Review of: "Turbulence structure of dusty cirrus and its relation to atmospheric conditions: A characterization using cloud-radar measurements and reanalysis data" by Pieper et al.

Content

This study provides the first longer-term, observation-based characterization of atmospheric conditions associated with Saharan dust-induced cirrus clouds (“dusty cirrus”) over central Europe. Using continuous cloud radar measurements and dust reanalysis data, the authors found a strong relationship between elevated dust concentrations and enhanced turbulence, with increased vertical updrafts potentially promoting heterogeneous ice nucleation. The results suggest that wind shear plays only a minor role and that current theories may not fully explain the processes governing dusty cirrus formation.

Overall impression and rating

The manuscript addresses an important and timely question regarding the role of turbulence in the formation of Saharan dusty cirrus and presents a novel analysis. I think it is an important contribution to the science community. The methodology is generally sound, Figures are clear, the text is easy to understand and well written, and the study provides valuable observational evidence that challenges some aspects of current conceptual models of dusty cirrus formation. I therefore recommend publication after minor revision, focusing primarily on the points listed below.

Main comments:

- My main concern is an independence problem with dust versus meteorology. In the manuscript it was frequently interpreted that high dust is directly connected to high turbulence. However, dust events and turbulence may both be consequences of the same large-scale dynamical situation. For example: warm conveyor belts, DIBS systems, frontal ascent, upper-level jets, gravity-wave activity. In other words, dust may be a tracer of dynamically active air masses rather than the cause of enhanced turbulence. So the dynamic situation could simply lead to more turbulence, entirely without the influence of mineral dust. The paper acknowledges this possibility in places, but I do not think it is fully resolved and should be better treated in the discussion section.

Further Specific comments/questions:

- Introduction: The introduction and the rest of the text do not discuss in-situ observations of dusty cirrus clouds in much detail, even though there are already several studies (e.g., Froyd et al. 2022, Juurikkala et al. 2026, Sakai et al. 2014) that I would recommend citing.
- Is there a specific reason why the year 2022 was chosen for the analysis? If so, it might make sense to mention this in the text.
- Page 4, line 120: Up to what altitude would the aerosol be integrated? Maybe you could mention that here in the text as well. I guess it's only important up to the tropopause.
- Page 7, lines 159-160: Where is it shown that temporal resolution does not play a significant role for EDR? Perhaps a paper could be cited here, or a figure in the appendix?
- Figure 1: In the text it is written on page 7, line 160 "The categorization product contains variables from different measurements (lidar, radar, microwave radiometer)." This is not explicitly shown in Figure 1, which mentions only the Cloud Radar. I therefore recommend listing all Cloudnet instruments used here in the figure as well.
- page 9/10, lines 240 and 248: The first section states that EDR retrieval always requires 5-minute time intervals, while the second section states that EDR is then calculated at a time resolution of 10 seconds. How do these two statements fit together? Or is it the case that, within the 5-minute time interval, the values are determined every 100 seconds when the slope in the power spectrum is met? This isn't entirely clear from the text.
- Figure 5 and 7: The relative humidity with respect to ice in Figure 5 is calculated using ECMWF data, and the relative humidity with respect to water in the Hysplit trajectories comes from GFS data. Since the manuscript deals with the formation of ice, only relative humidity with respect to ice should be used. This is the only way to compare the two figures and relative humidity values with one another. I would suggest consistently using relative humidity with respect to ice in Figure 7.
- Page 22, lines 450-465: I also think that heterogeneous freezing is the most likely pathway for ice nucleation in these cases. I just don't think the calculation example is very well done. The lifting of air occurs both through large-scale updrafts and through small-scale fluctuations, as described in Podglajen et al., 2016. However, the discussion only refers to the small-scale fluctuations. The trajectories in both cases exhibit, however, an ascent of more than 300 m in time shortly before the end and thus close to the observation (Figure 7 and 10), which could be attributed to a large-scale updraft, since small-scale fluctuations are typically not included in GFS/ECMWF models. Therefore, the line of arguments is only partially correct and should be revised accordingly.
- Page 27, lines 527-537: You write here that only small-scale uplift can account for the ice formation. However, that may not be the case. Large-scale uplift is not examined at all in the entire manuscript. One should at least be able to easily

demonstrate, using the trajectories, that no uplift occurred in the hours leading up to the observation. At the very least, the trajectories from the first two case studies show uplift in the hour before the observation. That could very well be the reason for the heterogeneous ice formation and the resulting release of latent heat, which in turn causes turbulence. Actually, this should be demonstrated in the manuscript so that the claim in that section can truly be substantiated.

Technical comments/questions:

- Page 2, line 40, 43 and other places: There's a strange symbol between the numbers e.g. 100 and 120 at least in my PDF Version; please change it to a hyphen.
- Page 5, line 164, 171: The verb "categorize" is often used in the text, even though it should actually be a noun. For example, "the categorize product." I would therefore suggest replacing it with the noun, i.e., "the categorization product."
- Page 10, line 247: Please change to " effects **on** cirrus cloud".
- Page 12, line 314: Please change "typically"
- Page 12, line 318: Please change to "Comparable".
- Page 12, line 318: Missing reference: citet{turbulence_anvil_cirrus}
- Page 15, line 351: Please change to "direction".
- Page 27, line 555: Please change to "agreement"
- Page 31, line 677: Please change to "contain all trajectories"

References:

- Froyd, K.D., Yu, P., Schill, G.P. et al. Dominant role of mineral dust in cirrus cloud formation revealed by global-scale measurements. Nat. Geosci. 15, 177–183 (2022). https://doi.org/10.1038/s41561-022-00901-w
- Juurikkala, K., Raatikainen, T., Krämer, M., Williamson, C. J., and Laaksonen, A.: Mineral dust concentration controlling convective cirrus structure and persistence: a large-eddy simulation study with observational constraints, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2026-2427, 2026.
- Sakai, T., N. Orikasa, T. Nagai, M. Murakami, T. Tajiri, A. Saito, K. Yamashita, and A. Hashimoto (2014), Balloon-borne and Raman lidar observations of Asian dust and cirrus cloud properties over Tsukuba, Japan, J. Geophys. Res. Atmos., 119, 3295–3308, doi:10.1002/2013JD020987.