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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Aging of aerosols emitted from biomass burning in northern Australia" *by* Andelija Milic et al.

Anonymous Referee #3

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This paper presents an analysis of atmospheric data from northern Australia, mainly AMS data impacted by biomass burning. A range of analyses is performed on the data, which gives some insight into the behaviour of the aerosol regarding the biomass burning plumes, ageing and also the formation of SOA from isoprene through the IEPOX route.

None of the results are particularly earth shattering or unexpected, given the pre-existing literature, and there is nothing really new on a process level compared to previous publications. However, there is currently a lack of in situ characterisation work like this in the tropics, so should probably be publishable on that basis. However, the paper is a little rambling and unfocused in paces, with discussions like isoprene SOA detracting from the supposed subject of the paper (ageing of biomass burning emissions), so the general theme of the paper should maybe be better defined.

I recommend publication for ACP, after the following comments have been considered:

Title: The title is possibly not appropriate, given that many other scientific phenomena other than the ageing of BB plumes (e.g. IEPOX-SOA) are discussed.

Page 8: 'Close BB' and 'distant BB' should be given more specific definitions. 'Prominent' is not very descriptive.

Page 10: Given the dynamic relationship between NOx and Ozone, it might be sensible to look at perturbations in 'potential ozone' (Ox = NO2 + O3) as well. This would remove the NO titration effect.

Page 10: The discussion of OA/CO vs time of day is problematic because the total amount of OA in a plume is the product of the total photochemical history of the airmass, not just the time of day that it was measured at. Without a more detailed treatment of the full airmass history, I do not see how any conclusions regarding things like fragmentation can be made.

Page 10: How is the change in SMPS size distributions consistent with SOA formation? Have you compared the particulate volume concentrations? Care should be exercised because an increase in mode diameter can occur simply through coagulation processes, which require no additional particulate mass formation.

Page 11: The assumption that f44 corresponds to photochemical activity is problematic. Biomass burning can produce a large amount of primary HULIS, which has a very high f44. Furthermore, while it has been shown that a plume's f44 will increase with time, it is not proven that photochemistry is necessarily responsible, particularly in the very early stages after emission where repartitioning or 'dark' chemical processes may occur. I would be more guarded and state that the high f44 implies a high level of oxygenation that could be caused by photochemistry.

Page 13: A lower boundary layer height can increase concentrations of primary emissions, but how would it increase IEPOX-SOA?

Page 14: There may be other explanations for a different f82, such as the formation of isoprene SOA through other routes (e.g. MPAN).

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