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.**

Overall Evaluation

Chen et al. report an observational study on Ice-Nucleating Particles (INPs) conducted in Lanzhou, a semi-arid inland city in Northwest China, from winter 2024 to spring 2025. The research addresses a critical gap in understanding INP variability in urban dryland regions, where natural dust and anthropogenic pollution interact. The core finding is that episodic, long-range transport of mineral dust is the primary driver of INP bursts, while persistent urban pollution plays a much smaller and potentially suppressive role. The study topic matches the journal scope. The manuscript is a bit too lengthy for the given content, and the overall quality and clarity of figures and writing should be improved for its consideration for publication in ACP. If the data scarcity and providing the long-term observational data are the main motivations of this study (L65, L69, L366-369), this manuscript might be more suitable to be a measurement report rather than a research article.

Response to the Overall Evaluation: We sincerely thank the reviewer for the careful and balanced overall assessment of our manuscript. We highly appreciate the recognition that the study addresses an important gap in understanding INP variability in urban dryland regions and that the topic fits within the scope of ACP.

We agree on the concerns regarding manuscript length, figure clarity, and overall presentation quality. In response, we have revised the manuscript in a targeted and substantive manner, focusing on areas identified in the detailed comments.

Regarding clarity and redundancy, we carefully revised paragraphs identified by the reviewer as unclear, overly long, or speculative. For example, the paragraph previously noted as speculative (former L287–295) has been substantially rewritten to provide a more evidence-based and literature-supported discussion. Long or complex sentences in both the Results and Discussion sections have been restructured into clearer, logically separated statements. Ambiguous terminology (e.g., "aerosol conditions") has been replaced with more precise wording (e.g., "aerosol event types" or "aerosol regimes") to improve conceptual clarity.

With respect to figure presentation, several figures have been revised to reduce visual complexity and improve readability. In particular, Fig. 8 has been simplified by retaining only the parameterization schemes directly discussed in the main text. A new panel has been added to facilitate clearer comparison of temperature-dependent surface site density parameterizations with previous studies. Supplementary figures have also been redesigned where necessary (e.g., revised presentation of PM_{10} and Ca^{2+} time series) to improve transparency and visual clarity. For Fig. 10, we have added explicit discussion

clarifying that the apparent straight-line features at low INP concentrations reflect the instrumental detection limit of the CFDC and the structure of the parameterization at relatively warm temperatures. The origin and implications of these features are now clearly explained in the manuscript.

In addition, we have improved methodological transparency by explicitly stating the effective detection limit under the operating conditions of this study, expanding the description of time synchronization among different datasets, adding missing references, and clarifying instrumental details where requested. Finally, the Conclusions section has been revised to remove introductory-style statements and to more concisely summarize both INP concentrations and ice-nucleating efficiency (surface site density), thereby strengthening the process-oriented interpretation of the results. Collectively, these revisions improve clarity, reduce unnecessary complexity, enhance methodological transparency, and sharpen the scientific focus of the manuscript.

Finally, we respectfully clarify that this study extends beyond a measurement report. While observational scarcity motivates our work, the core contribution lies in developing a regime-based aerosol classification framework (Clean, Moderate, $PM_{2.5}$ High, Dust) and systematically evaluating temperature-dependent INP behavior across contrasting environments. Our analysis reveals that dust transport enhances ice-nucleating activity, whereas pollution-dominated conditions show weak or negative associations between secondary inorganic aerosols and INP abundance. Backward trajectory clustering and land-cover statistics further link INP bursts to arid source regions. By quantifying intrinsic ice-nucleating efficiency through normalized activation fractions and surface site density, we separate particle abundance effects from nucleation efficiency differences. We refine a regionally adapted INP parameterization incorporating coarse-mode particle concentration and freezing temperature, providing process-consistent, modeling-relevant insights into aerosol – ice-cloud interactions in dryland urban environments. We have revised the manuscript to emphasize these analytical contributions and respectfully submit it as a Research Article.

Detailed comments

1. Title: Add “during Winter and Spring” at the end to clarify that the result is representative for specific seasons.

Response: Thanks for this comment. The title now includes “during Winter and Spring” to clarify the seasonal scope.

Title: “Anthropogenic Modulation of Dust-Dominated Ice Nucleation in an Urban Dryland City of China during Winter and Spring”

2. L9 …cloud and precipitation modulation, yet…

Response: Revised as suggested. The “cloud formation and precipitation” has been replaced with “cloud and precipitation modulation”.

L9: “Ice-nucleating particles (INPs) are crucial for cloud and precipitation modulation”

3. L10 Consider changing from influencing factors to sources or properties. “Influencing factors ” sounds awkward.

Response: Thanks. “Influencing factors” has been revised as suggested in the revised manuscript.

L10: “…yet their properties, variability and sources in urban dryland regions remain poorly understood.”

4. L10-11 the dominant INP source --> substantial INPs

Response: Revised as suggested. “the dominant INP source” has been replaced with “substantial INPs” in the revised manuscript.

L10: “While natural dust is recognized as substantial INPs, …”

5. L11 the extent to…INP abundance remains --> interactions between dust and anthropogenic pollutants, and how they alter INP abundance remain

Response: Thanks. The sentence has been updated accordingly.

L11: “…, interactions between dust and anthropogenic pollutants, and how they alter INP abundance remain …”

6. L15-16 Avoid using the word “two-parameter scheme” in the abstract. Generalize to something like INP parameterization based on aerosol size and freezing temperature or anything similar.

Response: Revised as suggested. The term “two-parameter scheme” has been replaced with a generalized description of the INP parameterization in the abstract.

L15-16: “…, an INP parameterization based on aerosol size (1–2.5 μm) and freezing temperature, which …”

7. L19 Ice“-”nucleating

Response: The hyphen has been added to “ice-nucleating”.

L20: “Ice-nucleating particles (INPs) …”

8. L19 …ice crystal formation on water-insoluble aerosol surface by…

Response: Thanks. The phrase “on water-insoluble aerosol surface” has been added.

L20: “… ice crystal formation on water-insoluble aerosol surfaces by …”

9. L21 10^-3 to 10_-5 numbers seem misleading. INP concentration depends on freezing temperatures, and could be an order of 10^-6 or lower. The authors may consider deleting the parentheses.

Response: Revised as suggested. The parentheses have been deleted.

L22： “Despite their extremely low atmospheric concentrations, …”

10. L23-24 What INP abundance ranges and properties are crucial for precipitation formation then? The authors may consider explaining a bit more in detail for readers.

Response: Thank you for this helpful comment. We agree that the original wording was overly general. While the first paragraph is intended to provide a broad conceptual introduction rather than a detailed discussion of abundance ranges and particle properties (which are addressed later), we have revised Lines 23–25 to strengthen the context. Specifically, we added a field-based cloud-seeding example (French et al., 2018) to more explicitly demonstrate how changes in INP concentrations can influence precipitation development.

L23-25: "Despite their extremely low atmospheric concentrations, INPs are critical for initiating ice-phase processes that govern cloud phase partitioning (Wang et al., 2015) and precipitation efficiency (Murray et al., 2012; Hawker et al., 2021). Owing to this microphysical sensitivity, cloud-seeding experiments have shown that artificially enhancing INP concentrations in supercooled clouds can shift the timing and spatial distribution of precipitation (French et al., 2018)"

11. L31 dominant --> abundant

Response: Thanks. The "dominant" has been replaced with "abundant".

L34: "Mineral dust is considered the abundant natural source of INPs …"

12. L44-45 This reviewer does not understand this sentence. Break it down to two sentences and add sufficient explanation for each reference.

Response: Thank you for the comment. We agree that the original sentence was unclear and combined two different findings without sufficient explanation. We have revised Lines 51–53 by separating the statement into two sentences and clarifying the specific findings of each cited study.

L51-53: "Moreover, anthropogenic pollution may influence cloud glaciation processes. Elevated cloud freezing temperatures have been reported under polluted conditions (Pan et al., 2024). INP number concentrations have been observed to differ between clean and polluted environments (Ren et al., 2023)."

13. L53 particle diameter --> particle larger than 0.5 micron diameter

Response: Revised as suggested. "particle diameter" has been replaced with "particle larger than 0.5 μm in diameter".

L61: "…particles larger than 0.5 μm in diameter …"

14. L54-56 How about empirical parameterizations, such as the Phillips parameterization (https://doi.org/10.1175/2007JAS2546.1)? The current discussion of INP parameterizations seems superficial and irrelevant to the study topic (interaction of anthropogenic and dust). More in-depth discussion incorporating previous INP parameterizations would be meaningful for readers.

Response: We appreciate this insightful comment. The discussion of INP parameterizations has been revised and expanded to incorporate empirical schemes such as Phillips et al. (2008) and to more clearly address the limitations of purely size-based approaches, particularly in environments influenced by interactions between mineral dust and anthropogenic aerosols. The revised text now provides a more in-depth and study-relevant discussion.

L60-65: "As an empirical, size-based approach, the D10 scheme primarily relates INP number to particles larger than 0.5 μm in diameter, offering a practical proxy for INP-relevant aerosols. However, it does not explicitly account for mixing-state effects or aerosol compositions as emphasized in Phillips et al. (2008). This simplification introduces additional uncertainty in polluted environments, where mineral dust-pollution interactions and abundant pollution-derived aerosols may alter INP activity and complicate size-based representations."

15. L106-108 This reviewer respectfully disagrees that time-averaging the data enhances the accuracy of the size distribution. It just offers a time-averaged representation, which smooths out the size distribution spectra as some pulsive data points get merged.

Response: Thank you for the suggestion. The statement has been modified to clarify that 10-minute averaging was applied for temporal alignment and reduction of short-term variability, rather than to enhance accuracy.

L115-116: "All particle size distribution data were averaged to 10-minute intervals to enable temporal alignment and reduce short-term statistical fluctuations before merging."

