Response to Comments of Reviewer #1:

We highly appreciate the reviewer for valuable comments. The reviewer comments are laid out below in black text, and specific concerns have been numbered. Our responses are given in blue text, and changes/additions to the manuscript are given in red text.

- **Black: the reviewer's comments.**
- **Blue: the authors' responses.**
- **Red: Quotes from the revised manuscript.**

Detailed comments

1. L11: Replace "..recognized as substantial INPs" with the following recognized as a substantial source of INPs, interactions...

Response: Thanks for this comment. The sentence has been revised accordingly.

L10-11: "While natural dust is recognized as a substantial source of INPs, ..."

2. L35: Dominant is a more appropriate description as originally stated. Overwhelming evidence for both flight based research and field measurements supports that below temperatures of -15° C, mineral dust is the primary INP (Kanji et al., 2017). For cirrus there is also a compelling argument mineral dust is the dominant source of INP for heterogeneously formed cirrus particularly in the Northern Hemisphere (Froyd et al., 2022).

Response: We agree that "dominant" is a more appropriate description in this context, and the sentence has been revised accordingly.

L33-34: "Mineral dust is considered the dominant natural source of INPs in mixed-phase clouds, ..."

3. L142-143: ...concentration distribution. Combined particle size results for the entire sampling period exhibited no significant biases and acceptable levels of precision (see Fig. S2).

Response: Thanks for this comment. The sentence has been revised accordingly.

L115-116: "Combined particle size results for the entire sampling period showed good continuity between the SMPS and APS measurements after merging (Fig. S2)."

4. L155: ...including fine mineral dust (FMD) and...

Response: Thanks for this comment. The capitalization has been corrected accordingly.

L128: "...including fine mineral dust (FMD) and..."

5. L186: Assuming this is the Handix Scientific instrument, include the model/manufacturer here.

Response: Thanks for this comment. The instrument used in this study is a commercially available CSU Continuous Flow Diffusion Chamber–Ice Activation Spectrometer (CFDC-IAS), originally developed by Colorado State University (CSU), while Handix Scientific Inc. serves as the distributor. As stated in Lines 156–157: "INP measurements in this study were performed using a commercially available CSU (Colorado State University) Continuous Flow Diffusion Chamber–Ice Activation Spectrometer (CFDC-IAS) (Rogers et al., 2001; DeMott et al., 2015) …"

6. L196: I prefer the format where the size threshold for ice classification was specified. This helps in comparing methods/results between studies. Clarifying the potential of droplet breakthrough would also be a welcome addition as is common for CFDCs running at the high supersaturations conditions used here.

Response: Thanks for this suggestion. We have now clarified that particles larger than 4 µm detected by the OPC were classified as ice crystals. This has been added in Lines 159–160: "… leaving only ice crystals larger than 4 µm for optical particle counter (OPC) detection (DeMott et al., 2025)." Although relatively high supersaturation conditions were used in this study, the water supersaturation (typically 4–6% SSw) remained below conditions where obvious droplet breakthrough was observed. As described in Line 162: "…, under which no evidence of droplet breakthrough was observed."

7. L263: ...the median INP concentrations decreased as the activation temperature increased from −35 °C to −15 °C...

Response: Thanks for this comment. The temperature order in the sentence has been corrected accordingly.

L215: "from −35 °C to −15 °C..."

8. L300: ...event types exhibit distinct...

Response: Thanks. The typographical error has been corrected.

L244-245: "...event types exhibit distinct..."

9. L327: ...Notably, a significant divergence...

Response: Revised as suggested.

L271： "...Notably, a significant divergence..."

10. Figure 3 Increasing the spacing between the two subplots on top and the middle panel will help with clarity. Currently the x-axis labels of (a) and (b) are within the top legend of the middle figure.

Response: Thanks for this comment. The subplot spacing and legend position in Fig. 3 have been adjusted to improve clarity and avoid overlap.

