General comment:

In this measurement report, the authors collected dust deposition from six urban and rural sites throughout the southern Qaidam Basin, China, between January 2020 to March 2023. The primary objectives of this work were to identify the primary sources of dust emissions in the region and investigate the influence of domestic heating over dust deposition. This investigation offers interesting findings; however, there are several major issues that undermine its conclusions.

First, the written structure of the manuscript is somewhat unwieldy and difficult to follow. For example, the introduction is disorganized and does not effectively contextualize the study for the reader. In another instance, the authors discuss the results of particle size analysis without first introducing (or even mentioning) the corresponding methodology earlier in the MS. **Second**, multiple key details critical to scientific rigor of the study are excluded from the MS (or not clearly communicated to the reader). One of the most concerning examples in this case is that the authors do not describe the methods used to test statistical differences between sample groups. **Third**, the analytical methods used in this investigation are not sufficiently described and possibly introduce unconstrained analytical error, particularly in the case of organic carbon measurements.

Given the abundance and severity of issues present, I unfortunately recommend that the editor reject the manuscript. After significant revisions and satisfactory QA/QC analysis, this measurement report could be re-submitted to ACP to undergo the peer review processes a second time.

Specific comments:

Line 62: The introduction would benefit from a general restructuring as its current form would be somewhat confusing for the reader. For example, the brief discussion regarding PM source characterization (e.g., PMF) is misleading when located in the opening paragraph – as it is not evident at that point why these methods matter. I would suggest starting the introduction more broadly by focusing on atmospheric dust deposition and its human / environmental impacts (e.g., glacier melt) – and then lead into sources / source characterization methods, study area, and study objectives.

Line 63: The authors need to more effectively define atmospheric dust and clarify how it relates to PM.

Lines 76-78: Further elaboration is needed here. Why is the QXP an important climate regulator? What are the features/mechanisms responsible for this influence? I assume it relates to the abundance of freshwater sources and glaciers in the region – but this should be made clear for the reader.

Lines 80-81: Please describe what black carbon is.

Lines 80-82: Make sure it is clear to the reader that BC enhances glacier surface temperatures through atmospheric deposition.

Line 85: I think this should be "QXP".

Lines 89 – 90: "It has a high population density and intense human activity, yet it is highly sensitive to climate change" - Could the authors provide some data or statistics to contextualize these statements? Additionally, I do not think the use of "yet" in this sentence is needed – I would also suggest merging this sentence with the following one.

Line 91: Did the authors mean to say "…exacerbating the impacts of atmospheric pollution."?

Lines 91-94: I am confused by this sentence. The authors state that the QDB relies on coal, yak, dung, and firewood. Three of the fuels listed are biomass, as such, this statement contradicts the previous phrase "Unlike South Asia….where biomass fuels dominate…". Some clarification is needed here.

Lines 95-98: The structure of this section is somewhat confusing. I would suggest reworking the discussion here so that there is a more direct emphasis on carbon emissions.

Lines 102-106: It should be made clear to the reader why the authors are interested in arid climates (i.e., this could be established in the introduction while discussing dust sources).

Line 119: The study objectives should be clearly outlined at the beginning of this final paragraph.

Line 122: The HYSPLIT model and PMF models need a brief description in the introduction.

Lines 122-125: This passage is difficult to follow. Please re-phrase.

Lines 119-128: As it is written, the introduction suggests that this study will be largely interested in carbon deposition; however, there is relatively limited discussion here describing carbonaceous PM (e.g., BC and brown carbon) and its impacts. The authors should expand on these concepts in the introduction.

Lines 133-135: Provide a data source or reference.

Line 138: The authors have referred to the QDB as a "treasure bowl" (line 88) and "treasure basin" (line 138); one of these is the correct name I assume.

Line 144-145: Are there any studies / reports that could support this claim?

Lines 146-147: Were these stations part of a network? Could the authors provide a link or reference for this resource?