16. L113-115 Please add an appropriate reference here. The citation is missing.

Response: We thank the reviewer for pointing this out. An appropriate reference has now been added in the revised manuscript.

L156-159: "INP measurements in this study were performed using a commercial Continuous Flow Diffusion Chamber–Ice Activation Spectrometer (CFDC-IAS), which is based on the well-established CSU-CFDC design (Rogers et al., 2001; DeMott et al., 2015) and is capable of autonomous operation

with minimal in-person handling and full remote access (Bi et al., 2019). Hereafter, this instrument is referred to as CFDC."

17. Sect. 2 The authors need to describe the time resolution of each aerosol measurement data and how they synchronized those data (time averaging, right?) for the correlation analyses presented later in the manuscript.

Response: The time resolution and synchronization of all aerosol measurements with INP observations are now explicitly described in Sect. 2.

Specifically, particle number size distributions were also averaged to a 10 min time resolution (Sect. 2.2, lines 115). Black carbon (BC) mass concentration was measured at a native time resolution of 1 min and averaged to 10 min before analysis to suppress short-term fluctuations (Sect. 2.3, lines 125). $PM_{2.5}$ mass concentration data were available at hourly resolution, and aerosol chemical composition (water-soluble inorganic ions and elemental species) was measured at an hourly time resolution (Sect. 2.3, lines 129-130).

All aerosol measurements were synchronized with INP observations using a unified nearest-neighbor time-matching approach. For each INP measurement, the closest-in-time aerosol data point from each dataset was selected, provided that the time difference did not exceed 30 min (Sect. 2.4, lines 188–192). This time window was chosen to ensure representative matching with hourly-resolved datasets and to avoid using aerosol data that are temporally too distant from the corresponding INP measurements. No temporal interpolation was applied. These synchronized datasets were subsequently used for the correlation analyses presented later in the manuscript.

18. L117 …in the upper growth region of simulated cloud particles.

Response: Thanks. The text in L162 has been revised accordingly: "… in the upper growth region of simulated cloud particles."

19. L118-119 The authors should offer a reference justifying 3 micron threshold size for ice crystals.

Response: Literature-reported thresholds within CSU-CFDC applications generally range from approximately 2 to 4 μm, depending on instrument configuration and detection criteria (Rogers et al., 2001; Kanji et al., 2011; Levin et al., 2014; Lacher et al., 2024; DeMott et al., 2025). The CFDC-IAS used here is a commercialized implementation of the CSU-CFDC and follows the same fundamental operational and detection principles. It employs its default internal OPC channel-based discrimination

scheme, and the 100-size channels do not correspond to a single explicitly defined physical diameter threshold. To avoid introducing a potentially misleading fixed-size value in this study, the explicit particle size threshold has been removed from the revised manuscript, consistent with previous CFDC-IAS applications (e.g., Bi et al., 2019).

L163-164: "In the lower evaporation region, liquid droplets rapidly evaporate, leaving only ice crystals for optical particle counter (OPC) detection."

20. L123 This reviewer disagrees. There are many papers reporting the gap/offset between online INP measurement techniques (e.g., CFDC) and offline ones at a certain freezing temperature range. Often, INP concentrations measured by online techniques reads higher than offline ones. This discrepancy can stem from different detection limits of detectable INP concentration for various techniques. What's the detection limit of CFDC that the authors utilized for this study? From Fig. 10, this reviewer guesses it's ~0.15 INP sL^-1? Please clarify this in the manuscript. A proper discussion of CFDC's INP detection limit in the manuscript might be beneficial for readers.

Response: Thank you for this insightful comment. We agree that systematic differences between online INP measurement techniques (e.g., CFDC) and offline immersion freezing methods have been widely reported (Demott et al.,2017,2018; Lacher et al., 2024). Accordingly, we have revised the text to remove the explicit reference to offline techniques and now refer only to immersion freezing conditions. In addition, we have clarified the effective detection limit of the CFDC used in this study in the manuscript.

L166-167: "Water supersaturation conditions (typically 4–6% SS_w), consistent with those reported by Moore et al. (2024) for immersion freezing, were maintained in the growth section."

L183-185: "The effective detection limit of the CFDC is not a fixed instrumental constant but depends, for example, on the sampling flow rate, integration time, and background ice counts (Demott et al., 2017). Under the operating conditions applied here, the effective lower detection limit is estimated to be ~0.13 INP sL^{-1}."

21. L129 What is the temperature ramping interval from -15 dC to -35 dC? Were the authors be able to observe homogeneous freezing at -35 dC?

Response: Thank you for the suggestion. The CFDC temperature was adjusted by the refrigeration system from −35 °C to −15 °C. INP concentrations were not measured during the cooling period but only

after the target temperature was reached and stabilized. Therefore, the cooling process does not affect the reported INP concentrations.

Regarding homogeneous freezing at −35 °C, the CFDC measurements in this study were conducted in alternating background and ambient sampling modes. Background measurements were performed by removing aerosol particles using a filter. If homogeneous freezing had occurred under the operating conditions at −35 °C, substantial ice counts would also have been observed during the background periods. However, no such increase was detected. This indicates that the ice signals observed at −35 °C required the presence of aerosol particles and were therefore dominated by heterogeneous nucleation rather than homogeneous freezing.

22. L141-143 Reference missing – if it's a commonly applied approach, some refs should be offered.

Response: Thank you for the suggestion. We agree that an appropriate reference should be provided here. Relevant references describing this commonly applied approach have now been added in the revised manuscript at L141–143, which better support the methodology adopted in this study.

L211： "This combined trajectory–land-cover analysis is commonly applied in field-based INP studies as a complement to in situ aerosol chemical measurements (Chen et al., 2024a)."

23. Sect. 3 (L217-228) Discussion of the impact of precipitation and gusty wind on INP suppression/abundance is missing. Airmass trajectory is important to trace the airmass source and path, but local-synoptic scale meteorological conditions could also be key. Dust in mid-latitude is tied to convective cloud and precipitation formation (https://doi.org/10.1016/j.atmosenv.2008.09.069). Often, the same convective system and associated surface winds can induce dust resuspension from surface sediments.

Response: We thank the reviewer for this insightful suggestion. We have added a discussion in Sect. 3 addressing the potential influence of near-surface winds and synoptic-scale meteorological processes on INP. The revised text now acknowledges that dust events in mid-latitude regions are often associated with convective systems, gusty winds, and precipitation, which can affect dust transport, removal, and resuspension.

L308-314： "Furthermore, near-surface wind fields and synoptic-scale meteorological processes may also modulate INP observations. Existing research indicates that dust events in mid-latitude regions often co-occur with convective systems, gusty winds, and precipitation processes, which can simultaneously

influence dust transport, removal, and resuspension of surface sediments (Rivera et al., 2008). Although this study did not quantitatively analyze the relevant weather processes, under the Dust scenario, the long-range transport characteristics indicated by the trajectory and the significant increase in the proportion of bare ground surface are consistent with the enhanced performance of INP in the low-temperature segment, and synoptic-scale processes may play a further modulating role."

24. L156-158 Too many things are discussed in a single sentence. The authors might consider explaining this thoroughly over several sentences.

Response: We thank the reviewer for this helpful comment. The text in Lines 220–226 has been rewritten into multiple sentences with clearer logical structure and more explicit explanations.

L220-226: "From December to April, INP concentrations at −15 °C and −20 °C remained relatively stable. In contrast, INPs at −25 °C, −30 °C, and −35 °C showed little variation from December to February, followed by a pronounced increase in March that persisted through April. Over the same period, coarse-mode aerosol (1 μm < D < 2.5 μm) number concentrations showed a similar increase. In addition, mass concentrations of mineral-related ions, such as Ca^{2+}, and crustal trace metals were also higher during this period (Fig. S7; Table S2)."

25. L167-168 It is very hard to see the relation between dust, PM10, and Ca ion concentrations in Fig. S4. Dust time series data are not even seen in Fig. S4. A better presentation needs to be offered; otherwise, the statement here is not convincing. Normalized concentration time series for Panels (c) and (d) might be a better representation to make the authors' point.

Response: We thank the reviewer for this helpful suggestion. We agree that the relationships among dust events, PM_{10}, and Ca^{2+} were not sufficiently clear in the original Fig. S8. In the revised Supplement, we have redesigned Fig. S8 by presenting PM_{10} and Ca^{2+} as separate panels, which substantially improves the readability of their temporal variability. In addition, we have added a panel showing the FMD to better illustrate the dominance of coarse-mode particles during dust events. These changes make the association between dust events and enhanced PM_{10} and Ca^{2+} concentrations more evident.

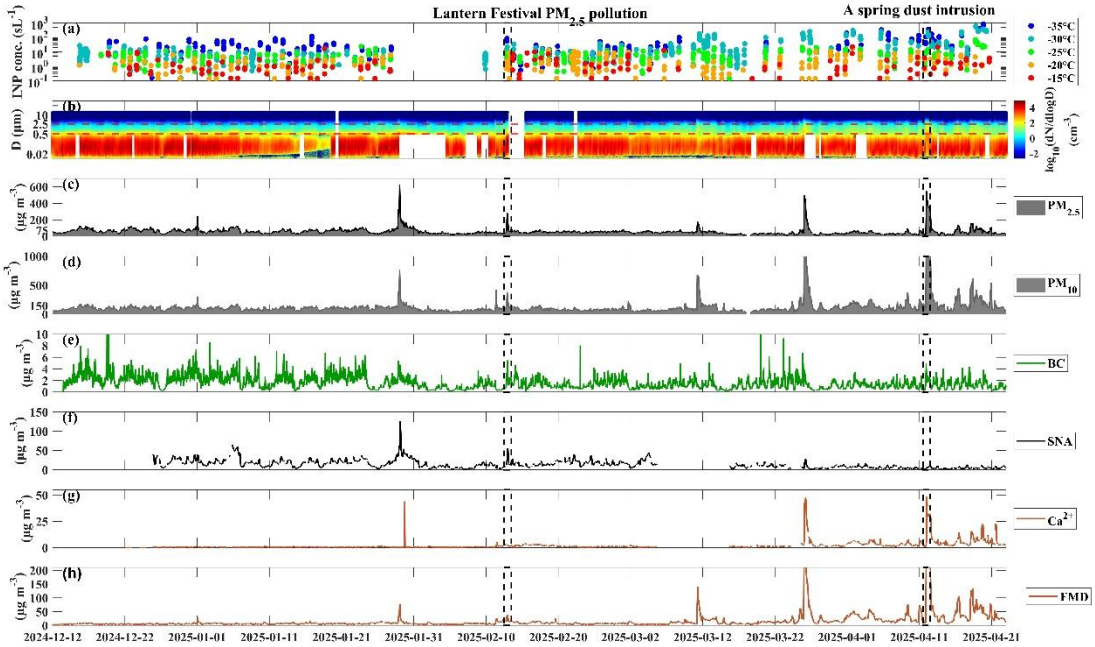

Fig. S8 Time series of INPs and aerosol characteristics.

