Response to Referee Comments on the Study of **"A satellite observation-based analysis of cirrus ice crystal number concentrations and underlying cirrus formation mechanisms"**

We sincerely thank the referee for their valuable comments and suggestions, which have greatly helped improve the quality of this manuscript. We have carefully addressed each comment and revised the manuscript accordingly, as detailed below:

Comments from Referee 1

The paper is still not ready for publication.

Here is the list of required improvements:

1、Section 2.1

Page 5, line 25: The retrieval product ʻICNC for sizes > 5 µmʼ is discussed, only! However, the authors pointed out that mostly observations of aged cirrus layers are in the used data base. That means that the solutions for ICNC (assuming crystal sizes >25 µm) are more appropriate than the solutions discussed. The 5 µm ICNC values are a factor of 2-3 larger than the 25 µm ICNC values. So, it is misleading to discuss exclusively the 5 µm ICNC values. Both ICNC numbers have to be mentioned and discussed. They better indicate the range of true ICNC levels.

Respone:We thank the reviewer for this helpful suggestion. Following the reviewer's comment, additional descriptions and discussions of N_i (>25 µm) have been included in Section 2.1 of the revised manuscript.The results show that while N_i (>5 µm) values are approximately 2-3 times higher than N_i (>25 µm), the spatial distribution patterns remain largely consistent. These additions help to better indicate the plausible range of ICNC levels and clarify the relationship between the two particle size thresholds.

In addition to Ni (>5 µm), Ni (>25 µm) was also analyzed to evaluate the sensitivity of the results to the particle size threshold. Given that the DARDAR-Nice dataset predominantly samples mature or aged cirrus layers, larger ice crystals (>25 µm) may better represent the evolved stage of cloud microphysical development. Therefore, examining Ni (>25 µm) provides complementary information and helps indicate the plausible range of Ni values.

The spatial distribution and vertical structure derived from Ni (>5 µm) and Ni (>25 µm) are largely consistent, indicating that the main structural features of cirrus over the Tibetan Plateau are robust across size thresholds. The primary difference lies in the absolute magnitude: Ni (>5 µm) values are systematically higher by approximately a factor of 2–3 compared to Ni (>25 µm), reflecting the contribution of smaller ice crystals to the total population. Since Ni (>5 µm) captures a broader fraction of the total ice crystal population, it is retained as the primary variable in this study. The corresponding Ni (>25 µm) results are presented in the supplementary_material to illustrate the range of Ni values and to demonstrate that the main conclusions are not sensitive to the selected particle size threshold.

2、Page 6, line 30, page 7, line 7: please use ‘meteorological conditions’ instead of ‘atmospheric conditions’. Atmospheric conditions include even chemical composition information. You probably mean ‘physical state of the atmosphere’.

Respone：We thank the reviewer for this suggestion. The term “atmospheric conditions” has been revised to “meteorological conditions” at the indicated locations in the manuscript.

To investigate meteorological conditions for the satellite observations, this study utilizes ERA5 reanalysis data from the European Centre for Medium-Range Weather Forecasts (ECMWF).

3、Page 6, line 21: … fall below -45° C. …. is better.

Respne：We thank the reviewer for the suggestion. The wording has been revised accordingly in the manuscript.

In addition, recent observational analyses by Mamouri et al. (2023) and Ansmann et al. (2025) suggest that smoke aerosols can exert a substantial influence on ice crystal formation at altitudes while temperatures fall below −45 ° C.

4、Section 2.3 Method:

It does not help when you explain the nice data processing scheme in the reply letter, ONLY! …. and avoid the presentation in the manuscript.

So, my request is: Please include and discuss this data processing scheme, shown in the reply letter, in this section 2.3! Both sketches should be presented and discussed.

Alternatively, you may want to present this scheme in the Appendix. But for clarity and transparency, this scheme needs to be presented and explained!

Respone:We thank the reviewer for this valuable suggestion. The data processing scheme has now been included in the Supplementary Material, and additional descriptions referring to these schematics have been added in Section 2.3 of the revised manuscript to improve the transparency of the methodology.

The overall data processing workflow adopted in this study is illustrated in Fig. S1 and Fig. S2. Fig. S1 outlines the procedure used to derive Ni statistics, including calculations in both the horizontal and vertical directions. Fig. S2 illustrates how aerosol classification data from CALIPSO are combined with cirrus cloud properties retrieved from the DARDAR-Nice product. These schematics provide a transparent overview of the integration and processing of the various satellite datasets used in this study.

