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Interactive comment on "Dominant contribution of oxygenated organic aerosol to haze particles from real-time observation in Singapore during an Indonesian wildfire event in 2015" *by* Sri Hapsari Budisulistiorini et al.

Anonymous Referee #1

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Review of "Dominant contribution of oxygenated organic aerosol to haze particles from real-time observation in Singapore during an Indonesian wildfire event in 2015" by Budisulistiorini et al.

The authors presented measurement results of particulate matter (PM) in Singapore affected by an Indonesian wildfire event in 2015. A suite of on-line and off-line measurement techniques were used to quantify chemical composition of PM. The major data from on-line ToF-ACSM measurements showed that oxygenated organic aerosol (OOA) contributed ∼50% to total OA in non-refractory PM1 (NR-PM1), while biomass

burning and peat burning OA (BBOA and PBOA) contributed 30%, with the remaining 20% being hydrocarbon-like OA (HOA) or residual in PMF/ME-2 analysis. By combining on-line and off-line data, the authors concluded that both oxidation of gaseous precursors and aging of primary OA (POA) contributed to secondary OA (SOA) formation in the wildfire episode. For the POA aging, I incline to agree with the authors that it might occur, but I did not see direct evidence from the data (see Major comment #1 below). I would suggest the authors to either provide more evidence to support this statement or tune it down as a mere speculation as I see it.

The measurements and data analyses are rigorous and the manuscript is quite well written. The content is surely within the scope of ACP and is to the interest of readers of ACP. I therefore suggest minor revision with a few comments as below.

Major:

1. The assertion that SOA was contributed by POA (BBOA and PBOA) aging was only supported by an observation that the OOA factor has slightly higher f60 than normal background levels (page 14, line 3-5). While the OOA factor was surely influenced by BB or PB, whether the influence was merely factor mixing by PMF/ME-2 or truly due to aging processes is not clear. Set aside the fact that statistical analysis like PMF/ME-2 might suffer from factor mixing problems, correlation between OOA and BBOA/PBOA is anticipated because oxidation of VOC precursors from BB and PB can surely contribute to the observed OOA too. Did BBOA and PBOA decrease substantially (being converted) in either absolute or relative sense during the period that aging to SOA was likely occurring?

2. The off-line analyses: 1) the authors mentioned in the Methods section that detailed chemical speciation approaches such as LC-MS and GC-MS were used. But I do not see any results from them (except levoglucosan, which was used for correlation with BBOA/PBOA); 2) Quartz filters might not be suitable for analysis of inorganic ions because leakages may cause serious positive artefacts, especially for cations such as

sodium ion. Were anions such as sulfate and nitrate measured by off-line IC as well? Why are they not shown in Table 1?

3. Page 11, line 5-10. Levoglucosan had an even higher correlation with PBOA (R-squared = 0.7) than that with BBOA (R-squared = 0.6). Why levoglucosan cannot be used as a tracer for PBOA? If yes, then it seems that this compound is not that specific as a “tracer” to differentiate BBOA and PBOA. The authors may want to discuss a bit on that.

Minor:

1. Page 2, line 13: “wildfire haze between” to “wildfire haze to be between”.

2. Page 2, line 17: delete “, in the models”.

3. Page 2, line 23: “,” after “Aerodyne Inc.”

4. Page 2, line 25: “,” after “bulk OA”

5. Page 3, line 24: if CE is composition-dependent, it should not be a single factor of 0.5. Around 0.5?

6. Page 4, line 2: “0.2-0.3%”. Is this accurate? 20-30%?

7. Page 4, lines 14 and 17: the “a” does not show.

8. Page 4, line 21: “factors solution” to “factor solution”.

9. Page 8, line 21-22: I do not think a high standard deviation can indicate the uncertainties of the thermodynamic model. I assume that the standard deviation is from the deviation of all data (pH) from the average pH value, not from running the thermodynamic model in serval ways and averaging all the values. The high standard deviation only indicates the spreading of the pH values during the campaign. Whether the thermodynamic model is accurate or not cannot be reflected by this standard deviation, although I agree with the authors that without NH3 and water absorbed by organics

might cause some uncertainties by ISOROPPIA II.

10. Page 10, line 22-31: the authors did not observe significant correlation between HOA and CO/EC, and suggested that it might be due to limited number of data points or influence by BB or PB (for CO). What about the correlation with NOx?

11. Page 14, line 6-16: how well does ToF-ACSM resolve ion peaks for H:C and O:C calculation? That is, what is the mass spectral resolution of the ToF-ACSM? It might be useful to mention it in the Methods section. Were the H:C and O:C calculated by Aiken’s method or the Ambient-Improved method (Canagaratna et al., ACP, 2015)?

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