Author Response to Referee #2
EGUSPHERE-2026-308

This study assessed the ice nucleating particle (INP) concentrations and efficiencies in an urban site in China. The authors were able to identify a clear mineral dust event as well as other high pollution events during their sampling period. Additionally, most of the sampling days were assigned as a "non-dust period" considering aerosol composition, particle size distributions, and air masses history. Although INPs are poorly constrained and studied in urban sites, the manuscript requires deep corrections. Also, I think this manuscript better fits into the "Measurement report" category.
Response: We thank the reviewer for the careful reading of our manuscript and for recognizing the value of the Taiyuan INP dataset. We appreciate the reviewer's constructive comments and have carefully revised the manuscript accordingly. Detailed point-by-point responses are provided below.

Major comments:

1. The authors made a distinction between a "dust event" and a "non-dust period". Although the INP concentration and ns values are quite different between both periods, I am not fully convinced that the "non-dust period" is really a period without the influence of mineral dust particles. As shown in the back-trajectories, during the "non-dust period" the airmasses crossed northern China and southern Mongolia deserts, including the Gobi Desert. Also, the elemental analysis is not a good way to confirm the absence of mineral dust particles during the "non-dust period" as aluminum (Al) and silicon (Si) were not included. This a key point to be addressed as most of the drawn conclusions took this apparent distinction into account.

Response: We agree that the term "non-dust period" in the original manuscript was not sufficiently precise, because it may imply a complete absence of mineral dust influence. Our intention was only to distinguish the clearly identified desert dust intrusion event (5–9 December) from the remaining observation periods. We have therefore revised the terminology throughout the manuscript to "non-desert-dust periods" (or equivalent cautious wording, depending on context). We now clarify that the desert dust event was identified based on multiple lines of evidence, including backward trajectories, coarse-mode particle enhancement, and elevated PM_{10}. Outside this event, we do not claim that mineral dust was absent, but only that no similarly clear desert dust intrusion signature was identified. The relevant interpretations have been revised accordingly.
Changes in manuscript:
Abstract: "with the remaining observation periods without clear desert dust intrusion"
Section 3.1: "which was approximately 2.7 times higher than the average concentration during the remaining observation period"
"The remaining observation periods are not assumed to be free of mineral dust influence; rather, no similarly clear desert dust intrusion signature was identified outside this event."
Section 3.2: "Taken together, these features indicate that episodic desert dust transport

was associated with the most prominent INP enhancements during this campaign, whereas the remaining variability during the non-desert-dust periods likely reflects a more complex mixture of sources and particle properties that cannot be uniquely resolved by the present dataset."

Section 3.4, title: The section has been revised from "Anthropogenic particle pollution affecting INP concentrations during non-dust periods" to "Anthropogenic particle pollution and INP variability during non-desert-dust periods" to avoid implying a direct influence in the section heading.

Section 3.4: "During the non-desert-dust periods, this factor is interpreted as mainly reflecting local dust-associated particles, such as road dust, fugitive dust from industrial and construction activities, and resuspended soil. This dust-related factor showed no significant correlation with N_{INP} abundance (Table 1)."

"These results suggest that, during non-desert-dust periods of this campaign, NINP variability was not clearly controlled by any of the major PMF-resolved source categories. This is consistent with previous observations at urban sites in different regions, where INPs remain scarce and episodic relative to total aerosol loading, and where their variability is often not well explained by the major anthropogenic aerosol components."

"The results provide observational constraints on wintertime INP abundance and variability in an industrial urban atmosphere."

"A key finding of this winter campaign is that the strongest INP enhancements in Taiyuan were associated with episodic natural desert dust transport."

"Overall, the observations from this winter campaign indicate that the strongest INP enhancements in Taiyuan were associated with long-range desert dust transport, whereas the remaining INP variability during the non-desert-dust periods reflected more complex influences that were not resolved by the major $PM_{2.5}$ source factors considered here. These results provide observational constraints for understanding wintertime urban INPs and for improving their representation in chemical transport and climate models."

2. The size range or cut-off size of the INP samples is not provided. This is very important as it is well-known that particle size is a key variable when assessing their ice-nucleating abilities. I only found the following information: "using a two-channel sampler without a cyclone". Does it mean that the samples correspond to Total suspended particles (TSP)? Did I miss something?

