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Comment on acp-2022-786

Anonymous Referee #1

Referee comment on "In-depth study of the formation processes of single atmospheric particles in the south-eastern margin of the Tibetan Plateau" by Li Li et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-786-RC1, 2023

This paper studied the chemical compositions and mixing states of single particles in a rural site in the southeastern margin of the Tibetan Plateau (TP). The major particle types and size distributions of single particles were discussed, and the results of backward trajectories were coupled to investigate the regional impact on the formation of single particles in the sampling site. Two episodes were selected to discuss the transport and secondary formation processes of single particles. In addition, the linear regressions between several marker ions and RH, Ox were explored to elucidate the formation processes of these secondary species. Generally, in view of the lack of field observation data in Tibetan Plateau, this study provides a good opportunity to investigate the mixing states and formation processes of single particles, which is of great significance to evaluate the influence of fine particles on the climate change in TP. However, several issues need to be addressed and some revisions are necessary before the acceptance of this manuscript.

- The size distributions of single particles from SPAMS should be scaled by other instruments such as SMPS, otherwise, the unscaled size distributions of single particles should be treated carefully, which mainly referred to the relative changes of same type particles at different period. The comparison of different type particles and quantitative description of size patterns are usually inaccurate. Authors presented many results of size distributions, but the length of discussions should be reduced and some expressions should be revised.
- Line 32-33: why the volatilization of nitrate would lead to more abundant of sulfate?
- Line 34-35: not all of these secondary species showed strong linear regressions with RH and Ox from the discussion of Section 3.3. Authors should give a more precise conclusion.
- Many field studies via SPAMS have been reported in recent years, thus, authors should add some new references especially those published after 2018.

- There are many grammatical errors and unprofessional descriptions in the manuscript, such as: line 47-48, “making their impact on the air more uncertain”; line74-75, “Atmospheric aerosols also can influence the properties and life span of clouds as cloud condensation nuclei”; line 81, “Most studies have focused on the influence of optical properties”; line 92, “with a high temporal resolution”; line 95, “AMS/ACSM mainly used to provide”; line 102, remove “full” from “determine the full chemical composition”; line 107, “The shortage of information”; line 115, “pre-monsoon, to continuously (i) investigate”; line 123, “2.1 Observation site”; line 129, “The villagers make a living by farming (e.g., potato and autumn rape), and biomass is the main residential fuel”; line 143, “a detection moment”; line 194, “Differently, few”. In general, the related grammatical errors are not limited to these examples, authors should carefully revise the manuscript to meet the quality of ACP.
- Line 49-59: these sentences in the introduction were repetitive and should be reduced in length.
- Line 94-95: The AMS and SPAMS both have its advantages to conduct the researches in aerosols, so you can directly present their application in the aerosol study instead of pointing out the things they can’t do.
- Line 165-168: the “Aging Element Carbon (EC-aged)” should be “aged elemental carbon (EC-aged)”. “Potassium-containing (NaK-SN),” doesn’t match its abbreviation, and what is the difference of this type with “Potassium-rich (rich-K)”?
- Line 177: For the trajectory clusters analysis, I don’t think the height of 500 m is reasonable to elucidate the transportation of air masses in consideration of the mountains and plateau surrounding the sampling site.
- Line 190: May be the name of secondary type particles is more reasonable than the name of rich-K in Table 1.
- Line 199: Authors made a mistake here. [$^{58}C_2H_5NHCH_2^+$] is not the marker of DEA, actually, the marker ion of DEA is [$^{74}H_2NC_4H_{10}^+$]. Thus, the following discussions associated with DEA were incorrect in lines 200-208, 270-273.
- Line 215-217: how do you make sure the contribution of BB transport under the influence of PBL change?
- Line 223: “and road dust from upwind areas”, this is a contradictory expression.
- Line 230: “The most dominant air masses are Cluster 1, 3 and 4 from northeastern Myanmar”, actually, you only have four trajectories, so you cannot say three trajectories were dominant.
- Line 257: Again, the discussions of size distributions associated with quantitative results were unreasonable.
- Line 295: High emission of sulfate from coal combustion, biomass burning, and vehicles?
- Section 3.3: The discussions of Figure 5 and 6 did not show much difference, and the related results were quite similar as Figure 3 and 4. Most part of this section provides little insights into the mixing states of single particles. Authors should add new data analysis and discussions.
- Line 356-358: “This might be influenced by the pollutant dispersion with the increased PBL height when Ox was evaluated (Fig. S9)”, I don’t think this is a reasonable explanation.
- Line 372: The formation of ammonium oxalate was not a correct explanation here.
- Authors didn’t compare the characteristics of single particles with those reported studies in rural and urban areas. This is encouraged to demonstrate the unique mixing states of single particles in TP.
- Overall, authors should emphasize the influence of regional transport on the mixing states of single particles in TP, and give more discussions on the aqueous phase and photochemical formation of secondary species in TP.