Jet Superposition and a Cut-off Low Behind a Rare Heavy Hailfall Episode in the United Arab Emirates (10–12 February 2024)

Response to Reviewer 1 Comments

Mansuscript ID: EGUSPHERE-2025-6305

Journal: Weather and Climate Dynamics (WCD)

Corresponding author: Noor AlShamsi (nalshamsi@ncm.gov.ae)

We thank the reviewer for their constructive and helpful comments. We believe that addressing them will improve the clarity and overall quality of the manuscript. Reviewer comments are shown in italics, and author responses are provided in blue plain text.

1. General comments

Comment 1: Lines 55-60 and conclusions: The possible roles of dust on hailstorms are highly complex, which seems a bit oversimplified in this study (see e.g., Varble et al. 2023, Brennan and Wilhelm 2024). The authors could solve this by including more literature and discussion. However, this also means that it is not easy to prove whether dust contributed in any way to the event studied here. The coarse dust dataset analyzed here is certainly not enough to conclude that dust was unimportant because microphysical processes cannot be investigated without convection-resolving models. I also have some doubts whether this dataset can accurately capture boundary layer dust concentrations, especially if local small-scale processes like haboobs are involved (lines 182-183).

Varble, A. C., Igel, A. L., Morrison, H., Grabowski, W. W., & Lebo, Z. J. (2023). Opinion: A critical evaluation of the evidence for aerosol invigoration of deep convection. Atmospheric Chemistry and Physics, 23(21), 13791–13808. https://doi.org/10.5194/acp-23-13791-2023

Brennan, K. P., & Wilhelm, L. (2024). Saharan dust linked to European hail events. EGUsphere, 2024, 1–19. https://doi.org/10.5194/egusphere-2024-3924

We agree with this point, and in hindsight the discussion on dust was too simplified given how complex these processes actually are. Our intention was not to dismiss a possible role of dust, but rather to avoid over-interpreting what can be inferred from the available data. That distinction was clearly not communicated well in the original version.

To improve this, we will expand the discussion and bring in additional literature (e.g., Varble et al., 2023; Brennan and Wilhelm, 2024) to better reflect the current understanding of aerosol–convection interactions. We will also make it clearer that assessing microphysical impacts on hail formation would require convection-permitting simulations, which are beyond the scope of this study.

In addition, we will explicitly acknowledge the limitations of the CAMS dataset, particularly its coarse resolution and its inability to capture boundary-layer processes such as haboobs.

Finally, we will revise the conclusions so that dust is discussed more cautiously, as a possible large-scale environmental factor, without making strong claims about its role in storm-scale processes.

Comment 2: References are missing in several places: lines 71-73, lines 155-157, lines 181-183, lines 235-236, lines 271 (why is this diagnostic more accurate?), 373-374, Line 427, Lines 508-512

All identified sections will be revised to include appropriate references supporting physical interpretations and dataset limitations. The description of VIHM will be expanded to clarify its physical basis as a mass-based, column-integrated diagnostic that more directly represents hail production compared to size-based or environmental proxies. Supporting references will be added accordingly.

Comment 3: The authors need to be more accurate in their severe convective storm terminology. Most large hail is produced by supercells (e.g., Blair et al. 2017, Feldmann et al. 2025). The single track and rightward storm motion in Fig. 9 suggest that the main hail producer here also was a supercell, although it is hard to say based on the limited data shown. In some places, the authors also mention a single updraft, which would be consistent with a single-celled storm mode (line 625). However, in other places they use "MCS" (lines 534, 627), which implies multiple updraft regions. Elsewhere, the authors even seem to link the hailstorm evolution to the evolution of the whole synoptic system (line 359). I recommend the authors to improve their understanding of severe storm dynamics (e.g., Markowski and Richardson, chapters 7-9). Then, a much deeper analysis seems warranted to investigate the storm structure. For example, radar reflectivity fields every hour would be good to show to give the reader a general impression how the convection was structured and evolved.

Blair, S. F., Laflin, J. M., Cavanaugh, D. E., Sanders, K. J., Currens, S. R., Pullin, J. I., ... Mallinson, H. M. (2017). High-resolution hail observations: Implications for NWS warning operations. Weather and Forecasting, 32(3), 1101–1119. https://doi.org/10.1175/WAF-D-16-0203.1

Feldmann, M., Blanc, M., Brennan, K. P., Thurnherr, I., Velasquez, P., Martius, O., & Schär, C. (2025). European supercell thunderstorms—A prevalent current threat and an increasing future hazard. Science Advances , 11(35), 1–12. https://doi.org/10.1126/sciadv.adx0513

Markowski, P., & Richardson, Y. (2010). Mesoscale meteorology in midlatitudes (Vol. 2). John Wiley and Sons.