Response: The INP filter samples in this study were collected without a cyclone, impactor, or other size-selective inlet. Therefore, no defined aerodynamic cut-off was applied during sampling, and the collected aerosol can be regarded as approximately total suspended particles (TSP). We have clarified it explicitly in the revised manuscript.

Changes in manuscript:

Section 2.1: "In addition, INP filter samples were collected using a two-channel sampler equipped with two parallel filter channels from 4 December 2023 to 5 January 2024. Polycarbonate filters (47 mm, Nuclepore Track-Etch Membrane, 0.2 µm pore size, Whatman) were used for sampling. The sampler was operated without a size-selective inlet.”

3. The PMF analysis was performed on PM2.5 particles; however, as mentioned above in point #2, based on the provided information, it seems that the INP samples correspond to TSP. If this is the case, the performed correlations are meaningless.

Response: We agree that this comparison has a clear limitation, because the PMF analysis was based on PM2.5 chemical composition, whereas the INP filter samples are more appropriately regarded as approximately representative of TSP. Thus, the PMF-resolved factors do not capture the full particle size range contributing to the filter-based INP measurements. Our intention, however, was not to use PMF for direct source attribution of INPs, but to examine whether the dominant $PM_{2.5}$ source factors covaried with the observed N_{INP} variability during the non-desert-dust periods. We have clarified this limitation in the revised manuscript and revised the discussion accordingly, so that the PMF–N_{INP} relationships are presented more cautiously as covariation with major $PM_{2.5}$ source factors rather than direct source attribution for INPs.

Changes in manuscript:

Section 3.4: “To further examine whether the dominant $PM_{2.5}$ source factors were associated with N_{INP} variability during the non-desert-dust periods, PMF analysis was applied to the $PM_{2.5}$ chemical composition dataset, resolving five major factors...”

“To evaluate whether any of these major $PM_{2.5}$ sources covaried with INP, Pearson correlations…”

“All correlations were weak ($|r| < 0.3$) and statistically insignificant ($p > 0.05$), indicating that the dominant $PM_{2.5}$ source factors did not show clear covariation with INP abundance.”

4. I was surprised that meteorology was not used at all. The authors argue in several parts that the INPs were from local sources during the “non-dust period”. It would have been nice to check the wind speed and wind direction to corroborate this.

Response: We thank the reviewer for this helpful comment. We examined the wind data and added wind rose plots for the identified dust event and the non-desert-dust periods to the Supplement (Fig. S6). These plots show that both periods were dominated by northerly flow, while the dust-event period exhibited a more concentrated northerly flow pattern. We have added a brief statement in the main text to refer to this result as supplementary meteorological support for the event classification. At the same time, we avoid overinterpreting the wind data as definitive source-direction attribution for INPs.

Changes in manuscript:

Section 3.1: “Wind rose analysis (Fig. S6) shows that both the dust-event and non-desert-dust periods were dominated by northerly flow, whereas the dust-event period exhibited a more concentrated northerly flow pattern. This serves as supplementary meteorological support for the event classification.”

Supplement: Wind rose plots for the identified dust event and the non-desert-dust periods have been added to provide meteorological context for the event classification.

5. Figures S4 and S5 are not mentioned/discussed in the main text. These figures contain important information that deserves to be deeply discussed.

Response: In the revised manuscript, we have added explicit references to these figures in Section 3.4 and briefly discussed how they support the PMF factor assignments.

Changes in manuscript:

Section 3.4: An explicit reference to Figs. S4 and S5 has been added in Section 3.4 to show that the PMF factor profiles and inter-species correlation patterns provide supplementary support for the factor assignments.

"The corresponding factor profiles (Fig. S4) are consistent with the assigned source categories, including crustal tracers in the dust-related factor and Cu–Ba–K enrichment in the fireworks factor. The inter-species correlation patterns shown in Fig. S5 also support these factor assignments."

6. "All samples in Taiyuan activated before −15 °C, with the most active samples initiating freezing near –5°C" and "It is also noteworthy that at relatively warm temperatures (approximately T > −12 °C), ns values of the ambient desert dust samples substantially exceed the parameterizations for pure K-feldspar". If this is true, this corresponds to extremely efficient INPs; however, a deep discussion on the high efficiency of these particles is not provided. Is this because the samples were TSP? Is this coming from an instrumental bias? Were these samples enriched in biological particles?