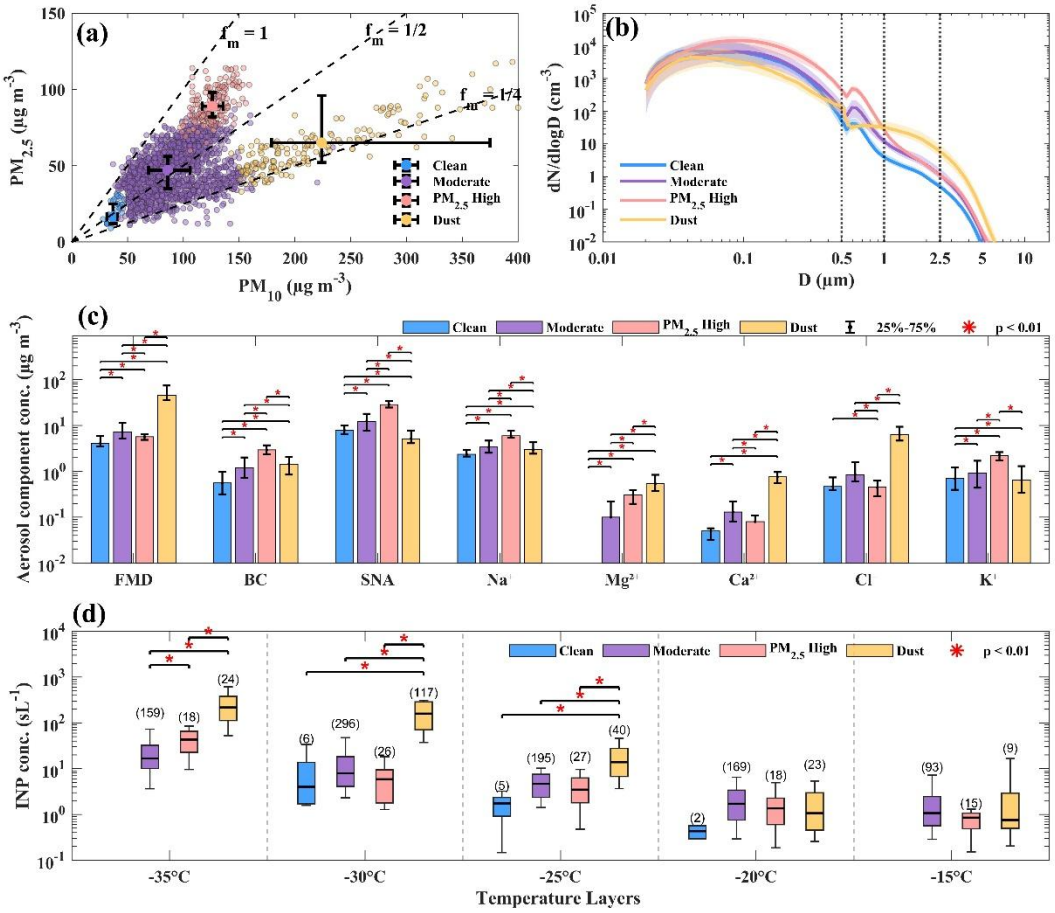

Fig. 3. Comparative analysis of physicochemical characteristics and INP concentrations across different aerosol event types.

Response to Comments of Reviewer #2:

We highly appreciate the reviewer for valuable comments. The reviewer comments are laid out below in black text, and specific concerns have been numbered. Our responses are given in blue text, and changes/additions to the manuscript are given in red text.

- **Black: the reviewer's comments.**
- **Blue: the authors' responses.**
- **Red: Quotes from the revised manuscript.**

Major comments

1. Fig. 8 & L412-414 & L485-486: Tobo et al. (2014) might be a better comparison for a mid-latitude dust, which can be enriched in organics... Why did the authors preclude fertile soil dust as a source of INPs?

