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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influences of emission sources and meteorology on aerosol chemistry in a polluted urban environment: results from DISCOVER-AQ California" *by* D. E. Young et al.

Anonymous Referee #1

Received and published: 6 February 2016

General/Main Comments:

This paper describes measurements of aerosol composition made in Fresno, California in wintertime during one of the NASA DISCOVER-AQ campaigns. Time series, diurnal cycles, inorganic/organic composition and sources, organic elemental ratios, size distributions, and source directions of submicron aerosol are presented. PMF was used to investigate source and processing for the organic aerosol. Effects of weekend/weekday, boundary/residual layer mixing and pollution events are discussed. Several meteorological and gas-phase measurements are used to interpret the aerosol measurements. Comparisons are made to a similar measurement dataset/publication

which showed some differences such as an additional BBOA factor (with fairly different mass spectra) and OOA factor and shifts in OA factor contributions, and different residual layer effects which is mainly attributed to meteorological differences coupled to possible changes in wood heating burning.

The manuscript is generally well written and describes a clear, straightforward, and thorough analysis of the measurements on a general level. However, like Referee 2, I also found that despite the clearly large effort that went into collecting and analyzing the measurements, there is little new in terms of methods or scientific insight nor are the measurements put in context compared to other such measurements at other locations. This seems to be particularly the case in light of the authors' similar publication of similar measurements and analysis at the same location during a different winter. Many observations are pointed out and then either one or a few possible explanations are briefly put forward usually with little in-depth analysis to investigate and better elucidate the underlying explanations. In short, it reads more like a report of measurements than a scientific study, especially given that new methods are not applied nor is the sample location/season new.

That said, like Referee 2, I would also err on the side of publication since the measurements and data analyses are thorough and high quality, and despite a lack of in-depth scientific analysis, there doesn't appear to be issues with over-stated conclusions that all-too-often find their way into the literature. Moreover, I suspect that documentation of these results will be useful in the context of ongoing DISCOVER-AQ analyses and future investigations and efforts to understand and mitigate the air quality issues of the SJV.

Detailed Comments:

P35059, L11: "This is an indication that nighttime chemistry might also be higher in 2013.": It is not clear what this means. Higher above ground? Faster? What types of chemistry? Please clarify.

P35060, L23-24: “representing an important PM component” is very vague. Be more specific.

P35061, L2-3:” In addition, the typical cold and wet weather in the winter promotes gas-to-particle partitioning of semi-volatile species.” Is this correct? Indeed cooler temperatures would promote higher partitioning of SVOC to the organic phase, but why would wetter temperatures promote gas-to-particle partitioning of SVOC? You mean of soluble compounds?

P35062, Sect 2.1: A reference to a white paper, overview or NASA website would be useful here. Also it would seem appropriate to introduce (mention) the aircraft measurements here.

P35063, L8: Add reference for SP2.

P35064, L18: change “low” to “lower”. Saying that a high-resolution ToF has low resolution seems contradictory. (also change high to higher).

P35064, L20-21: State frequency of calibrations during the campaign.

P35065, L12-14: People who aren’t intimately familiar with AMS workings won’t understand the point of the statement: “Nitrate was often observed to be an important component of PM1 during this study, yet the campaign average ($\pm$1s) CE was 0.5$\pm$0.04.” Consider rephrasing to make the point clear.

P35065, L16-18: “The AMS total mass-based size distribution compares well with the volume size distribution of the SMPS throughout the day (Fig. S2).” is vague. Correlation? Slope? Also wouldn’t the relevant comparison be mass vs mass using standard density algorithms to convert SMPS volume to mass?

P35065, L29: “for comparison to historical measurements”? Again, a non-AMS user will be lost here.

P35070, L26-29: Can the authors elaborate on why differences in the size distributions

is expected to be due to aqueous processing of inorganics. It seems like a broadening could be due to many different factors.

P35074, L10-12: Not clear if this is referring to the Ge et al. or this study (i.e. are the revised O/C what is noted for all of these studies which are being compared?

P35076, L26-27: In addition to the references above pointing to a large range of BBOA composition for ambient PMF factors, it would seem useful to reference studies where a range of O/C has been observed for biomass burning source studies, such as the FLAME studies, Carnegie Mellon or PSI groups, etc., since here the supposition is made that the differences may be due to differences in the primary BBOA rather than aged BBOA.

P35077, L18-19: “Consequently, adverse health effects associated with biomass burning emissions should be of even greater concern, especially during wintertime.” Even greater concern than what?

P35078, L20-22: “The O/C ratios of both OOA factors are significantly higher than those of the POA factors thus supporting the separation of the factors into primary and secondary OA.” This statement doesn’t seem like good justification. I.e. the O/C of the SV-OOA is the same as the O/C of BBOA which is a primary factor.

P35079, L10-13: Relevance of correlation if OOA and MSA ions needs more explanation.

P35079, L18: Do the authors mean “emissions patterns” (rather than “emissions”)

P35081, L26-28: O3 may also be larger on weekends due to the O3-formation chemistry being NOx-saturated in which has higher NOx => lower O3. For this reason it would be informative to show the diurnal cycle of Ox = NO2+O3 which helps separate these two effects (NOx-regime O3 chemistry and titration of O3 by NO emission) as has been done in multiple studies (and it seems the authors show for different purposes, 2010/2013 comparison, in Fig 11).

P35082, L12: Too many sig figs.

P35085, L12-14: “At around 17:00 PST the sun sets, the boundary layer starts to collapse, and any pollutants that mixed aloft during the day would be decoupled from the surface.” Please provide the reasoning for choosing that time. A rise in CO along would probably not be precise since the rise appears to be coincident with the evening rush hour. Perhaps the weekend CO profile supports this statement? Or something else the authors have in mind?

P35085, L16-17: “Ox is used here to indicate HNO3 production, although N2O5 chemistry can also influence the formation of HNO3”: Please elaborate. It is not clear how Ox would be a direct surrogate for HNO3.

FIGURES:

Fig. 1b: Can’t read features like “FWY-41” on the map. Perhaps it is just the rendering of the pdf, but best to be able to identify features that are pointed out in the text.

Fig 2e: It doesn’t explain in the legends or caption what the gray shading is on this plot?

Fig 2e: For SMPS, it would seem more appropriate to use a time-varying density as clearly there appears to be a substantial variability in density, in part seen by the relative change in inorganic/organic contributions in Fig 2.

Fig. S1: References for PMF factor densities?

Fig S6: It would be useful to write the species somewhere on this plot (e.g. on the blue areas at small sizes in white or yellow or next to the panel letter on top).

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 35057, 2015.