Response: The revised manuscript expands the discussion to address the three possibilities raised by the reviewer. First, because the INP samples were collected without a size-selective inlet, the filter samples can be regarded as approximately TSP and may have retained coarse particles, including potentially biologically derived material. However, this sampling configuration was the same for all samples and therefore cannot by itself explain why the strongest warm-temperature activity was observed specifically in the dust-affected samples. Second, instrumental bias is unlikely to be the main explanation, because the warm-temperature enhancement was event-specific rather than a general feature of all samples measured with the same cold-stage-based approach. Third, enrichment of heat-sensitive, potentially biological components remains a plausible explanation. Previous heat-treatment results during East Asian dust events showed that heat-sensitive INPs contributed substantially at relatively warm temperatures (Chen et al., 2021), and Hu et al. (2023) reported similar behavior during spring dust transport in Beijing. We have therefore revised the discussion to state that this elevated warm-temperature activity may reflect the combined influence of transported mineral dust and potentially co-transported heat-sensitive components, although such a contribution can only be inferred and cannot be directly assessed in the absence of heat-treatment analysis in this study.

Section 3.2: "However, the enhanced warm-temperature INP activity during this episode may not be fully attributable to mineral dust alone. During East Asian dust events, Chen et al. (2021) showed that heat-sensitive INPs made substantial contributions at relatively warm temperatures. Similarly, Hu et al. (2023) reported enhanced warm-temperature INP activity during spring dust transport in Beijing and

suggested, based on heat-treatment analysis, that transported dust particles may be mixed with heat-sensitive, potentially biological components. Together, these studies support the possibility of similar heat-sensitive contributions during the Taiyuan dust episode. In the present study, however, no heat-treatment analysis was performed; therefore, such a contribution can only be inferred and cannot be directly assessed."
Section 3.3: "The TSP-like sampling configuration may have facilitated retention of such coarse ice-active components, but it cannot by itself explain the selective enhancement observed during the dust event."

7. Why the PMF analysis was correlated with the INP concentration at -20 °C only. I suggest testing this comparison at -10 °C and -15 °C as this refers to highly efficient INPs.

Response: Following the reviewer's suggestion, we extended the Pearson correlation analysis to N_{INP} at −10, −12.5, −15, and −17.5 °C, in addition to −20 °C. Values beyond the quantifiable range due to complete freezing before the corresponding temperature were excluded from the analysis. No statistically significant correlations were found between N_{INP} and any PMF-resolved factor at the examined temperatures ($p > 0.05$). These additional results are now provided in Table S2, and the main text has been revised accordingly.
Changes in manuscript:
Section 3.4: "Similar analyses were also conducted at −10, −12.5, −15, and −17.5 °C (Table S2). No statistically significant correlations were found between N_{INP} and any PMF-resolved factor at the examined temperatures ($p > 0.05$), indicating that this result was not specific to the choice of −20 °C."
Supplement, Table S2: Pearson correlation coefficients between daily mean PMF-resolved source contributions and N_{INP} at different temperatures during non-desert-dust periods were added.

Minor comments:
Lines 19-20: "compared with periods without natural dust influence". I don't think the authors can completely rule out the presence of mineral dust particles in the "non-dust period".
Response: We agree that the original wording in the Abstract was too strong, because it could imply a complete absence of mineral dust influence outside the identified dust event. We have therefore revised this phrase to a more cautious expression referring to the remaining observation periods without clear desert-dust intrusion.
Changes in manuscript:
Abstract: "compared with the remaining observation periods without clear desert-dust intrusion"

Lines 26-27: "Observed NINP variability is likely governed by the interplay of episodic coarse-mode inputs and atmospheric processing rather than a single dominant source." I am not sure the authors can really say this as a direct correlation between INP concentration and coarse particle concentration was not provided.

Response: We have revised this sentence in the Abstract to a more observation-based summary limited to this winter campaign.
Changes in manuscript:
Abstract: "During this winter campaign, N_{INP} variability in Taiyuan was characterized by strong enhancement during the identified desert dust event and weak associations with the major $PM_{2.5}$ source factors during the remaining observation periods."