Response: Thanks for this insightful comment. Our original discussion primarily focused on comparisons with commonly used mineral dust parameterizations. We agree that fertile soil dust may also represent a relevant source of atmospheric INPs in mid-latitude regions. To provide a more comprehensive discussion, we have now included comparisons with Tobo et al. (2014) in the revised manuscript (Fig. S8b) and the revised manuscript. Lines 411–413 now state: "During dust events, the derived Ns values are systematically lower than mineral-dust-based parameterizations (Niemand et al., 2012) and fertile soil dust parameterizations (Tobo et al., 2014), …"

In addition, we have added discussion in the revised manuscript (Line 416-418): "Potential influences from mineral composition, soil organic matter, and particle mixing state may also contribute to the observed variability in *Ns* values (Kanji et al.,2017)."

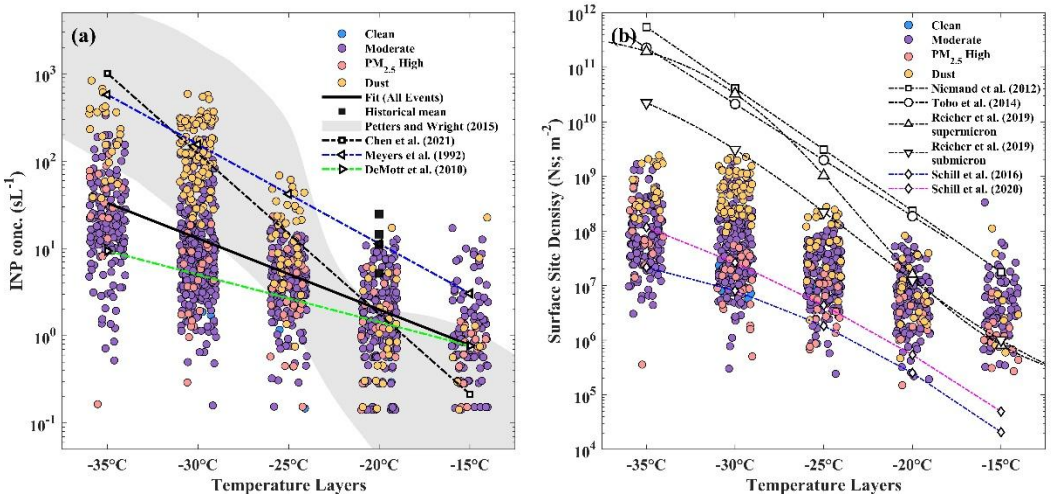

Fig. 8. Construction of single-parameter INPs parameterizations and comparison with existing parameterization schemes.

2. Fig. 10: How does Tobo et al. (2014) parameterization-based prediction compare to the D10 one?

Response: Thanks for this insightful comment. The original Tobo et al. (2014)-based prediction substantially overestimates the observed INP concentrations (Fig. R1), which is consistent with the higher N_s values already shown in Fig. 8. When an empirical scaling factor of 0.01 is applied, the agreement improves considerably, with FAC5 increasing from 4.84% to 42.15% and RMSE decreasing from 6.17 to 2.78. However, the performance still remains lower than that of the D10-based parameterization (FAC5 = 50% and RMSE = 2.04). This result suggests that N_s associated with fertile SOM-rich soil dust is substantially higher than that of the urban aerosols observed in Lanzhou.

