Atmospheric Chemistry and Physics Discussions
Open Access
EGU

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2021-522-RC2, 2021


# Comment on acp-2021-522

Anonymous Referee #2

---

Referee comment on "Dramatic changes in Harbin aerosol during 2018–2020: the roles of open burning policy and secondary aerosol formation" by Yuan Cheng et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-522-RC2, 2021

---

This work investigates the aerosol chemical composition in Harbin in the past 2 years based primarily on in-situ measurement. Generally, I find that this manuscript may be more like a measurement report since that it does not provide sufficient new insight into atmospheric chemistry but the dataset seems unique and presents some new perspectives. One main problem with this work is that it entirely attributed the variations of secondary aerosol to the chemical processes, but aerosol does have a lifetime of 1~2 weeks and regional transport could largely contribute to the temporal variation at one specific site. Overall, I think this work fits the scope of ACP but there are some issues that need to be addressed to improve this work.

The method ought to be detailed in the main text. Section 2 is too sketchy, in which some basic information like the geographic location of the campaign site, its representativeness, the main emission sources in the surrounding areas, the instrumentation and the QAQC should be included. Also, other analysis like AWC, Positive Matrix Factorization (PMF) results discussed in Section 3, which is not from the direct observation, should be described in this section. This article itself missed a lot of necessary information on the method and analysis tools. I suggest moving the supplementary method to the main text. Another issue related to the method is that PMF analysis usually needs a large amount of data to support the factorizations since that it basically is statistical analysis. According to the Supporting Information, the sampling number is around 200~300, which is not enough to get a reliable result.
The discussion part is too descriptive and the majority of the main text is just describing the variation of different aerosol components. In some places, the authors jumped to some conclusions without careful investigation. For instance, the RH-dependent increase of OC was attributed to heterogeneous reactions without investigating the cloud-water chemistry and diffusion/dispersion analysis (Line 236-238). Additionally, the author concluded that the inter-annual (I would not call it inter-annual since it is just a two-year comparison) variation of OC was related to RH levels without other vital information of the regional transport pattern and other meteorological parameters. Besides, the higher threshold RH for the sharp increase of SOR in Harbin is not certainly indicative of the fact that the heterogeneous formation of sulfate was less efficient there. Many previous works have pointed out that the polluted air mass from the southern area of Beijing also brings about humidity, rather than heterogeneous chemical reactions at a local/city scale. Thus,

some in-depth analysis and rigorous arguments need to be added to this work.
There are also some inconsistent arguments that needed to be checked. For instance, in the abstract, the authors claimed that "we found that open burning activities were actually not eliminated ……", but Line 168 stated that "indicating that agricultural fires were almost completely eliminated during the measurement period". It doesn't seem very clear.