26. L179 Define aerosol conditions.

Response: Thanks. We agree that the term "aerosol conditions" was ambiguous in the original manuscript. In the revised version, we have replaced this term with "aerosol event types" when referring to the classified categories (Clean, Moderate, $PM_{2.5}$ High, and Dust), and with "aerosol regimes" when describing the general aerosol background. The terminology has been revised throughout the manuscript accordingly.

27. L186-188 Offer a reference for dust event characterization here. Not just in SI.

Response: We thank the reviewer for pointing this out. As suggested, we have added a new Sect. 2.4, describing the aerosol event classification in detail, including the criteria used to identify dust events and the relevant references.

L271-276: "Since the sampling site lacked an online mass concentration monitoring instrument, hourly $PM_{2.5}$ and PM_{10} mass concentration data were obtained from the National Environmental Monitoring Station at the Railway Design Institute, located approximately 1.7 km away. To investigate how different aerosol conditions influence INP activity, we categorized the observation period into four event types (Fig. S6). Dust events were identified by $PM_{10} \geq 150$ μg m^{-3}, while non-dust conditions were further separated into Clean, Moderate, and $PM_{2.5}$ High categories according to $PM_{2.5}$ thresholds of 35 and 75 μg m^{-3}. The PM_{10} can effectively capture the arrival of dust intrusions and has been used in previous studies to identify periods of coarse-mode aerosol enhancement (Lei et al., 2014; Wang et al., 2024),

while $PM_{2.5}$ serves as a useful indicator of pollution severity (Ren et al., 2023). In combination, these two indicators offer a representative and widely comparable basis for separating dust events from varying levels of pollution. The occurrence frequencies and INP characteristics associated with the different aerosol event types are presented in Sect. 3.2."

28. Fig. 5 Why is volume site density offered? What is its significance to surface site density? To the reviewer's knowledge, surface site density is more relevant to INP as IN active sites are presumably on the surface of water-insoluble particles. Why volume matters? Please clarify. Otherwise, the reviewer suggests removing the volume site density discussion. The current manuscript seemingly does not offer the significance of the volume site density.

Response: We highly appreciate this insightful comment. We agree that ice-nucleating active sites are most plausibly associated with particle surfaces, and that surface site density therefore provides a more physically meaningful metric for characterizing INP activity. In the revised manuscript, we have removed the analysis and discussion of volume site density and now focus on surface site density. Corresponding text and figure elements have been deleted or revised accordingly.

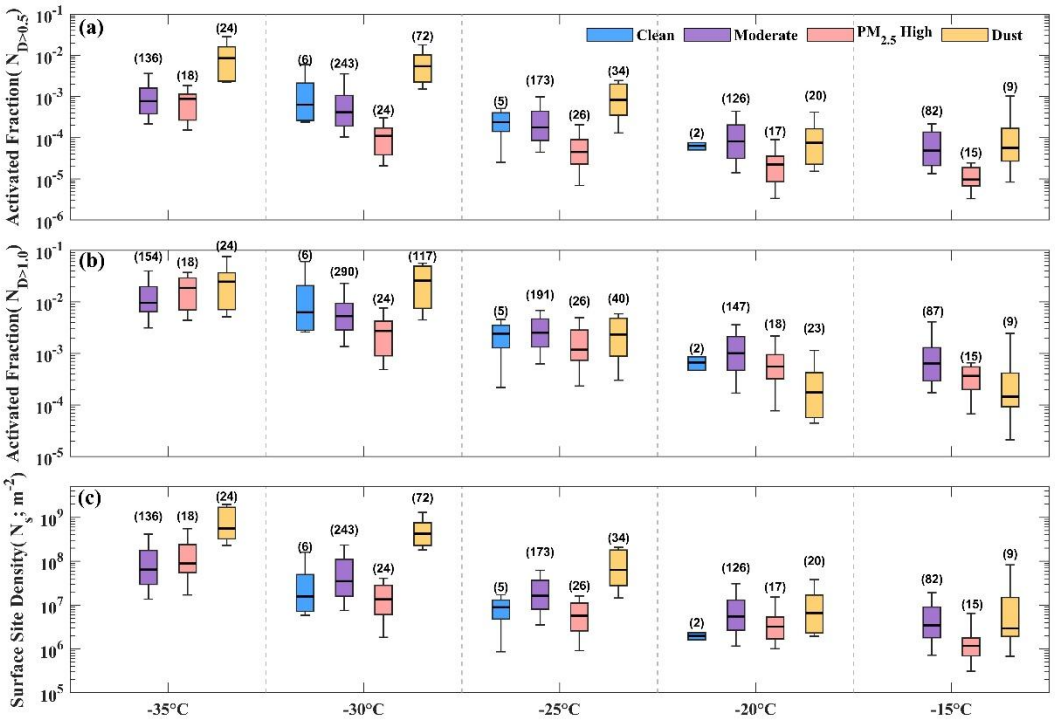

Fig. 4. Distributions of ice nucleation activity parameters.

29. L287-295 Sounds speculative and superficial.

Response: We have substantially expanded and restructured this paragraph to provide a more evidence-based and balanced discussion. The revised text now (i) clarifies the observational basis for the negative SNA–INP relationship under non-dust conditions, (ii) distinguishes observational constraints from mechanistic interpretations, and (iii) discusses both suppressing and non-suppressing effects of chemical aging on mineral-dust INPs reported in laboratory and field studies. These changes reduce speculation and place our interpretation in the context of existing literature.

L360-376: "A key issue, however, is that the negative SNA–INP relationship persists even after transported-dust events are removed, and even in partial correlation analyses that control for FMD to account for the temporal mismatch (Fig. 7c). This indicates that, under non-dust-transport conditions, elevated SNA corresponds to an aerosol regime in which efficient mineral-dust INPs are relatively scarce. Consistent with this interpretation, INP concentrations during $PM_{2.5}$ High conditions tend to be lower than those during Moderate conditions, although the difference is not statistically significant (Fig. 3d). This slight decrease may suggest that mineral dust present under polluted conditions has experienced physicochemical modification, which in turn modulates its ice-nucleating efficiency. Secondary inorganic coatings (e.g., $(NH_4)_2SO_4$) can increase the effective contact angles on mineral surfaces, thereby requiring higher supersaturation for activation (Eastwood et al., 2009; Chernoff and Bertram, 2010). H_2SO_4 coatings can even irreversibly suppress the ice-nucleating activity of mineral dust (Sullivan et al., 2010), indicating that anthropogenic SO_2 and NH_3 emissions can substantially alter the immersion-freezing efficiency of dust particles. At the same time, other studies have reported that the ice-nucleating activity of mineral dust is not necessarily reduced after atmospheric aging, and in some cases remains largely unchanged or even partially enhanced, depending on the chemical composition and extent of surface processing (Kanji et al., 2019; Bertozzi et al., 2021; Chen et al.,2023; Huang et al.,2025). For example, field observations in Beijing reported no systematic difference in mineral-dust-related INP activity between polluted and clean conditions (Chen et al., 2018; Zhang et al., 2022). Taken together, these results indicate that, although pollution aerosols are not efficient immersion freezing INPs, they may still influence the observed INP abundance indirectly when pollution loading is high, and mineral dust is scarce."

30. Fig. 8 looks very busy with many fits. Some can be moved to SI. A better presentation with only crucial info should be offered. The reviewer also wishes to see the comparison of surface site density

parameterizations from this study to previous studies as a function of freezing Ts. Perhaps the surface site density comparison can be offered in another panel.

Response: We thank the reviewer for this constructive suggestion. We agree that the original Fig. 8 was overly busy. Accordingly, we have reduced the number of fitted curves and now retain only the parameterization schemes that are directly discussed in the main text. In addition, we have added a new panel to Fig. 8 showing the comparison of surface site density (N_s) parameterizations from this study with those reported in previous studies as a function of freezing temperature. This revised figure provides a clearer and more focused presentation of the key results and facilitates direct comparison with existing parameterizations.

L413-423: "Temperature-dependent N_s relationships are also presented in Fig. 8b. During Dust events, the derived N_s values are systematically lower than mineral-dust-based parameterizations (Niemand et al., 2012), typically by about 1–2 orders of magnitude. Such discrepancies are not unexpected, as laboratory-derived schemes are known to represent fresh mineral surfaces and tend to overestimate ambient N_s (Connolly et al., 2009; Ullrich et al., 2017). In comparison, the observed Dust-event n_s values are closer to the lower bound of the size-segregated parameterizations proposed by Reicher et al. (2019), particularly the submicron scheme. For Clean, Moderate, and PM2.5 High events, the observed N_s values fall largely within the range predicted by combustion-related parameterizations for vehicle exhaust and biomass-burning aerosols (Schill et al., 2016; 2020), while remaining substantially lower than mineral-dust-based schemes. The pronounced variability in ice-nucleating efficiency across aerosol regimes indicates that temperature-only parameterizations are insufficient. An improved framework should either adopt aerosol-type-specific parameterizations or incorporate aerosol-related indicators that distinguish between dust-influenced and pollution-influenced conditions."