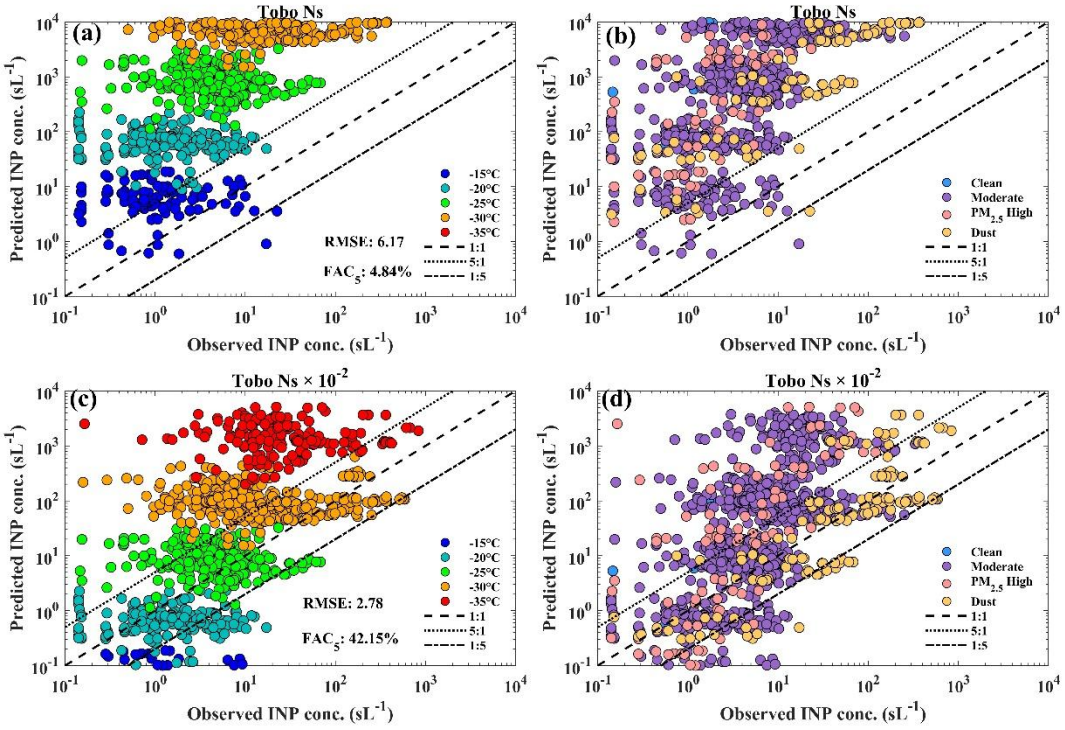

Fig. R1. Comparison between observed INP concentrations and predictions based on the Tobo et al. (2014) parameterization under different atmospheric conditions in Lanzhou.

3. L460: urban INP bursts --> regional INP bursts in our study area and seasons. If the authors say "urban", it will be misleading, in this reviewer's opinion. That's not how the INP burst is defined in L241-242. Since the authors state "elevated INPs are linked to long-range transport from arid source regions" in L465, it is more appropriate to be said for regional INPs specific to the authors' study area and time.

Response: Thanks for this insightful comment. To clarify this point while retaining the urban observational context, the sentence has been revised accordingly.

Line 464-465: "Springtime dust storms elevate INP concentrations at −30 °C by a factor of ~15, making episodic dust plumes the primary driver of regional INP bursts in our study area and seasons"

Detailed comments

1. L9-10: --> ⋯their spatiotemporal variability and sources in⋯

Response: Thanks for this helpful suggestion. The sentence has been revised accordingly.

Line 9-10: "…their spatiotemporal variability and sources in…"

2. L11: --> ⋯its interaction with anthropogenic pollutants, as well as INP abundance of the mixtures, remain⋯

Response: Thanks. The sentence has been revised accordingly.

Line 12-13: "…its interaction with anthropogenic pollutants, as well as INP abundance of the mixtures, remain…"

3. L15: --> We further refine an INP parameterization and demonstrate the INP concentration estimation based on⋯

Response: We agree that the estimation capability of the parameterization is important. However, to maintain the conciseness and overall flow of the sentence, we chose to retain the original wording, as the parameterization itself inherently represents the estimation of ambient INP concentrations.

4. Intro: Second and third paragraphs can be combined by emphasizing more urban and dust INPs.

Response: Thanks for this insightful suggestion. The second and third paragraphs in the Introduction have been combined.

5. L23-24: This sentence is misleading. The SNOWIE study did not measure INPs, and it is not appropriate to state "enhancing INP concentrations". Maybe ice crustal number, but careful wording is necessary.

