

## ***Interactive comment on “Size-resolved aerosol composition at an urban and a rural site in the Po Valley in summertime: implications for secondary aerosol formation” by S. Sandrini et al.***

**Anonymous Referee #2**

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This manuscript represents a thorough analysis of size-resolved aerosol composition measurements from two sites in the Po Valley during a summer campaign. In the introduction, the authors mention that the two site approach was intended to be used so that the rural site could serve as the ‘background’ for the urban measurements. In fact, the authors demonstrate that the higher relative humidity during the night at the rural site leads to substantially higher ammonium nitrate (and WSOC) at this rural site. In general, I thought the paper was clearly written, but it would be useful for the others to revisit this ‘Lenschow perspective’ in the conclusions and comment on whether a rural site can really be interpreted as the background for a nearby urban site. I also include some minor questions and suggestions:

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Page 5, Line 9 – ‘which did not justify the presence of ALW on particles’ should be reworded to ‘which prevented accurate calculation of ALW on particles’

Section 2.3 – Can the authors confirm whether inorganic carbonate salts have the ability to contribute to the WSOC reported by their measurement protocol? If not, in which category would the mineral dust carbonate be counted?

Section 2.4 – Which site was used as the origin of the back trajectories?

Page 11, Lines 25-30 – The moderately good correlation coefficient of 0.7 between WSOC and sulfate could be due to shared photochemical sources, but it may have other origins as well. Given that the values for each constituent range from 0.2 – 2, and there probably isn’t a factor of ten variability between photon flux between days, there’s likely at least one other factor driving the shared variability.

Section 3.5 – The numbering of the rotated components doesn’t make sense to the reader. Also, the last three components that are discussed are not identified by the numbering system used in Table 2.

Page 14, Lines 35-38 – Could the higher nighttime droplet mode sulfate measurements at SPC be the results of more cloud processing of SO<sub>2</sub> as a result of higher RH?

I suggest adding a scale bar to Figure 1.

Figure 7 – it doesn’t really make sense to have negative ALWC values. I would recommend using the boxes to show the interquartile range, or the 10th and 90 percentiles, rather than +/- the standard deviation.

Figure 8, panel a – it would be useful to have the y-axis of this figure on a log scale as well