

Hu et al. reported the HR-ToF-AMS results at an urban downwind site in Sichuan basin in winter. The chemical composition and size distributions of submicron aerosols were characterized, and the sources of OA were investigated by PMF. The authors also studied the aging of OA using various approaches, e.g., Van Krevelen diagram, f44 vs. f43, oxidation states, and OA/CO, etc. This study is helpful to understand aerosol variations and oxidation states in southwestern China. However, the English writing is poor, and I often missed the logic when reading this manuscript. In addition, the data quality needs to be further validated and some data interpretations are not convincing. A major revision is needed.

Comments:

1. I raised the same comments in my first review of this manuscript. My major concern is the PMF results. Although the author expanded the PMF results, I still didn't see PMF diagnostic plots for other solutions except Table S2 with a simple description of the reasons. In addition, I don't understand why the authors didn't use the VOCs measurements to evaluate the PMF results.
2. The authors assumed that aerosol particles were neutralized and then got a RIE = 4.04 for ammonium. How are the authors sure that aerosol particles were neutral? Why didn't the authors use pure ammonium nitrate particles from IE calibration data to get the RIE of ammonium?
3. The authors claimed several times the unique of this study "unique geographical and meteorological conditions", particularly in the abstract. But I didn't see the details for this uniqueness (the authors didn't describe it either except high relative humidity).
4. The authors emphasized "influenced by biomass burning" in the title, however this manuscript appears to miss this point in both abstract and text.