Line 178: What was the main motivation to run the back-trajectories at 859 m a.s.l.?
Response: The arrival height of 850 m a.s.l. was chosen to approximately represent the actual sampling inlet height, considering that the ground elevation of the sampling site is about 800 m a.s.l. and the sampling inlet was located on the rooftop at about 20 m above ground level. We have clarified this in the revised manuscript.
Section 2.2.4: "Trajectories were calculated with a duration of 72 h and an interval of 6 h between each trajectory, arriving at an altitude of 850 m above sea level (a.s.l.), which approximately represents the actual sampling inlet height, considering a ground elevation of about 800 m a.s.l. and a rooftop sampling height of about 20 m above ground level."

Lines 249-250: "while it is comparable to observations from Sisal, Mexico (Ladino et al., 2016), and New Delhi, India (Wagh et al., 2021) between −15°C to −20°C". I disagree with this statement. It would have been better to add to Figure 2 in-situ data
Response: The New Delhi dataset has been removed from Fig. 2 because it was obtained in the deposition mode. We also removed the direct "comparable to Sisal and New Delhi" statement from Section 3.2. To provide a more relevant comparison for the present winter campaign, we added wintertime urban INP observations from Beijing (Chen et al., 2018) and revised the Tokyo comparison to use wintertime data from Tobo et al. (2020). The associated text has been softened to present these datasets as observational context rather than direct source attribution.
Changes in manuscript:
Section 3.2: Section 3.2 and Fig. 2 were revised by removing the New Delhi comparison, adding wintertime Beijing observations from Chen et al. (2018), using wintertime Tokyo observations from Tobo et al. (2020), and softening the comparison with previous studies.

Lines 252-253: "This implies that the INPs observed in Taiyuan may also be influenced by anthropogenic dust particles." No information is provided to support this.
Response: We thank the reviewer for this important comment. Our intention was not to infer anthropogenic dust influence in Taiyuan directly from Chen et al. (2024), but rather to note that anthropogenic dust has been reported as an important urban INP source in some environments. To avoid this ambiguity, we have removed the above sentence and revised the surrounding discussion. The revised text now states more cautiously that Chen et al. (2024) and the present study together suggest that the relative importance of anthropogenic dust and transported desert dust may vary among urban environments and observation periods. In the present winter campaign in Taiyuan, the

strongest INP enhancements were associated with the identified desert dust event, whereas the remaining variability during non-desert-dust periods could not be uniquely attributed to anthropogenic dust based on the present dataset.
Changes in manuscript:
Section 3.2: The sentence "This implies that the INPs observed in Taiyuan may also be influenced by anthropogenic dust particles" has been removed.

Line 256: "they are broadly comparable with those reported in other relevant studies." I disagree with this statement.
Response: We have removed this wording and retained only the more specific comparisons presented in the surrounding text.
Section 3.2: The sentence "Although the N_{INP} levels in Taiyuan are relatively high compared with other urban observations, they are broadly comparable with those reported in other relevant studies" has been removed.

Lines 257-258: I don't think that it is a good idea to add the data from Petters and Wright (2015). I suggest removing it from Figure 2.
Response: We have revised the text to clarify that this range is retained only as a broad background reference for atmospheric INP abundance, rather than as evidence that the Taiyuan measurements are directly comparable to precipitation samples. The corresponding discussion has been softened accordingly.
Changes in manuscript:
Section 3.2: "Additionally, the gray shaded area denotes the N_{INP} range derived from precipitation samples summarized by Petters and Wright (2015), which is used here as a broad reference for background atmospheric INP abundance. At temperatures warmer than −15 °C, the Taiyuan N_{INP} values fall mostly within this range during the non-desert-dust periods."

Lines 261-262: "suggesting additional contributions from ice-active components or enhanced influence of transported desert dust during certain periods." This is contradictory as this data corresponds to the "non-dust period".
Response: We agree that the original wording was contradictory, because this part of the discussion referred to the non-desert-dust periods and therefore should not imply enhanced influence of transported desert dust. Our intention was only to indicate that, at colder temperatures, the measured N_{INP} values exceeded the reference range, suggesting the presence of additional ice-active components active at lower temperatures. We have revised the sentence accordingly.
Changes in manuscript:
Section 3.2: "At colder temperatures, however, the measured N_{INP} values exceed this range substantially, suggesting contributions from additional ice-active components active at lower temperatures."

