We are very thankful to the reviewer for further revising the manuscript. Please find the replies below.

The authors did not actually answer the question of how deltaOA was calculated. They addressed a question regarding the influence of varying background CO levels on deltaOA/deltaCO, but still do not actually state how deltaOA was calculated. What is the background OA assumed to be? I can understand that the results may not be particularly sensitive to the choice of background OA, but the authors should state this nonetheless. The authors should also move some of the discussion regarding insensitivity to the selection of background CO to the main text.

> Indeed the manuscript does not details explanation on how deltaOA (or background OA) was obtained. Given that the lowest observed OA concentration was quite small (0.2 ug m-3), we considered the background OA to be zero. The text on L.474 now reads:

"For the calculation of ΔCO, we chose as background the lowest CO mixing ratio observed throughout the campaign, 240 ppb. Phase-specific backgrounds would be calculated 290ppb and 240 ppb for phase I and II, respectively, a difference less than 10% of average CO level, thus with little impact in ΔOA/ΔCO mass ratios. Conversely, the lowest concentration of OA was only 0.2 μg m^{-3}, thus we chose zero as OA background.

I still have concerns regarding the f43 --> H:C translation. The authors mention that "similarly processed BBOA (DeCarlo et al., 2010) has shown an excellent agreement between the H:C ratio estimated from f43 and the directly observed H:C ratio (Ng et al., 2011a)." If I look at the figure in Ng et al., the Decarlo 2010 points are the two black points. One of these actually falls outside the +/-10% bounds. But, more importantly in my estimation, is that the slope defined by these two points is much steeper than that given by the average fit in Ng et al. My point is that for any given location/study the H:C vs. f43 relationship is different, especially the slope (which can be steeper or shallower), and it is the slope here that will determine the Van Krevelen slope for this study. The Ng et al. curve captures the average behavior, but this may not be appropriate for an individual study and may give a misleading picture of the Van Krevelen slope. This is addressed in some ways (lines 443-460), but I think that the authors should go a little further by adding additional panels to Fig. S8 that show how exactly they arrive at the range of possible slopes reported (-0.5 to 0.5). This, I think,

would make clear the uncertainty.

Following the reviewer suggestion, Fig. S8 now depicts as well the V-K diagram calculated using the relationship proposed by (Ng et al., 2011) added with an uncertainty area which comprises from the 10th percentile of H:C ratio minus 10% up to the 90th percentile plus 10%.

Fig. 2: The caption should be updated to indicate that the ACSM collection efficiency was adjusted to give good agreement here. Otherwise the readers may think that such good (nearly perfect) agreement is a priori obtained. But, as the authors note in the text, an "intercomparison" was performed to determine the CE, which I assume means that they varied the CE until a slope of 1 was obtained. It should also be noted in the text that an average CE of 1 was used. It is possible that the CE is time (composition) dependent, which could explain some of the scatter.

The assumed CE of 1, i.e., the assumption that only a negligible amount of non-refractory aerosol in the instrument size range is not detected, was validated against integrated volume from SMPS and time-dependent aerosol density. No other corrections were performed to the ACSM data a posteriori. Fig. 2 caption has been rewritten as such:

"Comparison between the sum of the mass concentrations of species measured by the ACSM (assuming a CE of 1) and Aethalometer (at 880 nm) vs. aerosol volume obtained from SMPS and time-dependent aerosol density."

Line 404: m/z seems to be missing a number (43).

Text was changed accordingly.

Table 1: For Dpg, the p and g should be subscript.

Text was changed accordingly.

Line 442: The meaning of "reaction tendency" is unclear.

The sentence "reaction tendency" has been replaced by "dominating reaction".

Line 450: "relative" should be "regarding."

Text was changed accordingly.

Line 478: "viewer" should be "viewed".

Text was changed accordingly.

Line 586: The authors state: "This increase in LV-OOA during mid-day parallels the increase in aerosol hygroscopicity observed under similar circumstances by (Rissler et al., 2006), consistent with the hygroscopic character of LV-OOA (Jimenez et al., 2009)." This may be true, but what is not taken into account is whether the inorganics are also changing, as these can have a very large influence on hygroscopicity. This statement should be revised to include consideration of co-variation of the inorganics. But, as I noted previously, I don't think that this mention of hygroscopicity is even necessary/relevant here, as the hygroscopicity measurements being referred to are not part of this study. I do not think that this adds value to the current manuscript (and I actually find it distracting, given the lack of mention of inorganics).

The authors agree with the reviewer and thus decided to remove this sentence.

Figures

Figure S8

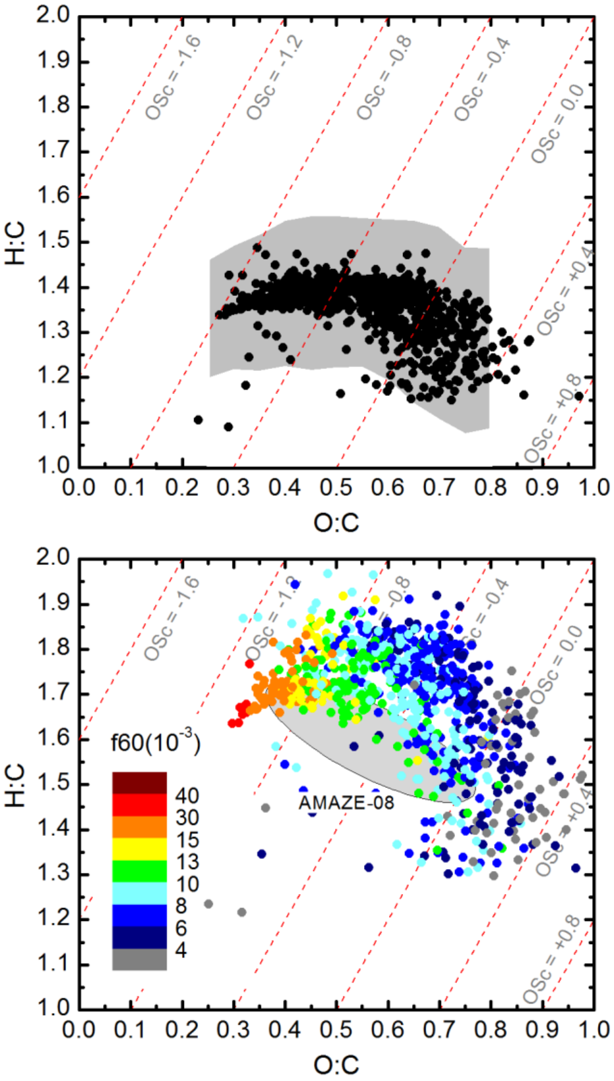

Fig. S8. Top: Results obtained here with uncertainty range (+10% above 90th percentile and -10% for 10th percentile) indicated in the gray area. Bottom: Similar to Fig. 07, taking into consideration the newly proposed corrections for HR-AMS O:C and H:C ratios according to Canagaratna et al., (2014). For consistency the AMAZE-08 V-K area based on Heald et al., (2010), shown here, has been corrected by similar factors.