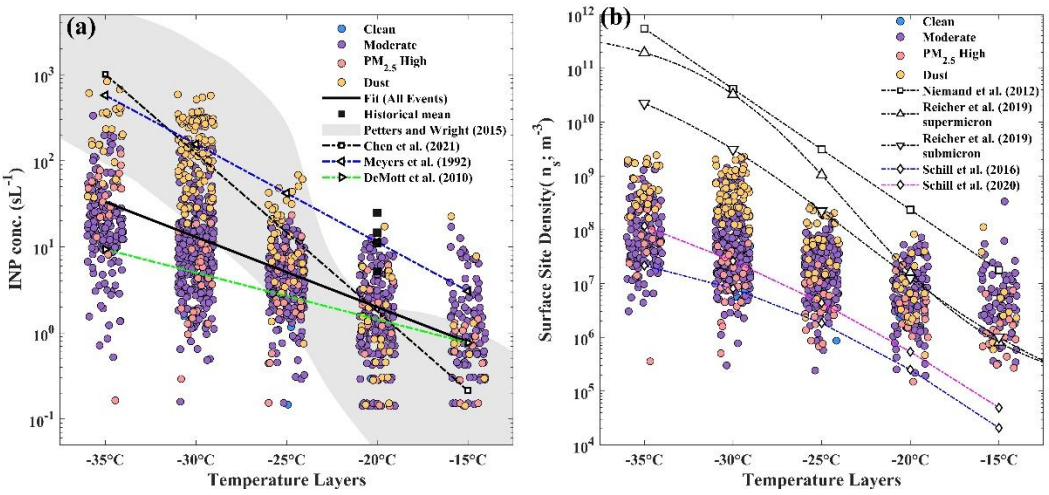

Fig. 8. Construction of single-parameter INPs parameterizations and comparison with existing parameterization schemes

31. Fig. 10 vertical lines at 0.15 sL^-1 on x-axis and horizontal ones ~1 sL^-1 on y-axis look like an artifact. It's probably due to the detection limit of observation and prediction. Regardless, why the minimum INP concentration values are different between observation and prediction? A relevant discussion seems to be missing. The authors may consider calculating time average INP concentrations so that artifact-looking straight lines would disappear from the figure.

Response: We highly appreciate this valuable comment. We agree that the straight-line features at low INP concentrations in Fig. 10 arise primarily from methodological limitations. In the revised manuscript, we clarify that the vertical alignment of observed INP concentrations at approximately 0.13 sL^{-1} reflects the instrumental detection limit of the CFDC measurements. In contrast, the nearly constant predicted INP concentrations at ~1 sL^{-1} at −15 °C indicate that the current parameterization does not capture aerosol-related variability at relatively warm temperatures and effectively yields a single representative value. This explains why the minimum INP concentrations differ between observations and predictions. We have added a corresponding discussion in the main text to explicitly address these issues. While time-averaged INP concentrations could reduce the appearance of such straight-line features, we retain instantaneous data to preserve event-scale variability and instead clarify the origin and implications of these artifacts in the revised manuscript.

L271-276: "Nevertheless, certain limitations remain. The vertical alignment of observed INP concentrations at approximately 0.13 sL^{-1} reflects the instrumental detection limit of the CFDC measurements. At −15 °C, the current parameterization does not capture aerosol-related variability and

effectively yields a single representative value. Despite these limitations, the overall improvement mainly reflects a regional optimization that better suits the urban environment, suggesting that parameterizations may benefit from incorporating region-specific aerosol characteristics when applied to complex environments. In addition, future coordinated online CFDC and offline droplet-freezing measurements may help better constrain low-level INP concentrations."

32. L358-359 This does not fit in the conclusion. Sounds like introductory info.

Response: We thank the reviewer for this comment. Accordingly, we have removed this sentence from the Conclusions section.

33. L366-369 The authors might consider summarizing IN efficiency (surface site density) besides INP conc. here.

Response: We thank the reviewer for this suggestion. Following this comment, we have added a summary of ice-nucleating efficiency based on normalized activation fractions and surface site densities in the Conclusions section, in addition to INP concentrations.

L468-472: "Normalized activation fractions and surface site densities confirm that the elevated INP concentrations during dust events are attributable to intrinsically higher ice-nucleating efficiency of mineral dust, rather than merely increased particle abundance. Backward trajectory and land-cover analyses further show that elevated INPs are linked to long-range transport from arid source regions, while $PM_{2.5}$ High events are mainly associated with near-regional air masses."

34. L370 What are urban-inland INP bursts? Define it well earlier.

Response: We thank the reviewer for this comment. We agree that the term "urban-inland INP bursts" was not sufficiently defined in the original manuscript. In the revised version, we have clarified this term at its first occurrence by defining it as episodic and pronounced increases in INP concentrations. The text has been revised accordingly.

L243-245: "To summarize, the springtime predominance of coarse dust, overriding the wintertime PM2.5 pollution, establishes the requisite conditions for the episodic INP bursts (i.e., episodic and pronounced increases in INP concentrations) that characterize the ice nucleation regime in the inland basin."

Reference

Bi, K., McMeeking, G. R., Ding, D. P., Levin, E. J. T., DeMott, P. J., Zhao, D. L., Wang, F., Liu, Q., Tian, P., Ma, X. C., Chen, Y. B., Huang, M. Y., Zhang, H. L., Gordon, T. D., and Chen, P.: Measurements

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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.**

Overall Evaluation

Chen et al. conducted field measurements using an online automated CFDC instrument from winter 2024 – spring 2025 in Lanzhou, China. Combining INP measurements, ambient monitoring measurements, and air-mass trajectories they determined that dust transport was associated with higher concentrations of immersion INP. Compositional analysis revealed that polluted urban environments are negatively correlated with INP concentrations, with implications for dust-pollution interactions where urban plumes can suppress INP concentrations. This study fits within the scope of ACP. However, there are several scientific issues that need to be addressed in a detailed revision. First, the authors discuss the inclusion of a new CFDC instrument, but details are not provided on the specifics of the instrument and a comparison to existing instruments not provided. The calibration detailed is also lacking, where only two compounds used to validate immersion freezing. Furthermore, the limitations of the compositional analysis provided needs to be included and reorganized. The manuscript would also benefit from reorganization, as methodologies are frequently described in the results. After a careful revision, this manuscript should be reconsidered for publication as a research article.

Response to the Overall Evaluation: We sincerely thank Reviewer 2 for the thorough and constructive evaluation of our manuscript. We appreciate the recognition that the study fits within the scope of ACP and that the dataset provides valuable insight into dust–pollution interactions affecting immersion INPs in a semi-arid urban environment. The reviewer raised concerns regarding (i) the description and calibration scope of the CFDC instrument, (ii) the limitations and organization of the compositional analysis, and (iii) structural issues in which methodological details were embedded within the Results section. In response, we have performed a comprehensive structural and technical revision of the manuscript.

First, we clarify that the instrument used in this study is not a newly developed CFDC system. INP measurements were performed using a commercial Continuous Flow Diffusion Chamber–Ice Activation Spectrometer (CFDC-IAS), which is based on the well-established CSU-CFDC design. The revised manuscript now explicitly states this point to avoid any misunderstanding that a new chamber architecture is being introduced. The operational principles and thermodynamic framework, therefore, follow established CSU-CFDC characterizations rather than representing a new instrument configuration.

In response to the reviewer's concerns regarding calibration, we have revised the wording and clarified the scope of the verification experiments conducted with ammonium sulfate and illite-NX. The

previous term “calibration” has been replaced with “performance tests” to more accurately reflect the intent of these experiments. These tests were conducted to verify stable chamber operation and reliable OPC-based ice-crystal detection under selected temperature and water supersaturation conditions before field deployment. They were not designed to derive full freezing-onset curves or to systematically determine homogeneous freezing thresholds across the entire temperature range investigated. This limitation is now explicitly acknowledged in the revised text. The supersaturation–temperature relationship of the CFDC-IAS follows the factory-calibrated CSU-CFDC design, which has been extensively characterized in previous laboratory and field studies (e.g., Rogers et al., 2001; Kanji et al., 2011; Levin et al., 2014). Readers are referred to those works for detailed freezing-onset behavior across broader thermodynamic conditions.

We have also substantially reorganized and clarified the compositional analysis. A new subsection entitled “Aerosol chemical composition and reconstructed tracers” has been added to the Methods section, where ion measurements, heavy-metal analysis, and reconstructed dust tracers are now described before their application in the Results. Crustal trace metals (Ca, Al, Si, Fe, Ti) measured by the heavy-metal analyzer are now incorporated to strengthen the identification of mineral dust influence. Importantly, the revised Discussion clearly acknowledges the interpretative limits of using bulk compositional proxies to distinguish dust and pollution contributions. We emphasize that these chemical indicators serve as source proxies rather than definitive mineralogical characterization, and that quantitative separation of mixed aerosol sources based solely on bulk composition carries inherent uncertainty. Dust influence in this study is therefore inferred from converging lines of evidence, including PM_{10}-based classification, trace metal enrichment, particle size distributions, and backward-trajectory analysis, rather than from a single compositional metric.

To improve structural clarity, we have thoroughly reorganized the manuscript. All methodological descriptions that were previously embedded within the Results section have been relocated to the Methods. A dedicated subsection on aerosol event categorization has been introduced to define dust and non-dust conditions before they are referenced in the Results. The HYSPLIT backward-trajectory analysis is now presented in its own section to clearly separate methodology from interpretation. Figures have also been simplified to remove redundancy and improve readability.