Lines 283-284: "particles larger than 0.5 μm in Taiyuan are likely dominated by local surface emissions from the surrounding Loess Plateau and fugitive dust from intensive industrial activities". No evidence is provided.

Response: Our intention was only to suggest that coarse particles present during non-desert-dust periods may differ in source and ice-nucleating efficiency from the transported desert dust represented by the D15 parameterization. We have therefore revised this sentence to a more cautious wording.

Changes in manuscript:

Section 3.3: "This discrepancy suggests that coarse-particle abundance alone may not fully constrain INP variability in Taiyuan. Particles larger than 0.5 μm in Taiyuan outside the identified desert dust event may differ in source characteristics and ice-nucleating efficiency from those of the highly active desert dust represented by the D15 parameterization. The apparent overestimation by D15 during pollution episodes implies that these coarse particles may possess lower ice-nucleating efficiency than the highly active desert dusts represented by the parameterization."

Lines 298-300: "This comparison suggests that, under the East Asian desert dust transport regime, typical atmospheric aging does not significantly modify the surface ice-nucleating activity of mineral dust, and that mineralogical composition and particle-size characteristics are likely more important determinants." I found this highly speculative.

Response: The original wording was too strong because the present study did not directly characterize the aging state of the transported dust particles or the specific atmospheric aging processes involved. We have revised this part to separate the observational comparison from any inference about atmospheric aging. The revised text now states that the n_s values of the Taiyuan dust samples broadly overlap with the Beijing dust parameterization, suggesting that strong ice-nucleating activity was retained during the Taiyuan dust episode. We also cite Chen et al. (2023), who reported observational evidence that atmospheric chemical modification did not suppress the ice nucleation activity of East Asian dust, as relevant context. However, this citation is not used to claim that the present dataset directly demonstrates an aging effect. We now explicitly state that the comparison in this study should not be interpreted as direct evidence for, or a quantification of, the effect of atmospheric aging.

Changes in manuscript:

Section 3.3: "As shown in Fig. 3, the Beijing dust parameterization broadly overlaps with the ns values of the dust samples in this study across the measured temperature range. This comparison is consistent with Chen et al. (2023), who reported observational evidence that atmospheric chemical modification did not suppress the ice nucleation activity of East Asian dust. However, because the present study did not directly characterize the aging state of the dust particles or the specific aging processes involved, this comparison is used only to suggest that strong ice-nucleating activity was retained during the Taiyuan dust episode, rather than to demonstrate or quantify the effect of atmospheric aging."

Lines 359-360: "provide a framework for evaluating the contributions of local anthropogenic emissions versus naturally transported particles." I don't think this was clearly demonstrated.

Response: We have revised this sentence to a more cautious description of the observational value of the present dataset.

Changes in manuscript:

Conclusions: "The results provide observational constraints on wintertime INP abundance and variability in an industrial urban atmosphere."

Lines 366-367: "suggesting that typical aging processes do not substantially suppress the INP efficiency of East Asian mineral dust." No evidence was provided for this.

Response: We have removed this statement and revised the text to a more cautious description based only on the observed strong ice-nucleating activity of the transported desert dust during the dust event.

Changes in manuscript:

Conclusions: "Comparison with previously reported dust parameterizations indicates that the transported desert dust retained strong ice-nucleating activity during the dust transport episode."

Line 375: "The findings show that long-range desert dust transport is the decisive driver of INP enhancements in Taiyuan" This is a very strong conclusion from a single dust event.

Response: We have revised this sentence to a more cautious formulation limited to the present observations.

Changes in manuscript:

Conclusions: "Overall, the observations from this winter campaign indicate that the strongest INP enhancements in Taiyuan were associated with long-range desert dust transport, whereas the remaining INP variability during the non-desert-dust periods reflected more complex influences that were not resolved by the major $PM_{2.5}$ source factors considered here. These results provide observational constraints for understanding wintertime urban INPs and for improving their representation in chemical transport and climate models."

Lines 378-379: "These observations help clarify the relative contributions of natural and anthropogenic sources to INPs". I am not fully convinced about this.

Response: We have revised this sentence to a more cautious statement emphasizing the observational constraints provided by the present study.

Changes in manuscript:

Conclusions: "These results provide observational constraints for understanding wintertime urban INPs and for improving their representation in chemical transport and climate models."

