Review of "Aging of aerosols emitted from biomass burning in northern Australia" by Milic et al.

This manuscript studied the biomass burning organic aerosol in Australia by using a c-ToF-AMS. The heavy biomass burning causes that 90% of NR-PM_1 is organics. Five intense BB events with different burning conditions are analyzed. The enhancement of ozone and SOA in biomass burning plumes in examined. PMF analysis on background OA resolved three factors, BBOA, OOA, and IEPOX-SOA. The IEPOX-SOA is identified for the first time in Australia. The manuscript is well structured and the content is appropriate for ACP. However, most of the work presented here has been done before. The manuscript in its current form does not add significantly to the literature. Thus, I recommend major revisions. More in depth analysis and conclusions are required.

Major comments.

1. Whether this study is suitable/capable to study the aging of biomass burning OA.

Previous field studies on this topic usually used aircraft to follow the biomass burning plumes, which ensures investigating the same air mass (Cubison et al., 2011; Yokelson et al., 2009; Forrister et al., 2015; May et al., 2015). However, this is not the case in this study as the air mass is changing. In fact, many observations in this study can be explained by changes in air mass or physical changes, instead of aging of biomass burning OA. For example, an increase in *f*44 and a decrease in *f*60 (Figure 4b) could be simply due to an BBOA-rich air mass mixing with OOA-rich background air or physical changes during dilution (May et al., 2015). This could be potentially revealed in Figure 4a (close BB) if the authors color the data points by OA concentration. The data points with higher *f*60 and lower *f*44 would have higher OA concentration.

The authors may use MODIS sensor and backtrajectory analysis (section 2.1.7) to pinpoint fire position and calculate transport time. Then, the oxidation rate of BBOA or its tracers (such as *f*60) can be calculated, which is highly uncertain now. The aging time may also be estimated by using the photochemical clock, such as NO_x/NO_y ratio.

The OA in the five biomass burning events (section 3.2.4) is dominantly from fresh biomass burning, which provides a good opportunity to study the properties of fresh BBOA. The study would benefit from expanding related discussions. For example, "different burning

conditions" are vaguely mentioned multiple times to explain the difference between the five biomass burning events. However, can the authors be more explicit about the relationship between specific burning conditions/burning materials with OA concentration, *f*44, *f*60, ΔO_3, etc? It is helpful to bring Table S2 to the main text.

2. IEPOX-SOA related.

Firstly, it is not appropriate to label the PMF factor as IEPOX-OA. While the majority of this factor is from the reactive uptake of IEPOX, this factor also likely includes contribution from isoprene OA formed via other pathways. For example, a recent study by Schwantes et al. (2015) showed clearly that SOA formed via the reactive uptake of isoprene nitrooxy hydroxyepoxide could also produce fragment *m/z* 82. In addition, the mass concentration of this PMF factor cannot be fully explained by the total IEPOX SOA tracers (Hu et al., 2015; Budisulistiorini et al., 2015). Thus, it is more appropriate the name the factor as isoprene-derived OA (Xu et al., 2016; Pye et al., 2016).

Secondly, since the isoprene-OA factor is identified for the first time in Australia, more analysis could be done related to this factor. For example, does this factor correlate with sulfate, which has been shown in previous studies (Xu et al., 2015a; Budisulistiorini et al., 2015)? What's the NO concentration at the measurement site? Is IEPOX formed locally or from transport? What's the particle pH? As shown in Figure 9f, in general, IEPOX-OA correlates well with Org 82. However, some data are scattered (i.e., Org 82 0.004 to 0.008 μg m^{-3} range). What happens for these data? Is it due to sources other than IEPOX uptake contributing to Org 82?

Minor comments.

1. Page 2 Line 6. Elemental carbon (EC) is not the same as black carbon (BC). EC is defined by thermal properties and BC is defined by optical properties (Andreae and Gelencsér, 2006).
2. Page 4 Line 4. It is stated that sampled air mass had mainly passed over land. However, this seems to contradict with the conclusion in Page 8 Line 18-20, that a significant portion of chlorides could be sea salt.
3. Page 4 Line 19-20. It is not clear if the CE is determined by using the UMR data or the HR data. Or HR data are used in the squirrel panel? Also, sulfate fragments should not have

interference from organic fragments in HR data. Thus, HR data should be used throughout the analysis.

4. Page 7 Line 31. It is not clear how the “close BB” and “distant BB” periods are separated. The criteria for separation should be clearly stated?

5. Page 8 Line 14. The authors can use the NO^+/NO_2^+ ratio to infer if the nitrate functionality originates from inorganic or organic nitrate (Xu et al., 2015b; Farmer et al., 2010).

6. Page 10 Line 22-23. This sentence is really confusing and should be re-written.

7. Page 11 Line 22. Is the OA mass loading related to wind speed or wind direction?

8. Page 12 Line 14. The authors may check if the relationship between *f*60 and *f*73 changes with burning conditions or burning materials.

9. Page 12 Line 31. Is there any study to show that fresh emissions from biomass burning contain OOA?

10. Page 13 Line 10. Cite Xu et al. (2015a).

11. Page 14 Line 8-11. The low correlation between IEPOX-OA and BBOA could be simply due to that these two factors are from completely different sources and a correlation is not expected. The discussion in this sentence is not supported and should be removed.

Reference

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