Lines 147-148: This would be a good place to reference Fig. 1.

Lines 149-150: Can the authors provide additional information on the operating principles of the MDCO methodology?

Lines 155-157: Without immediate context, this discussion around collection efficiency is more confusing than helpful. If wind speed impacts efficiency, what were the wind speeds during the study (at each site)? How might variable particle-size collection efficiencies impact your observations? How did you account for collection efficiency if it varies as a function of wind and particle size? How confident are you that your sample is representative of real-world deposition?

Line 157: Fine particles are <2.5 μm in diameter.

Lines 163-164: Where the samplers covered manually or automatically?

Lines 167-168: Were any replicate samples collected during the study?

Line 160: Are you measuring fine or total dust deposition? Make sure this is clearly stated.

Lines 215-220: More detail regarding sample treatment is needed here. For instance, what concentration of HCl and HF were used? What was the extraction temperature and length? etc.

Lines 215 - 224: I have some serious concerns regarding this methodology. The authors state that they treated the dust samples with HCl and HF and analysed the filtered solids for EC and OC; they further state that this method has a high EC recovery, but are unable to provide similar assurance for OC. Organic carbon within atmospheric PM often contains polar, basic, and mineral-bound compounds – these can be soluble in HCl / HF solution and would be lost during the filtration process. Without accounting for this loss, I suspect that the authors are underestimating OC by an unknown factor. Erroneous values would impact the accuracy of subsequent metrics derived from OC, including OC/EC ratios, POC, and SOC.

Line 247: It is best not to start a sentence with an acronym/abbreviation.

Lines 247-271: Previously, I identified my concerns regarding the accuracy of the OC measured from the dust samples due to the acid treatment method. As such, I have reservations regarding the accuracy of the SOC values, as these are estimated using the OC concentrations.

Line 257: What does $(OC/EC)_{pri}$ theoretically represent? This detail will help the reader understand the purpose of the MRS method.

Line 324: Are the values reported here (and throughout) the average of total deposition flux during the study period, ± 1 standard deviation? Please clarify.

Lines 324-336: In this section, the authors note that deposition flux was elevated during the heating period and suggest that this difference was due to increased tourism in the region. Perhaps this is true, but I don't think the authors can make this assertion before considering the influence of other factors like seasonal meteorology (variable wind speeds could easily explain differences in dust deposition; see Yang et al., 2024, DOI: 10.1016/j.rcar.2024.12.007). Now that I am thinking about it, there is very limited analysis and discussion regarding regional meteorology during the study period. I would strongly encourage the authors to include this in the manuscript.

Lines 326-331: This discussion compares deposition flux between different periods and stations; however, there is no mention of statistical tests here. How can we be confident that these observations are significantly different, especially when mean values are relatively close (e.g., 7.1% on line 331).

Line 344-345: If you state that sample groups are significantly different, you must describe how this was determined (i.e., what tests were used). I have noticed that discussion regarding statistical analysis is generally vague throughout the entire manuscript (see Figures 3 & 6, lines 383-384). This must be addressed.

Lines 375-382: This is where my concerns regarding the accuracy of the OC measurement data come up again. Without any knowledge regarding the collection efficiency of the OC method how can we be confident that the values reported here are actually low, and not instead a result of OC loss due to the HCl/HF treatment and filtration?

Figure 4: Elements of this figure are difficult to interpret. The colour of certain bar and whisker plots are too light and/or similar to the background colour (e.g., SOC).

Lines 396-397: How large of a difference in OC and EC was observed between these periods? Were these significant differences? Based on my visual inspection of Fig. 4., it looks like OC and EC at the rural sites are somewhat comparable between HP and NHP periods.

Line 797-801: This discussion regarding particle size distribution was somewhat confusing, as there is no mention of this analysis in the primary methods section.

Line 843: There is no need to introduce the abbreviation, "SO_3", if it is not mentioned again in the manuscript.

Line 933: Please check all references for formatting consistency.