5、Page 9, lines 11-14: Please remove this sentence starting with … Moreover … Such a sentence is confusing and simply not needed.

Respone:We thank the reviewer for this helpful suggestion. The sentence starting with “Moreover …” has been removed from the revised manuscript as suggested.

6、Page 9, line 21: ‘clean aerosol’ accounts for 1% of occurrences! Is that statement related to the column aerosol data (taken from the CALIPSO data base)? Or is this statement of 1% describing the aerosol conditions in the upper troposphere? Please state that clearly.

Respone:We thank the reviewer for the comment. The text has been revised to clarify that the 1% occurrence of “clean aerosol” refers to the column-level aerosol classification from the CALIPSO aerosol product.

Kim et al. (2018) performed a statistical analysis of different aerosol types in this product *and found that 'clean' aerosols account for only about 1% of occurrences in the CALIPSO column-level aerosol product, representing background aerosol with very low concentration, which further supports the validity of this assumption.*

7、Section 3.1 Distribution characteristics …

Page 10, line 15-28: Again! We simply do not know whether the 5 µm ICNC values or the 25 µm ICNC values better represent the real world cirrus ICNC levels. Therefore, it is necessary to discuss and mention both numbers.

Respone:We thank the reviewer for this suggestion. In the revised manuscript, additional discussions of *Ni* (>25 µm) have been included in Section 3.1, and both *Ni* (>5 µm) and *Ni* (>25 µm) values are now reported for comparison.

Based on the DARDAR-Nice PRO product, this study analyzes the spatial variation of Ni across all layers where the temperature is below -30°C during the study period. The horizontal distribution of Ni (>5 µm) in Fig. 1 demonstrates that the average concentration over the TP is 187 L-1 during the study period. The corresponding average concentration for Ni (>25 µm) is 87 L-1, which is less than half of Ni (>5 µm) (Fig. S2). The average Ni (>5 µm) reported here is higher than the approximately 150 L-1 over the TP reported by Gryspeerdt et al. (2018), who used DARDAR-Nice data from 2006 to 2013 to study global Ni (>5 µm) but focused only on cloud-top statistics. Considering that our analysis includes all layers below -30°C, the higher Ni (>5 µm) is reasonable and consistent with physical expectations, which also indirectly supports the reliability of our results. The average concentration in the south (24-30°N, 66-106°E) is significantly higher than other areas, reaching 213 L-1, with the maximum value located in the north-central region of India (24-26°N, 78-80°E), reaching 253 L-1. Over the north, including the Xinjiang, Inner Mongolia, the north of the Qilian Mountains and the Kunlun Mountains, Ni is only 143 L-1, only two-thirds of Ni compared with that in the southern region. For Ni (>25 µm), the corresponding average concentrations are 94 L-1 in the southern region, 112 L-1 in the north-central region of India, and 70 L-1 in the northern areas. Compared with Ni (>5 µm), the spatial distribution remains generally consistent, although the absolute concentrations are systematically lower.

8、Section 3.2.1 Contribution of the homogeneous nucleation

Page 14, line 18, and later, pages 15 and 16, including Figure 4: You compare ICNC values from satellite observations and homogeneous nucleation. That sounds like comparison of apples and oranges. Obviously, you compare ICNC observations of heterogeneous AND homogeneous ice nucleation with observations of pure homogeneous ice nucleation. Please improve the notation in the discussion and in the the figure, be more precise in wording here.

Respone：We thank the reviewer for this valuable comment. The wording and notation in the relevant sections and in Fig. 4 have been revised to improve clarity and to more clearly distinguish between Ni from all satellite observations and Ni under clean aerosol conditions.

Ni for each vertical layer is calculated using Eq. (2), and Fig. 4 depicts the vertical distribution of the Ni for all satellite-retrieved cases ('all') and for clean aerosol conditions ('clean'). Here, 'all' refers to Ni retrieved from all available satellite observations regardless of aerosol type, whereas 'clean' refers to cases where CALIOP does not indicate the presence of dust or smoke and the aerosol type is classified as 'clean'. Under clean conditions, ice formation is interpreted as being more likely influenced by homogeneous freezing. However, in this situation, purely *homogeneous nucleation cannot be assumed, and uncertainties remain regarding the relative contributions of different ice formation pathways.*

9、Page 15, line 14: …homogeneous nucleation is expected to be a major contributor …

Respone: We thank the reviewer for the suggestion. The wording has been revised accordingly in the manuscript.

