

This manuscript provides a valuable year-long dataset from the Tibetan Plateau, including chemical composition and optical properties of aerosols. While the data coverage is extensive and multi-methodological (e.g., UV-Vis, PARAFAC, PMF), the manuscript often reads more like a report than a scientific discussion. Greater effort should be made to interpret the results in light of previous literature and physical processes. Below are detailed comments tied to specific line numbers for clarity.

-More introduction on the background studies in the region is needed. importance of one year long and integrated measurement studies are needed. Also need to discussion about the previous studies has been done in this area

### **Methodology]**

- The method section heading should better reflect the full scope of the measurements (not just OC/EC or UV-Vis). Also need to be organized.
- Clarify how many samples were collected, and whether these are representative.
- Specify how OM, EC, and WSI were measured (e.g., AMS for OM? IC for WSI?) also need to explain how these data were used and what was the purpose for.
- OC does not necessarily represent the full water-soluble organic carbon — consider using WSOC instead.

### **Line 282–286]**

- The claim that  $\text{Ca}^{2+}$  increases in spring is not strongly supported in the figure. Please refer to figures or provide clear quantitative evidence. — this undermines the linkage to dust transport.
- Sulfate peaks in summer, not spring — this undermines the linkage to anthropogenic influence.
- Wind direction discussion is unclear; winds in spring appear similar to other seasons — clarify or expand.

**Lines 307–309:** Oxalic acid  $R^2 = 0.4$  is considered weak, not moderate. Revise the language and provide a better justification.

**Line 330:** The conclusion that low MAE365 in summer is due to photobleaching is speculative without any other supporting. Could it also be due to fewer chromophores? Further evidence is needed.

**Lines 333–336:** The MAE365 in spring is higher than at QOMS despite less biomass burning influence. Explain why this may be — secondary formation? Source region differences?

**Lines 344–349:** Comparing summer AAE with annual AAE from other studies is inconsistent unless justified. Align comparisons by season or explain the rationale.

**Lines 352–355:** The classification scheme from Saleh (2020) is introduced, but not clearly interpreted for your dataset. What does your MAE–AAE position tell us about the BrC types at this site?

**Line 362:** “Moderately absorptive BrC” implies some source or process — is this consistent with PMF or OA oxidation states?

**Lines 363–370, PARAFAC section:** The section lacks interpretation. What do the fluorescence numbers mean in terms of source or oxidation state? How does this support your conclusion?

**Line 399:** Only two OA factors (MO-OOA, LO-OOA) were resolved. Was there no biomass burning OA? Clarify if this was due to method limitation or absence of source.

**Line 417–420:** The slope analysis comparing O:C and H:C is unclear. What does a slope of  $-0.62$  imply versus  $-0.89$ ? Clarify the physical meaning.

**Lines 429–434:** Comparisons of MAC365 between urban and remote sites need qualification. The difference in formation pathways, oxidation levels, and sources must be considered.

**Lines 439–447:** The logic behind changes in MAE365 with O/C and H/C across seasons is not clearly supported. Summer O/C is not particularly high — so why the assumed aging effect?

**Lines 450–460:** The integration of PARAFAC and PMF components is potentially insightful but poorly explained. What are the identities and atmospheric implications of C1–C4? Why does C4 not correlate with OOA? Needs more in-depth discussion.