Response to Reviewer #4

General Comments:

The manuscript by Shen et al. presents an observational study of cirrus cloud formation near Midway Island in Pacific Ocean and the ice-nucleation properties of long-range transported desert dust. Authors have used observational data from Cloud-Aerosol Lidar with Orthogonal Polarization (CALIOP) and Cloud Profiling Radar (CPR), DARDAR and MERRA-2 datasets, and POLIPHON and HYSPLIT models for the data evaluation. As is, the manuscript addresses an important topic (especially for atmospheric and climate modeling communities) – the impact of long-range transport dust on the formation of cirrus clouds – but does not present an answer. Before the paper is published, some major concerns would have to be addressed.

My greatest concern with the manuscript is the lack of in-depth analysis – additional details (such as the nucleation mechanism, connection between the initial nucleation conditions and lab experiments) would improve the manuscript significantly. For this reason, these Major issues in the manuscript should be addressed.

Response: We sincerely appreciate the valuable feedback provided by the reviewer regarding our manuscript. Every comment has been thoughtfully taken into consideration, and necessary revisions have been made in the updated version. We consider that this revised manuscript has been largely improved. The main revisions are listed below:

(1) As strongly suggested by reviewer #2, here we decide to combine Part A and Part B and conduct the analysis by considering them as a whole, due to the uncertainty of DARDAR retrievals. The specific discussions have been largely updated now.

(2) As at least three reviewers suggest shifting this manuscript from 'Measurement Report' to 'Research Article', we would like to do so but also need to involve Handling Editor Prof. Krämer in the decision. In addition, considering the results have been largely rewritten by adding in-depth discussions, we also think it would be better to change it to 'Research Article'.

(3) As the cirrus clouds in the two cases have already formed for at least half an hour (as deduced from the vertical extent of the ice virga by assuming a falling velocity of 1 m/s), nucleated ice crystals may have undergone significant growth. Therefore, in the comparison between ICNC and INPC, we decide to mainly use the values of $n_{ice,25um}$ and $n_{ice,100um}$. The specific reason has been discussed in the text of the revised manuscript.

(4) As there is no evident indication of the depletion of dust INP, the possibility for occurring homogeneous nucleation is low. Therefore, we have removed all the discussions about the involvement of homogeneous nucleation in the cirrus clouds.

Major issues:

1. The idea and dataset chosen to show the nucleation and cloud formation are great, but the data analysis seems to be focused on an observation. It would be very interesting to see the atmospheric conditions data and the discussion of the initial heterogeneous ice nucleation (e.g., the transition from pure dust to pure cloud at 30.34/0.0 lat/lon on 2010-05-05 trajectory) to see the relation to the laboratory experiments (such as Koehler et al., Atmos. Chem. Phys., 10, 11955–11968, 2010).

Response: We are grateful for the reviewer's suggestion. Yes, as suggested, it would be better if we can connect the information from actual observations with that from the laboratory experiments. We have tried to do this in the revised manuscript as can be seen in the case study '**… In the case of polluted/coated dust, the ice-nucleating efficiency may possibly reduce to 10-20% of that for pure dust (Augustin-Bauditz et al., 2014; Wex et al., 2014); however, note that the coating-induced reduction in dust ice-nucleating efficiency is still not well-known quantificationally by the community. In addition, higher relative humidity is generally required for polluted/coated dust conditions (Koehler et al., 2010); Kärcher et al. (2022) used S_i of around 1.35 for the activation threshold for polluted dust. Thus, if multiplying by a fact of 0.1, we can obtain a modified U17-D-derived INPCs of 310.1 L^{-1}; due to the quick growth of ice**

crystals during their fall, we also reasonably use $n_{ice,25\ \mu m}$ and $n_{ice,100\ \mu m}$ for the comparison.' (Please see lines 312-318)

References:

Koehler, K. A., Kreidenweis, S. M., DeMott, P. J., Petters, M. D., Prenni, A. J., and Möhler, O.: Laboratory investigations of the impact of mineral dust aerosol on cold cloud formation, Atmos. Chem. Phys., 10, 11955–11968, https://doi.org/10.5194/acp-10-11955-2010, 2010.

Augustin-Bauditz, S., Wex, H., Kanter, S., Ebert, M., Niedermeier, D., Stolz, F., Prager, A., and Stratmann, F.: The immersion mode ice nucleation behavior of mineral dusts: A comparison of different pure and surface modified dusts, Geophys. Res. Lett., 41, 7375–7382, https://doi.org/10.1002/2014GL061317, 2014.