We will revise the manuscript to ensure consistent and physically accurate storm terminology, clearly distinguishing between storm-scale and synoptic-scale processes. Thank you for pointing out that readers are more familiar with radar reflectivity fields. A new time-sequenced radar reflectivity analysis will be introduced, showing hourly evolution of the hail-producing storm. A dedicated paragraph will be added to assess storm organization, noting that observed features (e.g., persistent core and rightward motion) are suggestive of organized convection, while avoiding definitive classification without Doppler-based confirmation. The discussion will be strengthened using established severe storm terminology (e.g., Markowski and Richardson).

Comment 4: Lines 401-404: Convection initiation is a complex process which in many cases involves rising thermals, orography, fronts, and upper-level lifting to different degrees. Please provide more evidence or remove such statements.

We will revise the text to reflect a multi-factor interpretation of convective initiation. Additional supporting evidence will be incorporated, including frontal gradients, low-level convergence, and upper-level divergence associated with jet dynamics. References to established conceptual models will be added, and the linkage between diagnostics and interpretation will be made explicit.

Comment 5: Line 443: The only sounding analyzed is from 00 UTC, so during the night. So it is not surprising that it shows a stable surface layer and no surface-based CAPE. However, CAPE can also be calculated for parcels originating at higher levels where they can become unstable (often used is most unstable CAPE). This would be more fitting given *the assumption that the storms were elevated. It seems your interpretation that CAPE was low in your environment is only based on surface-based CAPE (Line 707-712).*

Thank you for this insightful comment. To address this, we will include Most Unstable CAPE (MUCAPE) in the analysis and figures. This will allow us to better represent the presence of elevated instability, even in the absence of surface-based CAPE. We will also revise the discussion to clearly distinguish between the stable surface layer and the presence of instability aloft, which is more consistent with a nocturnal, dynamically forced setup.

2. Minor comments

Comment 6: Lines 45-48: There is repeated content here so these lines could be removed.

We agree. Those lines are repetitive and will be removed to streamline the text.

Comment 7: Some parts seem a bit out of context or unnecessary in a scientific paper and could perhaps be removed: Lines 38-39, lines 109-115, lines 236-241, lines 373-374

We appreciate this point. We'll go through them carefully and either trim them down or remove them where they don't directly support the core analysis.

Comment 8: 1: There is no red star ; also, does the last sentence refer to (b), then maybe put it earlier

Thank you for catching this. We'll correct the missing symbol in the figure and revise the caption so that the description of the panels is clearer and in the right order.

Comment 9: Lines 160-163: does this mean up to 3 day forecast times were used? why not use the closest analysis time which should be more accurate, no?

We'll revise this part to better explain which forecast lead times were used and why, and clarify the reasoning behind using them instead of the analysis fields.

Comment 10: Lines 167: do you mean "coarse" not "course"?

Yes, this is a typo. We'll correct "course" to "coarse" and do a general check for similar errors throughout the manuscript.

Comment 11: Lines 253: remove repeated words

Thank you. We'll remove the repeated words and check the surrounding text for any similar issues.

Comment 12: Section 2.4: most of this section seems unnecessary and could be incorporated into the previoues subsections. Only the part on hail reports gives a new component, so this could be put in single section maybe? Some more context on the halsizes would also be insightful (see major comment).

We agree with this suggestion. We'll reorganise text into the relevant sections (e.g. move methodology statements to the methodology section) while keeping only the material that introduces new information.

We'll also clarify the role of the hail data, emphasizing that we rely on radar-derived proxies rather than direct size measurements, and briefly discuss the associated limitations.

Comment 13: Line 379: Should this method not be explained in the methods section?

We'll move the explanation to the Methods section so that it's defined before being used in the analysis.

Comment 14: Lines 397: I suggest using the symbol for theta instead on PT

We will replace PT with θ and update the notation throughout the manuscript to keep it consistent.

Comment 15: Lines 399-401: I don't see a clear front, there is a gradient but not very localized, perhaps indicate with a line.

We understand the concern here. Rather than manually identifying a front, which can be somewhat subjective, we'll strengthen this part by including additional fields (e.g., MSLP and geopotential height) to provide a more objective representation of the baroclinic structure.

Comment 16: 4: In most plots the like in this Fig. the times and heights don't align which makes it hard to compare them. Of course this is not always possible so consider this small remark.

We appreciate this observation. The mismatch in times and levels is intentional, but we agree that this isn't immediately obvious to the reader.

The fields were selected to highlight the stages and altitudes where the relevant processes are most clearly expressed, and these don't always occur at the same time or level. Forcing strict alignment would, in some cases, make the signals less clear rather than more.

That said, we'll make this reasoning explicit in the captions and/or text so that the choice is clearer to the reader.

Comment 17: Lines 616-621: not sure what you mean, the plot only shows the height of maximum reflectivity, not if there is descent.

We agree that the wording here was not precise. The figure only shows the height of maximum reflectivity, so interpreting this as vertical motion is misleading. We'll revise the text to clearly describe what is actually shown.