We believe that these revisions substantially strengthen the manuscript by enhancing technical transparency, clearly delineating methodological scope, explicitly acknowledging interpretative limits, and improving overall structural coherence. We sincerely appreciate the reviewer’s detailed critique, which has helped improve the clarity and rigor of the work.

Detailed comments

1. Title should be specific you are testing immersion freezing. Maybe replace ice nucleation with Immersion Freezing?

Response: We thank the reviewer for this helpful suggestion. We agree that our measurements specifically quantify immersion freezing. In the atmospheric INP literature, “ice nucleation” is commonly used to describe the overall ice-nucleating behavior of aerosol particles, particularly in field-

based studies. Although the CFDC primarily measures immersion freezing, other nucleation modes may also occur to varying degrees within its chamber. Therefore, we consider the broader terminology more appropriate for the scope of this work.

2. Line 14 Elevated secondary inorganic aerosol associated with urban pollution was enhanced in the winter and…

Response: Thanks. The manuscript has been revised accordingly.

Line 14: "Elevated secondary inorganic aerosol during pollution in winter …"

3. Line 21 – While true, it can depend on region. Provide a citation to strengthen the statement that INP are a very low constituent of background INP.

Response: Thanks for this comment, and the parentheses have been deleted. After carefully reconsidering this statement in light of both reviewers' comments, we found that the sentence may lead to overgeneralization across different regions. To maintain precision and avoid potential ambiguity, we have removed the statement from the revised manuscript.

Line 22: "Despite their extremely low atmospheric concentrations …"

4. Line 35 – 38. This is misleading as currently stated and can be improved. The role of BBA is highly debatable within the INP community and arguably not largely supported by experiments. You even reference the study by (Chen et al., 2025) describing this discrepancy is likely due to co-lofted materials such as dust rather than a systematic underestimation of BBA particles themselves. Numerous compositional aircraft ice residual analyses also not corroborate this finding either, as cloud residuals are frequently depleted in carbonaceous BB. From the paper cited, BBA was also binned into categories (residential, agricultural, natural, and others) rather than a lumped BBA.

Response: We highly appreciate this important comment. We agree that the role of biomass burning aerosols (BBAs) as a source of INPs remains debated and is not firmly established by current experimental evidence. Accordingly, we have revised Lines 38-42 to adopt a more cautious wording.

Line 38-42: "On a global scale, the contribution of biomass burning aerosols (BBAs), mainly emitted by wildfires, to atmospheric INPs remains uncertain and highly variable. Although some field observations report elevated INP concentrations under biomass-burning-influenced conditions (Zhao et al., 2024), this association may partly reflect the presence of co-lofted materials such as mineral dust rather than a direct contribution from BBAs themselves (Chen et al., 2025)."

5. Line 39 – 42 Metallic particles from industrial sources in urban environments have also been identified as an INP source. You later measure this so indicating their importance prior would be beneficial.

Response: A statement emphasizing the relevance of metal and metal-oxide particles to atmospheric INPs has been added to the Introduction.

Line 46-49: "The ice-nucleating efficiency of metal and metal-oxide particles varies strongly with chemical composition, and many pure metals and metal oxides exhibit poor ice-nucleating ability (Yakobi-Hancock et al., 2013); however, 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."

6. Line 83 Please include AGL height to your latitude and longitude coordinates.

Response: The sampling height above ground level (AGL) has been added to the site description in Line 92.

Line 92: "The sampling site was located on the campus of Lanzhou University (103.86°E, 36.05°N, 3m above ground level, AGL) in Lanzhou City, Northwest China, which represents a typical semi-arid inland urban environment (Fig. S1)."

7. Line 87 A critical description or citation of the aerosol inlet used is missing from the methodology. For any field measurement studies, this description is essential. At the very least, an existing citation be provided with a D50.

Response: A brief description of the aerosol inlet, together with an appropriate citation, has been added to the methodology.

Line 100-102: "Ambient aerosols were sampled using the same container-based mobile observation system as described in Wang et al. (2025), without an external size-selective inlet. The CFDC instrument is equipped with two size-selective inlets connected in series, and details are provided in Sect. 2.5."

8. Fig 1. Contrast of text, trajectories, and background colors make this figure difficult to read, particularly panel A. Perhaps reducing the opacity of the background? You can also label the CFDC as CFDC-IAS to be more specific as this figure is introduced before the instrumentation is described.

Response: We thank the reviewer for this helpful comment. We have revised Fig. 1 to improve its overall readability and visual clarity. Specifically, the background and color contrast have been adjusted, and the previous instrument photograph has been removed to reduce visual complexity. In addition, an urban

functional zoning map has been incorporated, which better highlights major land-use features around Lanzhou and allows industrial source regions in the vicinity of the sampling site to be more clearly identified.

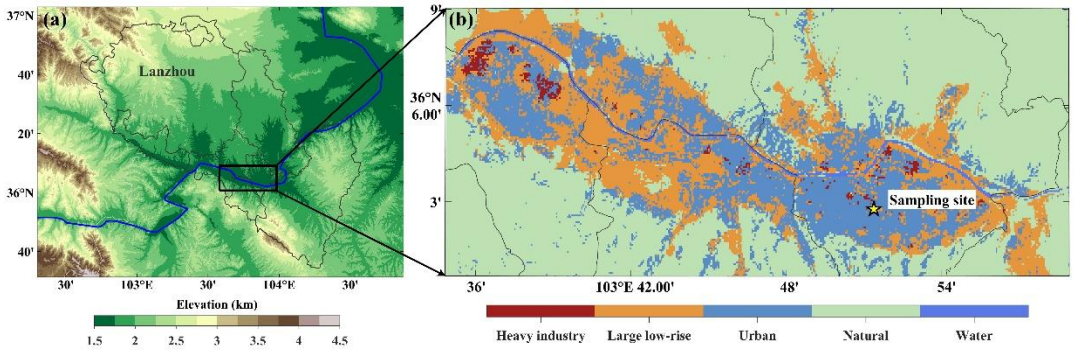

Fig.1. Overview of the location of observations.

9. Line 87-97 In your overview of sampling methods conducted, you are unspecific in instrumentation for everything but the Aethalometer and heavy metal analyzer. Be consistent in how you are describing instrumentation for the overview. This can inadvertently highlight certain instruments as the "highlight(s)" of the campaign. Arguably, list the variables measured and reference the appropriate sections of the manuscript where they are discussed in more detail.

Response: We appreciate this valuable comment. We have revised Lines 98–100 to provide a more consistent and balanced overview of the measurement campaign. In the revised text, instrument-specific descriptions have been removed, and the overview now focuses on the atmospheric variables measured (e.g., INP concentrations, aerosol particle number size distributions, black carbon mass concentrations, and aerosol chemical composition), with references to the relevant sections where the corresponding instrumentation and methodologies are described in detail. This revision avoids unintentionally highlighting particular instruments and improves the consistency between the overview and the subsequent methods sections.

Line 98-100: "The observation was conducted using a container-type mobile station, where key monitoring parameters, including INP concentration and aerosol particle number size distribution (N(D)), and black carbon (BC) mass concentration, were continuously measured."

10. Line 92 Are there any pertinent citations on the operation of these monitoring stations?

Response: Yes. We have now added pertinent citations describing the operation and configuration of the monitoring station.

Line 104: “A detailed description of the monitoring station can be found elsewhere (Du et al., 2020; Li et al., 2023).”

11. Line 94-95 Measurement units for compositional aspects? Are these arbitrary ion intensities?

Response: The compositional data represent quantitative mass concentrations rather than arbitrary signal intensities. Units (μg m^{-3}) have now been added.

Line 125-129: “In addition, The National Atmospheric Super Monitoring Station, provided PM2.5 measurements of major water-soluble inorganic ions (NH_4^+, NO_3^-, SO42−, Na+, Mg2+, Ca2+, Cl−, and K^+) as mass concentrations (μg m^{-3}) from an online ion chromatograph (IC) and elemental mass concentrations (μg m-3; e.g., Ca, Al, Fe, Ti, Si) from a heavy-metal analyzer (XHAM-2000), offering essential support for the aerosol composition analysis (Tian et al.,2023)”

12. Line 99 – 100. The SMPS measures the electrical mobility diameter. What shape factor, particle density, and other corrections are you using to get an equivalent Stokes diameter?

Response: Thanks for your constructive comments. The SMPS classifies particles according to their electrical mobility and directly reports the electrical mobility diameter. In the present study, no explicit conversion from mobility diameter to Stokes diameter was applied. Instead, the Stokes diameter (D) is used as a Stokes-equivalent diameter and is assumed to be numerically equal to the electrical mobility diameter under the common assumption of spherical particles, consistent with established aerosol measurement theory (Hinds, 1999; Baron and Willeke, 2011). Under the spherical particle assumption, they are numerically identical. Therefore, no particle density, shape factor, or additional corrections are required for the SMPS-derived particle sizes.

Line 106-109: “A Scanning Mobility Particle Sizer (SMPS; Model 3082, TSI Inc.) was employed to measure the electrical mobility diameter in the size range of 13.6 nm to 532.8 nm, divided into 103 bins, with a complete scan conducted every two minutes. The Stokes diameter is denoted as D and is approximately equal to the electrical mobility diameter (Hinds, 1999; Baron and Willeke, 2011).”

13. Line 105 I would add a sentence that you are converting diameters to merge the SMPS and APS measurements together.

Response: Thanks! A sentence has been added to clarify that the APS aerodynamic diameters were converted to D in order to place size distributions from different instruments on a consistent particle size basis and enable their merging. The manuscript has been revised accordingly.

Line 112-114: "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}, a typical value for urban aerosol environments (Zhang et al., 2022), so that the size distributions from different instruments could be merged."

14. Line 106 Averaging isn't what allows you to merge the data sets, the equivalent diameter conversion in combination with averaging is.