Response: Thanks. The sentence has been revised using more careful wording related to cloud seeding.

Line 23-24: "Owing to this microphysical sensitivity, cloud-seeding experiments have shown that artificially seeding supercooled clouds can shift the timing and spatial distribution of precipitation (French et al., 2018)."

6. L26: What do the authors mean by “by regulating ice crystal number concentrations”? Please elaborate.

Response: Thanks for this comment. We agree that the original wording was not sufficiently clear. The sentence has been revised to more explicitly describe the influence of INPs on ice crystal formation and number concentrations.

Line 27-28: “Therefore, by influencing ice crystal formation and number concentrations, INPs subsequently affect cloud microstructure, ⋯”

7. L32: --> including but not limited to⋯ The authors should be aware that the INP variations can be influenced by many other things, such as dynamic and synoptic scale factors etc., and this should not be oversimplified.

Response: Thanks for this comment. The sentence has been revised accordingly.

Line 33-34: “These variations are influenced by multiple factors, including but not limited to particle source, chemical composition, and surface morphology (Kanji et al., 2017).”

8. L34: effective --> active INP

9. L34: The authors are not explaining why it's active...

Response to 8-9: Thanks for the comment. Revised to clarify that the cited studies mainly emphasized the efficient transport of East Asian dust to altitudes relevant for mixed-phase cloud formation, rather than intrinsically higher ice-nucleating activity.

Line 34: “Mineral dust is considered the dominant natural source of INPs in mixed-phase clouds, with East Asian dust being efficiently transported to altitudes relevant for mixed-phase cloud formation (Chatziparaschos et al., 2023; Kawai et al., 2021).”

10. L35: contribute --> act as ambient INP

Response: Thanks for this comment. Revised accordingly.

Line 35: “… act as ambient INP …”

11. L35 and elsewhere applicable: warm --> high; cold --> low. Temperature is physically scaled for low-high, not cold-warm.

Response: Thanks for this comment. Revised throughout the manuscript where applicable.

12. L36: biologically active --> biogenic particle dominant

Response: Thanks for this comment. Revised accordingly.

Line 36: "··· biogenic particle dominant…"

13. L46: This reviewer respectfully disagrees - Cziczo et al. (2009) found that lead-containing particles are among the most efficient INPs of tested metals. Lead was observed in mid-latitude mixed-phase clouds. Later on, in a laboratory study, Yakobi-Hancock et al. (2013) found that commercially available lead iodide (PbI2) was a highly active INP in comparison to 23 other samples, including clays, metal oxides, and dust particles.

14. L46-48: This reviewer does not understand the logic of "however, metal···".

15. L50-53: This reviewer does not understand the logic of this part.

Response to 13-15: Thanks for these comments. We agree that the original wording and logic were unclear. The text has been revised to clarify that the ice-nucleating ability of metal-containing particles depends strongly on chemical composition. While some specific lead-containing particles (e.g., PbI_2) have been reported to be highly ice active (Cziczo et al., 2009; Yakobi-Hancock et al., 2013), field observations suggest that metal-rich particles may contribute only negligibly to ambient atmospheric INP populations (Corbin et al., 2012). In this study, metal elements are discussed primarily as tracers of fine mineral dust rather than direct evidence of ice-active metal particles.

Line 44-48: "Although some specific lead-containing particles (e.g., PbI_2) have been reported to be highly ice active, field observations suggest that metal-rich particles may contribute only negligibly to ambient atmospheric INP populations (Cziczo et al., 2009; Yakobi-Hancock et al., 2013; Corbin et al., 2012). Metal elements are widely used as tracers of fine mineral dust (Liu et al., 2022) and thus provide useful information on mineral-related contributions to atmospheric INPs."

16. L78: long-term --> multi seasonal

Response: Thanks for this comment. Revised accordingly.

Line 79: "… multi seasonal …"

17. L81-83: This sentence does not fit here. The authors may omit it.

Response: Thanks for this comment. Revised accordingly.

