

Review of 'Measurement report: Influence of long-range transported dust on cirrus cloud formation over remote ocean: Case studies near Midway Island, Pacific'

Principal criteria:

Scientific significance: Fair (2)

Scientific quality: Good (3)

Presentation quality: Fair (2)

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 indepth 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:

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).
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.
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.

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

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'.
5. Line 70, 'concerned' should be 'considered'?
6. Mistypes and inconsistent labeling of the instrument and datasets: sections 2.1: MEERA-2; 2.5: HYSPLIT.
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).
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.
9. Throughout the manuscript there are numerous grammatical errors (similar to the ones pointed out above) and it should be very carefully revised.

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.
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.
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.
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?