Section 3.3. I suggest adding a Table similar to Table S1 but for the dust period

Response: We considered adding a table analogous to Table S1 for the dust period. However, the identified desert dust event in this study was limited to a short time window (5–9 December), resulting in too few samples for a robust Pearson correlation analysis comparable to Table S1. We were concerned that a dust-period correlation table based on such a limited sample size would be statistically unstable and potentially overinterpreted. We therefore chose not to add a separate correlation table for the dust period. Instead, the dust event is characterized in the manuscript using its time evolution, backward trajectories, coarse-particle enhancement, PM_{10} increase, and the corresponding N_{INP} and ns behavior.

Figure 1. Panel a is not easy to follow. Perhaps using more contrasting colors could enhance its readability

Response: We revised the way special periods are indicated, replacing the original shaded highlighting with dashed boundary lines and colored arrows. This improves readability without obscuring the plotted data.

Changes in manuscript:

Figure 1:

The original shaded highlighting of special periods has been replaced by dashed boundary lines and colored arrows to improve readability and make the event periods easier to follow.

Figure 3. add ns values for literature studies on urban particles and dust

Response: We have revised Fig. 3 by adding n_s values for urban anthropogenic dust particles reported by Chen et al. (2024), in addition to the existing desert-dust and K-feldspar parameterizations and the Beijing dust-event observations. This added reference helps place the Taiyuan n_s values in the context of both transported desert dust and urban dust-related particles.

Changes in manuscript:

Figure 3 and its caption were revised to include literature n_s values for urban anthropogenic dust particles reported by Chen et al. (2024).

“The light-pink dashed line denotes the median trend of n_s values for urban anthropogenic dust particles in Beijing, digitized from Chen et al. (2024).”

Figure 4. This data adds little to the discussion.

Response: We have revised the accompanying text to clarify that Fig. 4 provides the temporal context for the PMF–N_{INP} correlation analysis by showing the daily contributions of the major $PM_{2.5}$ source factors during the non-desert-dust periods. This helps interpret the subsequent correlation results, because secondary aerosols and coal/traffic emissions dominated the $PM_{2.5}$ mass during much of the non-desert-dust period but did not show significant covariation with N_{INP}. We therefore retained Fig. 4 in the main text but revised the discussion and caption to make its purpose clearer.

Changes in manuscript:

Section 3.4: “The reconstructed daily $PM_{2.5}$ contributions (Fig. 4) provide the temporal context for the subsequent PMF–N_{INP} correlation analysis, showing that secondary aerosols and coal/traffic emissions dominated the $PM_{2.5}$ mass during much of the non-desert-dust period, whereas fireworks emissions became prominent during the New Year period.”

Figure 4 caption: "Daily $PM_{2.5}$ source contributions resolved using the PMF model, shown together with the corresponding N_{INP} at −20 °C during the non-desert-dust periods. Data from the identified desert dust event are not shown."

Technical comments:

Line 15: Replace "campaign in Taiyuan" by "campaign in Taiyuan (China)"

Response: Revised as suggested.

Line 69: Replace "dust in cities" by "dust in urban environments"

Response: Revised as suggested.

Line 71: "urban and peri-urban regions (Chen et al., 2024)". I suggest adding other references.

Response: After rechecking the relevant literature, we found that direct field evidence for anthropogenic dust as an atmospheric INP source in urban or peri-urban environments remains limited. Therefore, instead of adding less directly relevant references, we revised the sentence to make this limitation explicit and retained Chen et al. (2024) as the most directly relevant reference.

Changes in manuscript:

Introduction: "Furthermore, the influence of dust in urban environments is not limited to long-range natural transport; anthropogenic dust may also contribute to atmospheric INPs in some urban and peri-urban regions, although direct field evidence remains limited (Chen et al., 2024)."

Line 35: "thermal activation behavior" What does it mean?

Response: We agree that the phrase "thermal activation behavior" was unclear, and we have revised it to a more specific wording.

Changes in manuscript:

"thermal activation behavior" has been revised to "temperature dependence of INP activation".

References

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Chen, J., Wu, Z., Chen, J., Reicher, N., Fang, X., Rudich, Y., and Hu, M.: Size-resolved atmospheric ice-nucleating particles during East Asian dust events, Atmospheric Chemistry and Physics, 21, 3491–3506, https://doi.org/10.5194/acp-21-3491-2021, 2021.

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