Although homogeneous freezing may represent an important contributor under sufficiently cold and supersaturated conditions. (Cantrell and Heymsfield, 2005), heterogeneous nucleation has lower activation requirements and may occur earlier, potentially consuming water vapor and influencing subsequent ice formation.

10、Page 19, lines 7-22, and Fig 7: Again, it is confusing when you discuss ICNC values at different heights, and the non-dust scenarios lead to higher ICNC values than the dust scenarios. The confusing aspect is that you link (or combine) ICNC data in the upper troposphere with aerosol column information (with the main aerosol contribution from the lower troposphere). One has to clearly state and repeat this statement several times in the discussion to avoid confusion and misunderstanding.

Respone:We thank the reviewer for this comment. Additional explanations have been added in the relevant sections of the revised manuscript to clarify that the aerosol information represents column-level aerosol occurrence within each grid cell rather than vertically co-located aerosol-cloud interactions. This point is now explicitly emphasized to avoid potential confusion.

It should be emphasized that the aerosol information used in this study represents column-level aerosol classification within each grid cell, rather than vertically co-located aerosol-cloud interactions. Dust aerosols are predominantly distributed in the lower troposphere. Therefore, the comparison between upper-tropospheric Ni and aerosol occurrence reflects statistical associations at the grid-cell scale, which may be influenced by vertical transport and large-scale dynamical processes, rather than direct, instantaneous interactions at the same altitude.

11、Fig. 7: Question: The non-dust cases include even the homogeneous freezing cases (i.e., the ‘clean aerosol’ scenarios)? or are the clean aerosol cases excluded….?

Fig. 8: Same question. Does the non-smoke class include ‘clean aerosol’ cases (and thus homogeneous freezing events) or not?

Respone:We thank the reviewer for this helpful question. To clarify, additional explanations have been added to the figure captions of Fig. 7 and Fig. 8 in the revised manuscript. Specifically, the definitions of the non-dust and non-smoke categories are now more clearly explained.

Fig. 7. (a) Horizontal distribution of ICIC(dust) and (b) the vertical profile of the Ni affected by dust and non-dust events. Here, 'non-dust' refers to all cases in which CALIPSO does not detect dust aerosols, including clean aerosol conditions.

Fig. 8. (a) Horizontal distribution of ICIC(smoke) and (b) the vertical profile of the Ni affected by smoke and non-smoke events. Here, 'non-smoke' refers to all cases in which CALIPSO does not detect smoke aerosols, including clean aerosol conditions.

Comments from Referee2

I thank the authors for responding to my comments and for their substantial revisions following the input of three referees. I now find the manuscript much clearer and more balanced in its conclusions. While some interpretations could still be discussed, I consider that uncertainties and limitations are now sufficiently addressed. I therefore recommend publication after minor revisions.

Specific comments:

1、 The title has been modified following referee #1's recommendation. I agree that the new title is clearer, but it remains important to keep the study and its conclusions within the context of a regional analysis. I therefore strongly suggest adding "over the Tibetan Plateau" at the end of the current title.

Respone： We thank the reviewer for this valuable suggestion. We agree that adding "over the Tibetan Plateau" enhances the clarity of the title. Therefore, the title has been updated accordingly to include "over the Tibetan Plateau" at the end.

A satellite observation-based analysis of cirrus ice crystal number concentrations and underlying cirrus formation mechanisms over the Tibetan Plateau

2、 I appreciate the effort to disentangle the sensitivity of the ICIC variable to IWC in order to limit its influence on the Ni ~ ICIC sensitivity analysis. However, the computation of the partial correlation coefficient is not clearly described and would benefit from additional details. Initially, I thought the sensitivity had been computed within IWC bins, which does not seem to be the case. In its current form, I am not convinced that the concerns raised by Fig. 3a (showing a dominant sensitivity to IWC) are fully resolved. Please clarify this analysis.

Respone:We thank the reviewer for this constructive comment. In response, additional details have been added to the manuscript to clarify the computation of the partial correlation coefficient between ICIC and Ni.