18. Fig. 1: Add wind roses so that the readers can guide their eyes to understand the upwind/downwind coordination in general.

Response: Thanks for this suggestion. We prepared wind roses during the revision process to help illustrate the general upwind/downwind relationship. To keep Fig. 1 concise and focused on the sampling

locations and regional setting, the wind roses have been included in the Supplementary Material as Fig. S1.

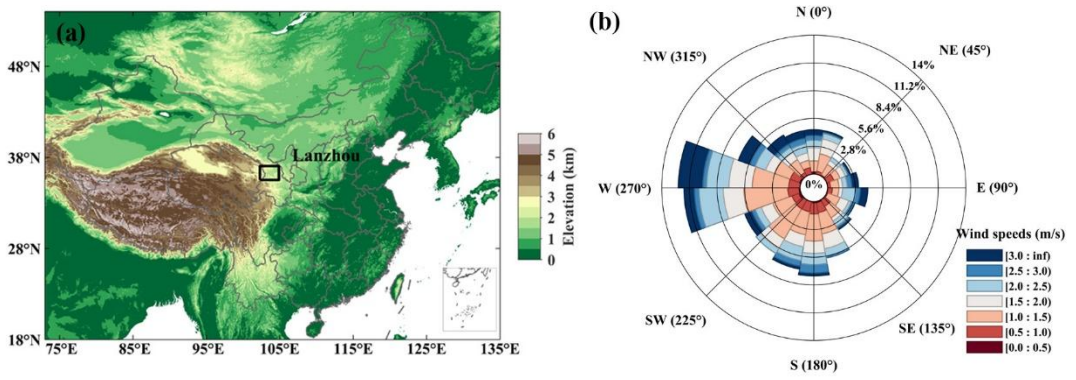

Fig. S1. Geographical location of Lanzhou City and wind characteristics during the observation period.

19. L110-112: What is the dynamic shape factor used for the conversion? All assumptions need to be listed.

Response: Thanks for this comment. Additional assumptions used in the APS aerodynamic-to-physical diameter conversion have now been clarified in the manuscript.

Line 109-111: "The *D*a distribution obtained by the APS was converted to *D* distribution by assuming an effective particle density of 1.5 g cm^{-3} and spherical particles with a dynamic shape factor of 1 (Zhang et al., 2022), so that the size distributions from different instruments could be merged."

20. L116: The attached diagram (Fig. S2) shows --> Figure S2 shows

21. L117: What does "no significant biases and acceptable levels of precision" mean quantitatively?

Response to 20-21: Thanks for this comment. The sentence has been revised for clarity.

Line 116-117: "Combined particle size results for the entire sampling period showed good continuity between the SMPS and APS measurements after merging (Fig. S2)."

22. L129: F --> f

Response: Thanks for this comment. Revised accordingly.

23. L131: "because it is a characteristic temperature at which immersion freezing is most frequently examined." This reviewer is not sure if this is a good rationale and justification. The authors need to show proper citations.

Response: Thanks for this comment. The rationale for selecting −30 °C has been clarified and supported with appropriate references in the revised manuscript.

Line 331-341: "…, we focused on −30 °C for detailed analysis because this temperature lies within a commonly investigated immersion-freezing regime in previous studies (Burrows et al., 2022; Moore et al.,2024) and showed clear contrasts among different aerosol conditions (Fig. 6)."

24. Fig. S4: What are occasional INP peaks for AS representing? I see that the authors corrected the background INPs.

Response: Thanks for this comment. The occasional INP peaks observed for AS were mainly associated with short-term fluctuations in aerosol generation and particle loading during the performance tests, rather than instability of the CFDC thermodynamic conditions or OPC detection. Overall, the tests still demonstrated consistent immersion-freezing activation behavior and reliable chamber operation under the selected experimental conditions.

25. L217: from --> in the range from

Response: Thanks for this comment. Revised accordingly.