Response: We agree with the reviewer. The manuscript has been revised to clarify that merging of the size distributions is enabled by the equivalent diameter conversion, whereas the 10-minute averaging is applied to facilitate temporal alignment among datasets. The corresponding text has been simplified accordingly.

Title 115-116: "All particle size distribution data were averaged to 10-minute intervals to enable temporal alignment and reduce short-term statistical fluctuations before merging."

Section 2.3

15. Major Revision: If this is a new CFDC instrument as you indicate, you need to include a much more thorough description of the instrument. At the minimum it should match a short version of existing descriptions of CFDCs (Garimella et al., 2016; Kulkarni and Kok, 2012; Lacher et al., 2017; Rogers et al., 2001; Stetzer et al., 2008). If this is a previously used instrument, you can cite that study but as you state it is new.

Response: This valuable comment is highly appreciated. As suggested, we clarify that the instrument used in this study is not a newly developed CFDC. INP measurements were performed using a commercial CFDC-IAS based on the established CSU-CFDC design. The manuscript has been revised to explicitly state this and to cite the relevant previous descriptions (Rogers et al., 2001; DeMott et al., 2015; Bi et al., 2019).

Line 156-159: "INP measurements in this study were performed using a commercial Continuous Flow Diffusion Chamber–Ice Activation Spectrometer (CFDC-IAS), which is based on the well-established CSU-CFDC design (Rogers et al., 2001; DeMott et al., 2015) and is capable of autonomous operation with minimal in-person handling and full remote access (Bi et al., 2019)."

16. Line 115 Design of chamber? Relationship between flow, vapor pressure, and temperature? Steady state profiles? Length of main chamber and evaporative section? Aerosol inlet? This all needs to be addressed if it's a new instrument.

Response: Thanks for this great suggestion. As clarified above, the instrument used in this study is not a newly developed CFDC. The chamber design, flow configuration, temperature, and vapor pressure control, and steady-state profiles follow the established CSU-CFDC architecture, which has been described in detail in previous studies (Rogers et al., 2001; DeMott et al., 2015; Bi et al., 2019). The manuscript has been revised to explicitly state that a commercial CFDC-IAS based on this design was used, and readers are referred to these references for detailed technical descriptions.

Line 159-160: "Detailed technical specifications and theoretical descriptions have been comprehensively documented in previous instrument publications (Lacher et al., 2024; DeMott et al., 2017, 2025)."

17. Line 120 Is there a reason your sheath to sample flow ratio is 5? That seems low in comparison to other instruments and could lead to significant lamina spreading and particle losses (Garimella et al., 2017).

Response: We thank the reviewer for this valuable comment. The sheath-to-sample flow ratio of 5 is the standard operating configuration of the commercial CFDC-IAS used in this study and follows the established CSU-CFDC design. No abnormal behavior indicative of substantial particle losses, such as unstable particle transmission or anomalously low ice detection signals, was observed during operation.

18. Line 125 State the freezing modes of your calibrations. You also need to be consistent with your units for water saturation. Supplement uses SSw and methods use RHw. I would also include the onset conditions you achieved for these calibrants and provide a brief comparison to literature.

Response: Thanks for this suggestion. We note, however, that the experiments with illite-NX and ammonium sulfate were not designed to systematically determine ice nucleation onset conditions. Instead, these tests were performed at fixed temperatures (−20, −25, and −30 °C) and selected SSw settings solely to examine the OPC counting response and peak behavior for ice crystal detection.

As a result, quantitative onset temperatures and SSw values for these materials were not derived in this study. Such onset conditions have been extensively reported in previous CFDC and cold-stage studies (e.g., Niemand et al., 2012; Ullrich et al., 2017; Knopf et al., 2018), and we refer readers to those works for detailed freezing onset characteristics.

Line 166-168: "Water supersaturation conditions (typically 4–6% SS_w), consistent with those reported by Moore et al. (2024) for immersion freezing, were maintained in the growth section. Here, SS_w denotes supersaturation with respect to liquid water and is defined as $SS_w = RH_w - 100\%$."

Line 169-175: “Before the experimental observations, the operational performance of the CFDC was assessed using ammonium sulfate and illite-NX under representative temperature and supersaturation conditions (Fig. S4–S5). These tests were intended to verify stable OPC-based ice crystal detection and chamber operation under immersion-freezing conditions. Illite-NX and ammonium sulfate are standard reference materials widely used in CSU-CFDC studies to characterize heterogeneous and homogeneous freezing (Richardson et al., 2010; Hiranuma et al.,2015). The thermodynamic freezing onset characteristics of these materials have been systematically documented for the standard CSU-CFDC configuration over comparable temperature and supersaturation ranges (e.g., Rogers et al., 2001; Kanji et al., 2011; Levin et al., 2014).”

19. Line 125 I would demonstrate that your freezing onsets for your calibrants are consistent with literature values across a broader range of conditions. Your calibration also seems limited to temperatures in the range of -20 to -30 °C but conducted experiments from −15°C to −35°C? Ideally you should plot your onsets of standards on a temperature vs RHice or RHliq and include respective lines for water saturation, homogeneous freezing etc. For new CFDC’s, obtaining a homogeneous freezing point is required to validate functionality and can be provided with ammonium hydrogen sulfate or ammonium nitrate which should only undergo homogeneous freezing. For a new instrument, presenting this summary of calibrations in the article is important.

Response: We thank the reviewer for this suggestion. As clarified above, the CFDC-IAS used in this study is not a newly developed instrument but a commercial system based on the established CSU-CFDC design, whose thermodynamic freezing characteristics and homogeneous freezing benchmarks have been extensively documented in previous instrument studies (e.g., Rogers et al., 2001; DeMott et al., 2015; Lacher et al., 2024).

The pre-campaign tests performed here were intended to verify stable chamber operation and reliable OPC-based ice detection under representative immersion-freezing conditions, rather than to re-establish full freezing onset curves across the entire temperature–RH space. Because no structural modification was made to the factory-validated instrument, additional homogeneous freezing validation experiments were not repeated in this field study. We have clarified this scope in the revised manuscript.

Line 156-159: “INP measurements in this study were performed using a commercial Continuous Flow Diffusion Chamber–Ice Activation Spectrometer (CFDC-IAS), which is based on the well-established

CSU-CFDC design (Rogers et al., 2001; DeMott et al., 2015) and is capable of autonomous operation with minimal in-person handling and full remote access (Bi et al., 2019)."

Line 159-160: "Detailed technical specifications and theoretical descriptions have been comprehensively documented in previous instrument publications (Lacher et al., 2024; DeMott et al., 2017, 2025)."

Line 171-172: "These tests were intended to verify stable OPC-based ice crystal detection and chamber operation under immersion-freezing conditions."

Line 173-175: "The thermodynamic freezing onset characteristics of these materials have been systematically documented for the standard CSU-CFDC configuration over comparable temperature and supersaturation ranges (e.g., Rogers et al., 2001; Kanji et al., 2011; Levin et al., 2014)."

20. Line 127 It may be better to just formally refer to aerosol transmission as D50 throughout the article and briefly explain it before the first use (inlet preferably). Cut size may not be intuitive to some readers.

Response: The manuscript has been revised to consistently use D50 to describe the aerosol transmission characteristic of the impactors, and a brief definition of D50 has been added at its first occurrence.

Line 176-179: "Aerosol particles that could interfere with the optical detection of ice crystals were removed by passing the aerosol stream through two identical single-jet impactors in series (D50 = 2.5 μm), where D50 denotes the aerodynamic particle diameter at which 50% of particles are transmitted through the impactor."

21. Line 144 There isn't just results in this section as you also include interpretation. It should be labeled Results and Discussion to make this clear.

Response: The section heading has been changed from Results to Results and Discussions.

22. Fig. 2 - There is a lot of information here and some of it is redundant when considering Fig. 3. I would only select subplots for the monthly variations and seasonal differences and related aerosol characteristics be moved.

Response: We agree with the reviewer. Fig. 2 has been simplified to retain only the subplots showing monthly variations and seasonal differences, while the related aerosol characteristics have been moved to Fig. S7.

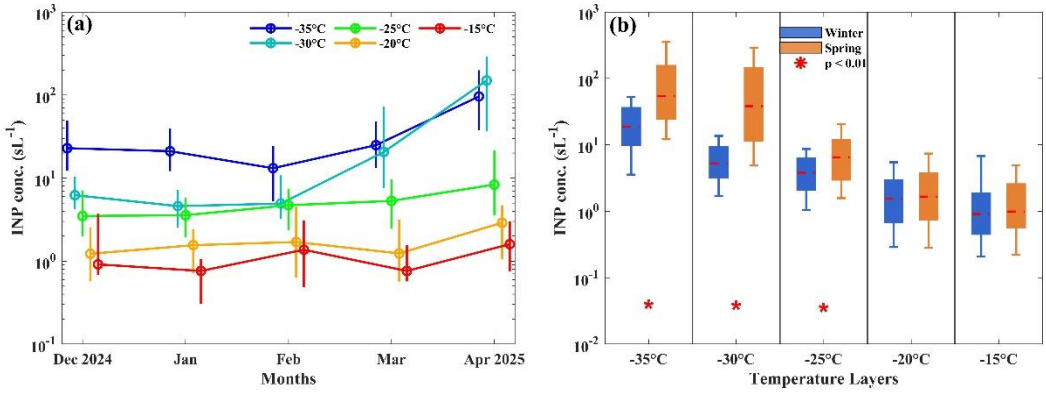

Fig. 2. Monthly variations and Seasonal differences in INPs.

23. Line 155 – 178 Your results/conclusion appear to depend on the assumption that PM10 and water-soluble Ca2+ are definitive markers of mineral dust, with HYSPLIT corroborating this result. While I appreciate the inclusion of composition measurements, I would be very clear in explaining that your chromatography ion concentrations are the soluble fraction in figures and further results. You stated measurements were also collected using a XRF heavy-metal analyzer, but I do not see those presented until later and only in the context of PM2.5. Was that instrument measuring PM2.5 only? This is a more convincing technique to indicate mineral dust aerosol rather than IC which only gives information on surface water soluble material. Al, Fe, and Si would be much stronger markers than water soluble Ca. As you indicated later, the IC aspect is important for aged mineral dust but to define the presence of dust using IC isn't appropriate.