This formulation aims to reduce the confounding influence of IWC on the apparent Ni-INPs relationship. To further demonstrate the robustness of this normalization, we compute the partial correlation between INPs and Ni after removing the effect of IWC. Specifically, the partial correlation coefficient is calculated by statistically removing the linear effect of IWC from both Ni and ICIC across all grid cells and then computing the correlation between the residuals. This approach does not eliminate the physical influence of IWC on Ni, but allows an evaluation of the Ni-INPs relationship independent of the first-order linear contribution of IWC. The resulting partial correlation coefficient ($r = -0.38$) indicates that a relationship between ICIC and Ni remains after accounting for IWC, although IWC itself continues to exert a strong influence.

3、 In Fig. 4 and the related discussion, the labels "observations" and "homogeneous nucleation" are potentially confusing. If I understand correctly, "observations" refers to all satellite-retrieved Ni values, whereas "homogeneous nucleation" refers only to retrievals under clean conditions (no dust or smoke). It would be clearer to use labels such as "all" and "clean" in the figure, and leave the interpretation to the text. Also note that the absence of dust detected by CALIOP does not imply purely homogeneous nucleation. Homogeneous may be dominant, but uncertainties remain - that should be reminded.

Respone： We thank the reviewer for this valuable comment. Following the reviewer's suggestion, we have revised the labels in Fig. 4 to use "all" and "clean" instead of "observations" and "homogeneous nucleation" for clarity. In the revised manuscript, "all" refers to all satellite-retrieved Ni values, while "clean" refers to retrievals under conditions where no dust or smoke are detected by CALIPSO. This distinction is now made more clearly in both the figure and the text.

Additionally, we have added a reminder in the manuscript to clarify that the absence of dust detected by CALIPSO does not imply purely homogeneous nucleation. Instead, homogeneous nucleation may dominate under clean conditions, but uncertainties remain regarding the relative contributions of different nucleation mechanisms, and this has been explicitly stated in the revised manuscript.

Ni for each vertical layer is calculated using Eq. (2), and Fig. 4 depicts the vertical distribution of the Ni for all satellite-retrieved cases ('all') and for clean aerosol conditions ('clean'). Here, 'all' refers to Ni retrieved from all available satellite observations regardless of aerosol type, whereas 'clean' refers to cases where CALIOP does not indicate the presence of dust or smoke and the aerosol type is classified as 'clean'. Under clean conditions, ice formation is interpreted as being more likely influenced by homogeneous freezing. However, in this situation, purely homogeneous nucleation cannot be assumed, and uncertainties remain regarding the relative contributions of different ice formation pathways.

Technical comments:

4、 Page 1, line 29: "characteristic" is unclear; please remove it. It is also not part of the main conclusions, and I am unsure why it is highlighted in the abstract. Similarly, I suggest removing "which is dominated by homogeneous freezing," since the same shape is found with and without aerosols.

Respone： We thank the reviewer for pointing this out. The sentence has been removed from the revised manuscript as suggested.

5、 Page 4, line 10: use "contributing to Ni" instead of "determining."

Respone： We thank the reviewer for this helpful suggestion. The wording has been revised accordingly in the manuscript, and "determining" has been changed to "contributing to Ni" as recommended.

It is generally recognized that homogeneous nucleation is the dominant mechanism contributing to Ni (Cantrell and Heymsfield, 2005).

6、 Page 3, line 17: It remains unclear why "deep convective outflows" are presented at the same level as homogeneous and heterogeneous nucleation mechanisms. Please justify this more clearly or remove it.

Respone： We appreciate the reviewer's comment. In response, the discussion regarding "deep convective outflows" has been removed from the revised manuscript to eliminate potential

confusion and maintain a more focused analysis on the nucleation mechanisms.

7、 Page 9, lines 11-14: This sentence is confusing. What is meant by “purely homogeneous freezing”? Homogeneous freezing from water vapour does not occur, whereas it is possible from supercooled droplets. I suggest removing this sentence for clarity.

Respone： We appreciate the reviewer’ s question and the opportunity to clarify this point. In response, the sentence has been removed from the revised manuscript.

8、 Table 1: Use “Period” instead of “Duration.”

Respone：We thank the reviewer for this suggestion. The term “Duration” in Table 1 has been changed to “Period” as recommended.