Line 217: "… in the range from …"

26. L219-220: This reviewer does not see a profound increase in INP at -25 dC. Rephrase it to statistically significant – this reviewer understands that the authors ran the Mann-Whitney U test. A careful wording is needed.

Response: Thanks for this comment. The description at −25 °C has been removed to better reflect the observed results.

Line 217: "In contrast, INPs at −30 °C and −35 °C showed little variation from December to February, …"

27. L233: reflects --> implies

Response: Thanks for this comment. Revised accordingly.

28. L238: This reviewer disagrees that the met parameters were stable. There are occasional precipitation events, and the authors should discuss their effect on INPs. This was suggested in the first round review, and this should not be part of an outlook (L308-311).

Response: Response: Thanks for this comment. We have added further discussion regarding the potential influence of meteorological conditions and precipitation events on INP variability in the main text, as described in Line 238: "Meteorological conditions did not exhibit large variations during most of the observation period (Fig. S9), with precipitation occurring during only 3.12% of the sampling period, suggesting a limited influence of wet scavenging on the observed INP variability."

29. L339-340: It’s hard for this reviewer to find a positive association for SNA-Dust. It looks just scattered, and any fit positive slope might be just a coincidence for limited data. The same trend holds for SNA-PM2.5 High. The authors need to carefully phrase what they observe vs. what they wish to see.

Response: Thanks for this comment. The description has been revised to adopt a more cautious interpretation of the relationship between SNA and INPs during dust and PM2.5-high events.

Line 277-280: “Across all events, INP concentrations decreased with increasing SNA (R = −0.71), whereas this relationship became much less clear during dust events, with substantial scatter in the observations (Fig. 6d).”

30. L341: a reference is missing for “the ratio of soluble Ca2+ to elemental Ca, an indicator of dust dissolution”

Response: Thanks for this comment. The appropriate reference has been added.

Line 341: “To further investigate this divergence, we examined the ratio of soluble Ca2+ to elemental Ca, an indicator of dust dissolution (Chen et al., 2024a).”

31. Fig. 8b: m^-3 --> m^-2

Response: Thanks for this comment. The unit has been corrected from m^{-3} to m^{-2} in Fig. 8b.

32. L403: --> threshold. In contrast, …

Response: Thanks for this comment. Revised accordingly.

33. L461-462: “revealing a clear” – this reviewer does not understand this part.

Response: Thanks for this comment. The sentence has been revised to provide a more cautious interpretation of the temperature-dependent influences of dust and pollution aerosols on INP activity.

Line 465-466: “In contrast, $PM_{2.5}$-rich pollution episodes only contribute significant ice-nucleating ability below −35 °C, highlighting the distinct temperature-dependent influences of dust and pollution aerosols on atmospheric INPs, with potential implications for identifying dominant INP sources in cloud and weather forecasting studies.”

34. L467: The first sentence seems not to be needed.

Response: Thanks for this comment. The sentence has been removed accordingly.

35. L474-476: L467: The first sentence seems not to be needed.

Response: Thanks for this comment. The sentence has been removed accordingly.

36. L483: How about mixed-phase clouds? A careful wording seems necessary here. If the authors claim the aerosol-ice-cloud interactions, readers may take ice-cloud as cirrus, which is not studied in this work.

Response: Thanks for this comment. The wording has been revised to more clearly specify the relevance of the results to mixed-phase cloud processes and avoid potential ambiguity in the term "ice-cloud interactions".

Line: 485-487: "This study could provide region-specific constraints on aerosol interactions with mixed-phase clouds in arid North-West China and offer observational insights that can inform parameterizations for other dryland cities where urban air interacts with transported dust."

Reference

Tobo, Y., P. J. DeMott, T. C. J. Hill, A. J. Prenni, N. G. Swoboda-Colberg, G. D. Franc, and S. M. Kreidenweis (2014), Organic matter matters for ice nuclei of agricultural soil origin, Atmos. Chem. Phys. Discuss., 14, 9705–9728, doi:10.5194/acpd-14-9705-2014.