Wex, H., DeMott, P. J., Tobo, Y., Hartmann, S., Rösch, M., Clauss, T., Tomsche, L., Niedermeier, D., and Stratmann, F.: Kaolinite particles as ice nuclei: learning from the use of different kaolinite samples and different coatings, Atmos. Chem. Phys., 14, 5529–5546, https://doi.org/10.5194/acp-14-5529-2014, 2014.

Kärcher, B., DeMott, P. J., Jensen, E. J., and Harrington, J. Y.: Studies on the competition between homogeneous and heterogeneous ice nucleation in cirrus formation, J. Geophys. Res.-Atmos., 127, e2021JD035805, https://doi.org/10.1029/2021JD035805, 2022.

2. The manuscript is presented as a Measurement Report, however it is not a new or original measurement, but rather a reanalysis of an old and public dataset. As a result, the manuscript does not provide substantial insight and conclusions. I would suggest the authors to perform and write-up an in-depth analysis of the processes based on their expertise and reconsider the manuscript as a research article.

Response: Thank you for the suggestion. Taking the reviewer's suggestion, we have removed 'Measurement report' in the title. Here we would also like to involve our Handling Editor Prof. Krämer to judge if it is justified to shift this manuscript from 'Measurement Report' to 'Research Article.' As the significant modifications have been made according to all the four reviewers' valuable comments, we hope that this manuscript may fulfill the requirements of a research article.

3. Another major shortcoming of the article is the Results section, where most of the section is focused on spelling out the results seen in figures with little interpretation. It is good to see that the summary of the results has been provided in Table 2, in the Discussion section, but I would expect it as part of Results.

Response: We have moved Table 2 to the results section and added a new subsection 3.3 to summarize the observational results of the two cases. Also, a new paragraph has been added in subsection 3.3. (Please see lines 337-352)

For these reasons, in my view, this work is not yet sufficient for publication and I would reconsider the manuscript after major revisions.

Response: After addressing the comments from all four reviewers, we have significantly modified the manuscript and hope that the updated work can be reconsidered by this reviewer.

Minor issues:

4. In Abstract, lines 24 and 25, the text says '[...] nucleation is dominated [...]' and '[...] nucleation can still be dominated [...]' while it should be 'dominant'.

Response: The word 'dominated' has been replaced with 'dominant'.

5. Line 70, 'concerned' should be 'considered'?

Response: The word 'concerned' has been replaced with 'considered'.

6. Mistypes and inconsistent labeling of the instrument and datasets: sections 2.1: MEERA-2; 2.5: HYSPIT.

Response: The title of section 2 has been updated to 'Instruments, Datasets, and Models'.

7. Missing satellite track, it should be added to the lon/lat maps. Vertical profile figures should have double latitude + longitude axis on abscissa (same as provided by CALIPSO).
Response: The figure 3, 4, 5, 7, 8, 9 have been updated in the revised manuscript.

8. The results section is riddled with 'Figure <n> shows [...]' sentences. I would suggest rephrasing them to 'As seen in Fig. <n>[...]', 'Based on data shown in Fig. <n>[...]', etc.
Response: We have revised the relevant sentences, reducing the use of 'Figure <n> shows' and employing alternative phrases.

9. Throughout the manuscript there are numerous grammatical errors (similar to the ones pointed out above) and it should be very carefully revised.
Response: The revised manuscript has been thoroughly checked to ensure there are no grammatical errors. Thank you for bringing this to our attention**.**

10. The authors have a significant number of self-citations that are present in addition to other previous works, e.g. (He et al., 2021b, 2022b), (Jing et al. 2023). I would suggest removing them where appropriate.
Response: We have reduced the occurrence of such self-citations.

11. The references (He et al., 2021a) and (He et al., 2022c) are not mentioned in the text at all. Please make sure that all unused references are removed.
Response: Thank you for pointing it out. They have been removed now.

12. The abstract provides a good summary, but is quite verbose and lengthy. If it is possible to shorten it without affecting quality, it would be excellent.
Response: We have rewritten the abstract according to the modifications in this round.

13. In Figure 8, the dust mass column density is for 2010-05-05, but the HYSPLIT trajectories span from 2008-04-23 to 2008-04-28. Is it a mistake in the caption, or was wrong dataset used for the figure?
Response: Thank you for pointing out this mistake. The correct date in Figure 8 should be 2008-04-27.