Response: We agree with the reviewer. We now clarify that IC measurements represent only water-soluble fractions. To better indicate mineral dust, we have added Table S1 showing monthly and seasonal concentrations of crustal trace metals (Ca, Al, Si, Fe, and Ti) measured by the XRF analyzer ($PM_{2.5}$). The manuscript has been revised accordingly.

Line 221-225: "In contrast, INPs at −25 °C, −30 °C, and −35 °C showed little variation from December to February, followed by a pronounced increase in March that persisted through April. Over the same period, coarse-mode aerosol (1 μm < D < 2.5 μm) number concentrations showed a similar increase. In addition, mass concentrations of water-soluble mineral-related ions, such as Ca^{2+}, and crustal trace metals were also higher during this period (Fig. S7; Table S1)."

Line 227-230: "Seasonal comparisons further confirmed the significance of these differences: spring INP concentrations were a factor of 1.4–2.2 higher than winter levels (Mann–Whitney U test; Fig. 2b). These

increases coincided with enhanced coarse-mode aerosol and Ca^{2+}, as well as elevated crustal trace metals (Table S1), whereas $PM_{2.5}$, BC, SO_4^{2-}, NO_3^-, NH_4^+, and Cl^- were all higher in winter (Fig. S7)."

Line 235-236: "Indeed, a Spring dust intrusion coincided with enhanced coarse-mode aerosol, PM_{10}, FMD, and Ca^{2+}, accompanied by a pronounced increase in INP concentrations at −25 °C, −30 °C, and −35 °C (Fig. S8)."

24. Line 162 I am surprised your BC is not correlated with K as it is a strong biomass burning marker.

Response: We thank the reviewer for this insightful comment. Potassium is indeed widely used as a tracer for biomass burning in many environments. However, in urban Lanzhou, elevated K^+ concentrations are frequently associated with episodic fireworks and fireworks-related emissions during festival periods, in addition to resuspended dust and industrial sources. These sources are not necessarily accompanied by strong BC emissions. Meanwhile, BC in Lanzhou is mainly influenced by fossil-fuel combustion and traffic-related emissions. This source mismatch between BC and K^+ in an urban setting likely explains the absence of a significant BC–K correlation in our dataset.

25. Line 180 Clearly define the clean, moderate, PM2.5 high, and dust categories including any thresholds you considered. This would be better placed in the methods early on so the reader expects to see these categories and knows how they are defined. There is also no definition of "high" provided. What is the mass threshold being used? If you associate this with specific ions (particularly nitrate and sulfate), then perhaps categorized as Urban?

Response: We thank the reviewer for this helpful suggestion. We have added a new section (Sect. 2.4, Aerosol event categorization) in the Methods to define the aerosol categories before their use in the Results. Specifically, Dust events are identified by $PM_{10} \geq 150$ μg m^{-3}. Non-dust conditions are further classified into Clean, Moderate, and $PM_{2.5}$ High using $PM_{2.5}$ thresholds of 35 and 75 μg m^{-3}, corresponding to Clean ($PM_{2.5} < 35$ μg m^{-3}), Moderate ($35 \leq PM_{2.5} < 75$ μg m^{-3}), and $PM_{2.5}$ High ($PM_{2.5} \geq 75$ μg m^{-3}). We use the term "$PM_{2.5}$ High" rather than "Urban" because the classification is based on mass concentration thresholds rather than inferred emission sources.

Line 148-150: "Dust events were identified by PM10 ≥ 150 μg m^{-3}, while non-dust conditions were further separated into Clean, Moderate, and $PM_{2.5}$ High categories according to $PM_{2.5}$ thresholds of 35 and 75 μg m^{-3}."

26. Line 186 Here you define dust events without water soluble Ca. Be consistent throughout the article on how you are defining dust.

Response: We thank the reviewer for pointing out this inconsistency. We clarify that dust events in this study are consistently defined using PM_{10} mass concentration thresholds ($PM_{10} \geq 150$ μg m^{-3}), as described in Sect. 2.4. Particle size distributions, water-soluble ions (e.g., Ca^{2+}), and crustal trace metals are used as independent chemical and microphysical evidence to verify the dust influence, rather than as classification criteria. In addition, HYSPLIT backward-trajectory analysis provides transport-based evidence for the dust origin. We have revised the relevant text throughout the manuscript to clearly distinguish between the mass-based classification and these supporting lines of evidence.

27. Line 191 Which ions and specify these are water soluble ions

Response: Thanks for this comment. We have revised the text to clarify that the ions refer to major water-soluble ions, and we additionally included BC and FMD as complementary chemical indicators of particle sources.

Line 254-256: "Finally, chemical composition indicators, including major ions, BC, and FMD, provided conclusive evidence for distinct particle sources (Fig. 3c)"

28. Fig 3. Should have the heavy metal analysis included.

Response: We thank the reviewer for this helpful suggestion. Following this comment, we have added FMD in Fig. 3c as a chemical indicator derived from crustal trace metals.

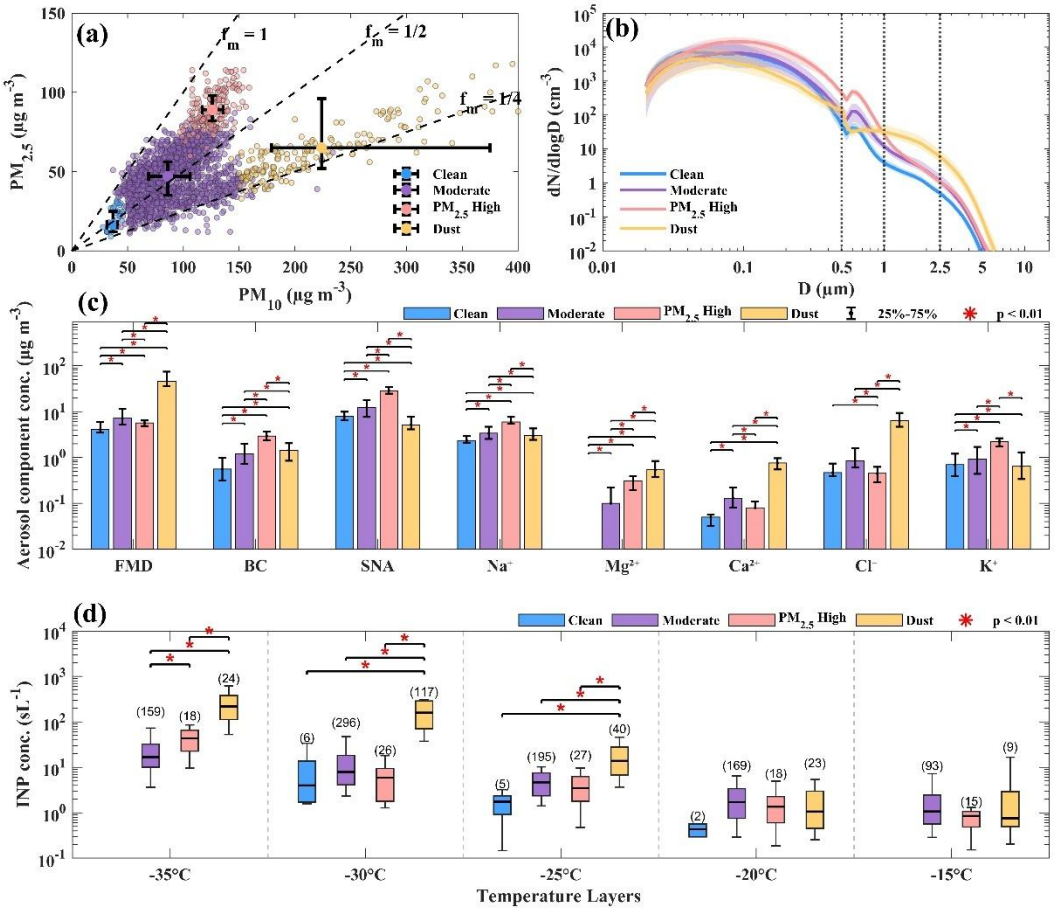

Fig. 3. Comparative analysis of physicochemical characteristics and INP concentrations across different aerosol event types.

29. Line 214 This is vague, if the atmosphere is clean wouldn't the low INP concentration be due to the low concentration of aerosol in general not the ice nucleating activity?

Response: Thanks for this insightful comment. We agree that lower INP concentrations during Clean events likely reflect reduced aerosol loading rather than weaker intrinsic ice-nucleating activity. The sentence has been revised to clarify this point.

Line 277-280: "As expected, Clean events consistently exhibited the lowest INP concentrations across all temperatures, aligning with the overall low aerosol loading and reduced abundance of coarse-mode particles."

30. Line 217 The HYSPLIT analysis should be its own section.

Response: We appreciate this constructive suggestion. Following this comment, we have reorganized the manuscript and moved the HYSPLIT backward-trajectory analysis into a dedicated section (Sect. 3.3, Backward trajectory and source region analysis), where the methodology and results are now presented

in a more coherent and focused manner. Additional analyses of trajectory clustering and source-region characteristics have also been included in this section.

31. Line 230 Satellite imagery rather than elevation would be more convincing in showing dust sources. You could also shade/circle desert regions for the source trajectories. Include in the HYSPLIT section.

Response: We thank the reviewer for this helpful suggestion. As suggested, we have revised the figure and analysis in the HYSPLIT section. The main figure now shows land-cover types, including desert and barren surfaces, to more directly indicate potential dust source regions along the trajectories. The elevation map has been moved to the Supplementary Material, where it is provided as supporting information. This revision allows the main figure to better highlight source-region characteristics relevant to dust emissions.

