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

Anonymous Referee #1

Received and published: 31 December 2015

Review of "Evidence for ambient dark aqueous SOA formation in the Po Valley, Italy" by Sullivan et al.

This study reports on aerosol measurements in the Po Valley of Italy during two distinct periods to show that there is aqueous formation of secondary organic aerosol (SOA) during the nighttime. The authors state that this formation is not due to clouds during the nighttime but rather in wet aerosols which is helped by enhanced aerosol liquid water (due to enhanced nitrate levels). The paper adds weight to the idea that oxalate should be used as a tracer for aqueous SOA formation in clouds and not wet aerosols, which has been discussed in previous works already. With revisions, this paper can provide a useful contribution to the growing body of knowledge about the formation mechanisms of organic aerosol, which is certainly a topic of interest to the journal. The

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methods used are sound. The paper is written and organized very well. In addition to comments below, the major weakness is that the conclusions are based on two short periods of data which was puzzling to me. With so many days of data it would seem important to at least discuss the nature of those measurements in the context of Figures 3-5. It seems that the criteria leading the authors to use the limited days that they did was rooted in air mass source characteristics, but this leaves the reader wondering if the periods were chosen based on desirable responses in the spaces shown in Figures 3-5. To add more confidence in the conclusions, the authors should address the rest of the data in some capacity. The current usage of such limited datapoints to reach the stated conclusions is an issue in this reviewer's opinion. A number of issues are raised below that are rooted in the few number of points used in the analysis that deserve serious attention prior to consideration of this work for publication.

Comments: Section 3.1: In the discussion of cloud cover, as indicated by satellites, the authors should provide exact details of the cloud data (parameter value, time of data, and spatial area of data obtained relative to the sample site) and discuss the satellite retrieval(s) used.

Section 3.1: In addition to the previous comment, were there reports of precipitation in Period B to the west or anywhere near the measurement site? In other words, was wet scavenging important in explaining any aspect of the data?

Section 3.1, Lines 11-25 on pg 35493: the discussion about correlation coefficients is useful, but I am not sure what rationale went into the three categories (>0.7, 0.4-0.7, <0.3). How do these relate to statistical significance on a standard students t-test table for the respective degrees of freedom used to generate the best fit lines? More discussion is required about the choice of these three categories.

Figure 2: for Period B there seems to be a gap in ALW data. why is that? also, why aren't data used (in the context of Figures 3-5) for the next couple days when large changes in ALW are observed?

Figure 2: Avoid having numbers overlap on the y-axis for the two panels.

Figure 3: add error bars in the RH axis too for each marker. Also, it would be useful to report the number of points used for each marker in the two panels. For example, is the 80% RH point in panel A based on very few points compared to the other markers? And why isn't there a point at 80% RH for the bottom panel for Period A. The decrease in RH should start at 80% if the increase in RH ended near 80%. It seems as though if the RH increased to 80% that one can be picky as to which panel that marker is placed in and obviously it looks much better in the top panel to make the case for the reported conclusions. This is an issue that needs to be discussed in a revision.

Figure 8C: what would the correlation be if the outlier point farthest to the left is omitted and are there any special characteristics associated with that datapoint? In the absence of that point it could be argued that a somewhat similar relationship exists as compared to panel D.

Figure 8A-B: it seems that the same general positive trend exists in both panels. The issue in panel B could be that there is a bit more noise and it has (what appears to be) fewer data points.

Figure 9: Panel B shows the presence of 2 outlier points to the middle-right that reduce its correlation. Having more datapoints would help in this case as it is unclear as to what explained those outlier points. The same applies to Panel B; note also that the right value on the x-axis of panel d is cut off.

Table 2/3: These tables seem somewhat distracting in my view and I am not sure how important they are to the discussion in the paper. The authors should strongly consider incorporating discussion of those tables to a larger extent if they think they should be kept.

-It would be useful to know exactly the times corresponding to when RH was increasing and decreasing for the plots shown in Figure 3/4/5. Some discussion about what other

factors varying during those two periods of time would be helpful to show that the authors have considered all possibilities affecting their organic aerosol data and why their stated conclusions are the most obvious reason as to why different results exist.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 35485, 2015.