

Interactive comment on “Evidence for ambient dark aqueous SOA formation in the Po Valley, Italy” by A. P. Sullivan et al.

Anonymous Referee #2

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General Comments

This excellent manuscript describes carefully-designed field measurements carried out with the goal of identifying evidence of aqueous SOA formation in Italy's Po Valley. The project was highly successful, acquiring the sought-after evidence and richly describing characteristics of aqueous SOA and its nighttime formation during a stagnant pollution episode. The findings that aqueous SOA production depended on local particulate nitrate formation and high ammonia concentrations will be of special interests to aerosol chemists. Overall, this work represents a substantial contribution to the field of atmospheric chemistry.

The manuscript clearly presents the field data and its context, and, with one exception described below, makes clear and compelling arguments interpreting the data. I

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recommend publication in ACP after the authors consider these comments.

Specific Comments

p. 35497 lines 3-19: This argument distinguishing between OOA-1 and OOA-2 needs to be clarified, and requires more data to be shown in support of the distinction. The authors state in line 12 that OOA-1 correlates with WSOC during increasing RH in period A, but not period B, as shown in Figure 9. This also appears to be the case for OOA-2. Furthermore, the authors state in line 5 that both OOA-1 and OOA-2 increase with RH and WSOC throughout period A, but do not show this data. They state in line 10 that OOA-1 drops in late morning when the RH declines (perhaps visible in Figure 8a?), and use this to argue for reversible aqueous formation of OOA-1. Then, the authors “illustrate” this reversibility by appealing to increasing RH data. They state that OOA-1, but not OOA-2, correlates with RH during times of increasing RH during period A, but fail to show this data, either. These observations are then used to make inferences about the different natures of OOA-1 and OOA-2, but too little data distinguishing the two (beyond the O/C ratio) has been shown to make the argument convincing.

It would be appropriate to reference the paper by Jian Yu et al. (2005), who identified the correlation between aerosol oxalate and sulfate, either in the last paragraph of the introduction or with the Sorooshian reference at the bottom of p. 35494.

This reviewer would like to see more connection or comparison made between the results of this study and the closely related work of El-Sayed (2015), briefly referenced in line 12 of p 35494.

References

Yu, J. Z.; Huang, X. H. H.; Xu, J.; Hu, M., When aerosol sulfate goes up, so does oxalate: Implications for the formation mechanisms of oxalate. Environ. Sci. Technol. 2005, 39, (1), 128-133.