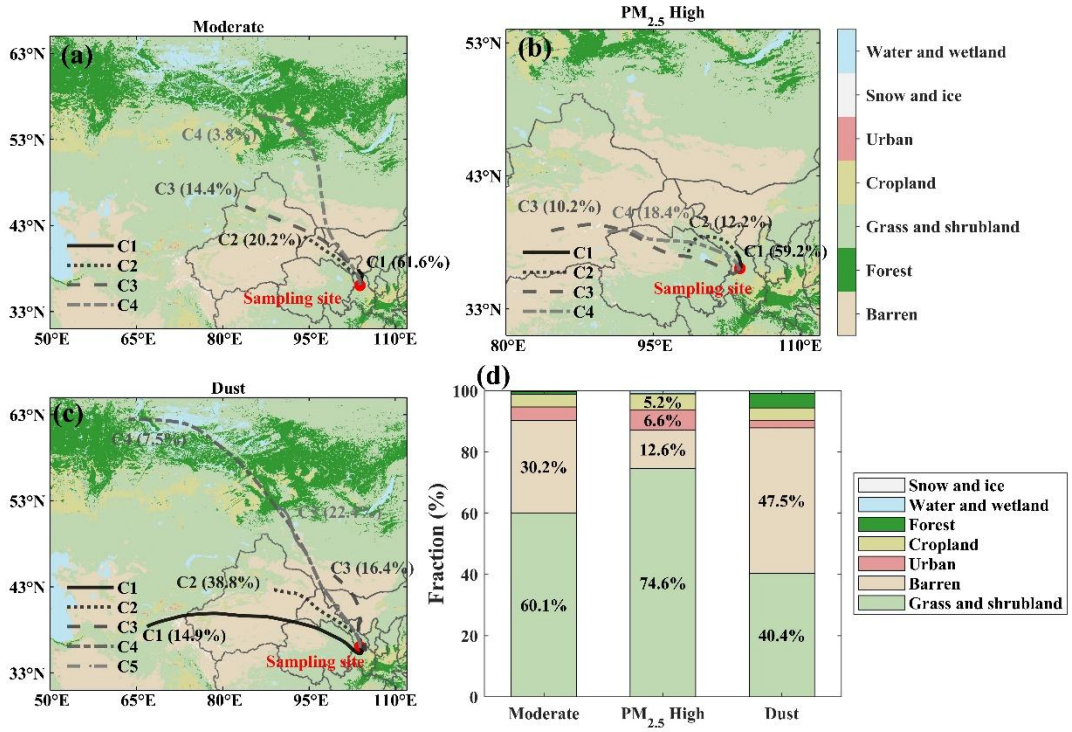

Fig. 5. Spatial distributions of 72-h backward air-mass trajectories and associated land-cover characteristics for different aerosol events.

32. Fig. 5 I agree with Reviewer 1, the volume site density isn't previously described and is confusing here.

Response: We thank the reviewer for this comment and agree that the volume site density was not sufficiently introduced earlier in the manuscript, which may confuse. Following this suggestion, we have

removed the volume site density from Fig. 5 to maintain clarity and consistency with the defined activity metrics.

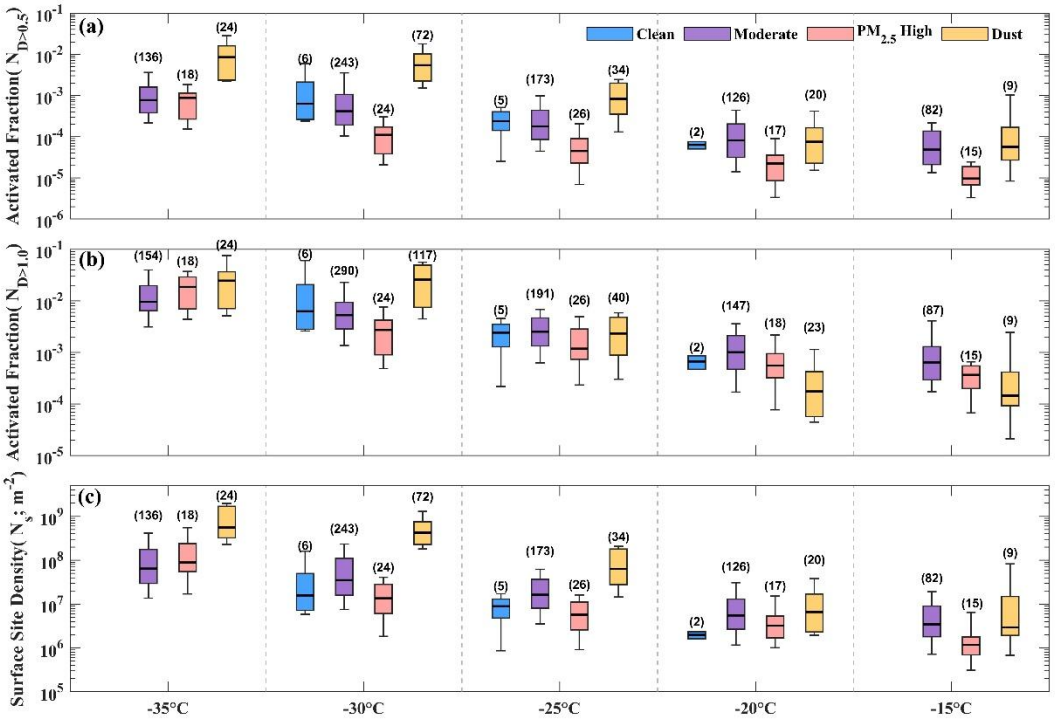

Fig. 4. Distributions of ice nucleation activity parameters.

33. Line 245 Given your compositional measurements, I would include these here to strengthen the argument this is dust.

Response: We thank the reviewer for this suggestion. During the reorganization of the manuscript, the sentence at Line 245 was removed, and the identification of dust influence is now supported by aerosol compositional measurements (major water-soluble ions, BC, and FMD) presented elsewhere in the Results and described in Sect. 2.3.

34. Line 247 Define SNA before using the acronym.

Response: Thanks for this comment. The acronym SNA is now defined at its first occurrence in Sect. 2.3 (Aerosol chemical composition and reconstructed tracers) before being used in the Results section.

35. Lines 253 – 280 Definitions of compositional methods should be in the methods section, not the results. These are also supported by citations which would strengthen the argument earlier rather than later. I also see more inclusions of the heavy metal analysis

Response: Following this comment, we have moved the definitions and descriptions of the compositional methods from the Results section to a new Methods subsection (Sect. 2.3, Aerosol

chemical composition and reconstructed tracers), where they are now introduced before their application. This section also includes the relevant citations supporting these approaches, as well as a clearer description of the heavy-metal-based analysis and reconstructed tracers. The Results section now focuses on presenting and interpreting the corresponding outcomes.

36. Line 296 This sentence is confusing as it is currently written.

Response: Sorry for the confusion. We have rephrased the sentence to better reflect the transition from correlation analysis to the subsequent dust-sensitivity analysis.

Line 377-378: "Beyond the correlation analysis, this study further evaluated the sensitivity of INPs to dust under different levels of SNA loading."

37. Fig. 9 There are a lot of fits on each of these plots, particularly the top row. The correlations between these are also very low. I wonder if this figure may be better placed in the supplement or only include the figures you reference in Lines 325 – 335.

Response: Thanks for this helpful suggestion. As suggested, we have revised Fig. 9 to retain only the correlation plots for particles larger than 1.0 μm, which are directly discussed in Lines 325–335. The correlation plots for particles larger than 0.5 μm have been moved to the Supplementary Material. This revision allows Fig. 9 to focus on the size range most relevant to ice-nucleating particles.

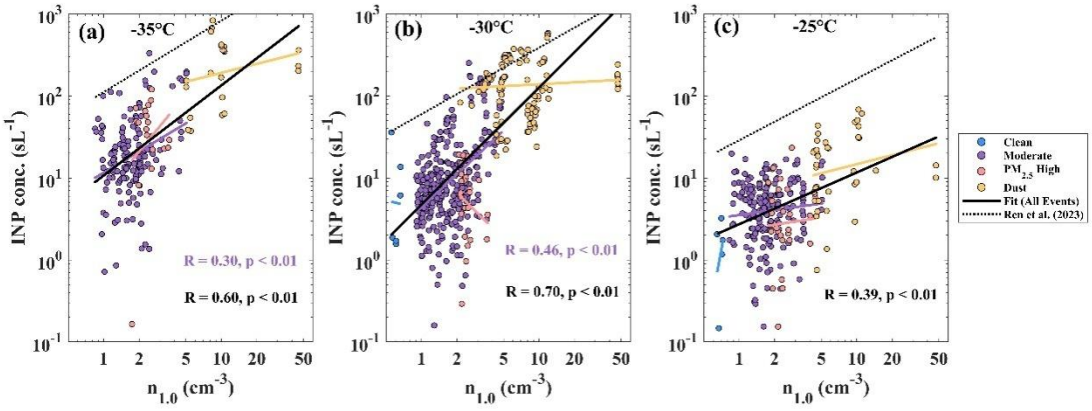

Fig. 9. Relationships between INPs and aerosol number concentrations.

38. Lines 358 – 365 This reads more like introductory material.

Response: Thanks. We agree that the text in Lines 358–365 was introductory in nature. Accordingly, this paragraph has been removed from the Conclusions section.

39. Lines 384 …study provides… measurements that can improve parameterizations of aerosol-ice-cloud interactions in North-West China with implications for other dryland cities where urban air interacts with transported dust.

Response: We have revised the conclusion to emphasize that our measurements provide region-specific constraints that can improve parameterizations of aerosol–ice–cloud interactions in North-West China, with implications for other dryland cities.

Line: 489-491: "This study could provide region-specific constraints on aerosol–ice-cloud interactions 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

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