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Comment on acp-2022-477

Anonymous Referee #2

Referee comment on "Measurement report: Intensive biomass burning emissions and rapid nitrate formation drive severe haze formation in the Sichuan Basin, China – insights from aerosol mass spectrometry" by Zhier Bao et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-477-RC2, 2022

The authors reported measurement results of PM2.5 components at a site in Sichuan basin, China, using a time-of-flight aerosol chemical speciation monitor (ToF-ACSM). General results of the one-month campaign in winter 2021/2022 were presented with routine but rigorous data analysis tools. Three haze events, each accompanied with a foggy period, were selected for case studies to identify the reasons behind haze formation. The authors concluded that intensive biomass burning and rapid nitrate formation might be the reason behind the formation of those haze events. The study is in general well designed and properly conducted, and the manuscript is fairly well written. I therefore recommend Minor Revision before publication.

Main:

- The authors tried to make a point in the title that "intensive" biomass burning and "rapid" formation "drive" severe haze formation in their campaign. Yet, I do not see clear evidence supporting such a statement. First, for biomass burning, BBOA contributed 20-30% to OA, and maybe 10-15% of NR-PM2.5 during haze events (Figure 10a). Yes, it is non-negligible, but I would not say that it drives the haze formation. In addition, I do not see evidence for "intensive" biomass burning during haze events. Maybe showing some fire spot data from satellite archive will help. Second, for nitrate, the contribution of around 30% to NR-PM2.5 during haze events is

of course quite substantial. But I do not see any evidence of "rapid" formation of nitrate. Maybe showing some cases of fast growing of nitrate concentrations in some haze events would help.

- Sections 3.1 – 3.3 are quite routine and do not contribute much to the value of this study. I suggest shortening these three sections and focus on (expanding) discussion of the reasons behind haze formation (i.e., section 4).

- There are a few contradictory statements in the manuscript that I suggest the authors to resolve in the revision. For instance, it was suggested that aqueous-phase reaction was not important in OOA formation (L557), but in the discussion in L511 the authors suggested otherwise; the discussion on nitrate formation (L309-316) is interesting, but I do not follow 1) why the abundant ammonia can accommodate plenty of basic species (L310), and 2) how did the authors reach the conclusion that nitric acid was formed heterogeneously (which the authors thought that was not important in L290 and L303), and then take up ammonia?

Minor:

- L30: add "processes" after "aqueous-phase"?

- L61 and a few other places: citation format not in accordance with that of ACP.

- L387: aqueous-state should be aqueous-phase?

- Figure 12: better to clearly indicate the site, and Deyang and Sichuan in the maps. It is hard to follow when they are referred to in L475-485.