total mass? The total (organic + inorganic) mass should be also reported in Table 1. Given these rather small fractions, the text should be reworded accordingly. b) The authors continue using 'significant' for contributions of > 2% within the diacid mass (p. 8, l. 5). I would not call such masses 'significant' given that these species contribute overall < 0.1% tot the total organic mass in the aerosol.

4) VOC measurements a) Only the total VOC mixing ratios are reported, assuming that all of them could be precursors for the target species. Figure 2 might be more meaningful if only a few selected VOCs are shown that have been shown to act as precursors for the identified compounds in Table 1. b) I think it is rather unusual that VOC measurements were performed at University of Irvine but neither in the acknowledgement nor in the author list anyone from this place is listed.

5) Comparison to previous studies How much of diacids and related compounds is expected to be in the size range of PM2.5 to PM10? I.e. is it reasonable to assume that the same scaling factor (0.91) for the total PM2.5/TSP mass can be applied to the diacids and levoglucosan? Are there any measurements (from other locations) that support this assumption?

Minor comments

p. 3, l. 28: 'secondary oxidation' seems redundant

p. 3, l. 15: Are these considered primary sources? In my understanding, combustion of fossil fuel or biomass is an oxidative process and thus the products are secondary.

p. 11, l. 3: What is the meaning of the slope of the correlation?

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Table 1: All values should be rounded to significant digits, e.g. 86 +/- 33 instead of 86.2 +/- 33.8

Figure 4: The x-axis is very blurry. I suggest using fewer tick marks.

Technical comments

p. 2, l. 6: ‘measure’ should be ‘measured’

p. 4, l. 22: blank samples

p. 4, l. 24, and remainder of the manuscript: VOC sampling (and VOC samples etc)

p. 6, l. 21: remove ‘the trajectories’

p. 6, l. 22: have been given

p. 13, l. 20: ‘related’ should be ‘correlated’

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