

Review of manuscript “egosphere-2025-6305”

Title: Jet superposition and a cut-off low behind a rare heavy hailfall episode in the United Arab Emirates (10-12 February 2024)

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Summary and recommendation

This study investigates the atmospheric processes leading to a hailstorm over the United Arab Emirates which occurred in February 2024. Using different observation-based data sources, the authors examine the synoptic-scale and mesoscale processes associated with the storm, the influence of dust on the storm, and link radar-based to surface hail measurements. The authors conclude that synoptic to mesoscale processes were the primary forcing mechanisms of the hailstorm while dust had only a minor influence.

Overall, I found the multi-source perspective on a rare hailstorm in the desert interesting, potentially providing a basis for a scientific study. However, as the study currently stands, I feel I cannot recommend its publication. The manuscript is excessively long and descriptive, having numerous arguments and reasoning without support from convincing evidence, and thus lacking the scientific rigor and depth that readers expect. Below I provide a few general and several detailed comments that further support my evaluation, and hopefully, that provide some guidance for revising the manuscript.

General comments

1. Jet dynamics / jet superposition. One of the key selling points of the study seems to be the “superposition of the subtropical and polar jet streams”. However, a thorough discussion on this aspect seems absent in the manuscript and it remains unclear how these processes played a role in the formation of the hailstorm. All that Fig. 1a seems to show is a zonally oriented subtropical jet along with a (transient) northerly flow being part of a midlatitude wave structure merging with the subtropical jet. Complex phrases such as “Coupling between the polar and subtropical jet streams resulted in jet superposition” (lines 8-9) appear somewhat meaningless and empty given the lack of substantial analysis and quantification. The introduction states “However, the specific role of the jet superposition (JSP) in organizing hail-producing convection within arid subtropical environments remains insufficiently quantified”. The present study seems not to provide any quantification, and therefore, it remains unclear what the present study adds.

2. Lack of evidence supporting statements and findings. The manuscript is full of arguments and reasoning pointing to a variety of processes at the general level without that any evidence for these claims is shown in the results (e.g., figures). For example, the manuscript emphasized the importance of a cut-off low given the mentioning in the title, the abstract, introduction, and section 3.2. While I do not doubt that such a weather system was central to this extreme event, this feature has not been shown in any of the figures. While this could easily be remedied by overlaying 500-hPa geopotential height contours in any of the figures, similar issues are prominent throughout the manuscript, making multiple references to “upper-tropospheric divergence” (lines 8-9, 347, 449, 692, 730), vertical motion or dynamic forcing for ascent (lines 347, 695, 804), cyclogenesis (lines 350,695), without that these processes are shown.

3. Manuscript text. The length of the manuscript is overly long exceeding 800 lines without that the presented findings seem to justify this length. At numerous occasions the text discusses aspects in

detail which do not seem absolutely needed for the manuscript. Moreover, the text is very descriptive lacking the scientific rigor that readers may expect. Some stretches of text seem repetitive given that similar aspects have been discussed at multiple occasions. Several examples are listed under detailed comments below. Additionally, I cannot help but I am left with the impression that the writing heavily relies on used large-language models as the writing seems at first sight very polished, but the underlying meaning of sentences and context becomes confusing and fuzzy at numerous occasions. It leaves me with an unsure feeling whether I am reading the results of a well-thought case study or merely the output of a large-language model given the long breath of generalized statements lacking precise connection with the presented study findings.

4. Influence of dust on the hailstorm. While the analysis on dust seems interesting, it remains largely unclear and speculative how dust may have influenced the formation and evolution of the convective storm.

5. References to existing literature. The manuscript discusses several key processes which have been documented in previous studies addressing extreme precipitation events and convective storms in the Middle East region without that these studies are cited. Examples are cut-off lows, potential vorticity structures, moisture transport, the Red Sea trough, and surface cyclones. Current references seem a rather general mix of previous studies on rather distant topics (e.g., linked to tropical cyclones, atmospheric rivers, etc.), while studies much closer to the present work are not cited. This should be corrected by adequately referencing previous related work.

Specific comments

Line 8. "Coupling between the polar and subtropical jet streams" reads a bit abstract. Should this be interpreted as the jet streams being merged?

Line 9. "Cut-Off Low" does not need to be capitalized.

Lines 16-17. What kind of "atmospheric instability"?

Line 36. The introduction mentions the role of a cut-off low in this specific event. This weather phenomenon has been discussed in previous case studies and climatological investigations in relation to heavy precipitation in the region. These studies should be cited and acknowledged.

Lines 40-41. The sentence refers to a role of the vertical alignment of the jets, while Fig. 1 seems to show a merging of the jets in the horizontal plane instead of any vertical cross section.

Lines 45-48. These two sentences seem to repeat the few foregoing sentences.

Lines 52-53. The phrase "The resulting circulation patterns thereby establish a robust dynamical framework..." is one of many occasions that make me feel the text overly relies on the use of large language models. The writing seems polished and strong, but the underlying meaning fuzzy and off point.

Line 58. "Diurnal conditions"; probably, "daytime conditions" is meant?

Lines 61-64. This sentence seems to repeat the topic of the foregoing paragraph.

Lines 64-66. It remains unclear how dust can affect processes relevant to convective storms and hail.

Lines 69-70. "limits systematic surface documentation"; what is meant by that? Should "event confirmation" be "event identification" or is this phrasing specific to observations of hail storms?

Lines 70-73. This reads rather as the conclusion of the paper. If this has been shown in previous studies, relevant citations should be provided.

Lines 81. It is not clear how addressing the influence of dust can "enhance predictive capability".

Line 92. In the title "Study area and overview", overview, of what?

Lines 112-115. This sentence seems to mix up rather different phenomena into one sentence, e.g., the Indian summer monsoon, ENSO, and then winter precipitation trends.

Section 2.1. I have generally the feeling that much of this subsection would also fit well in the introduction.

Lines 137-148. This paragraph reads as a summary of the research findings and comes at a very odd moment, at the end of section 2.1, introducing the study area.

Lines 259-275. Would these sentences fit better in the corresponding parts of section 2.2?

Lines 355. The notion of "upper-tropospheric subsidence" seems a bit in contrast with conditions favorable for the development of convective storms and "baroclinic forcing", even though subsidence may occur particularly at the west flank of the high-PV air.

Section 3.1 is an example of a paragraph that is full of inferences to atmospheric processes and proposed linkages which are not shown.

Line 361. "increases static instability" seems incorrect as any arising static instability would immediately be eliminated by convective overturning.

Line 378. The title mentions "cyclone evolution" but the text speaks about a cut-off low, which is not the same.

Line 380-381. The sentence "This feature .. trough intensification", is a rather strong statement without that supporting evidence is provided.

Line 382-383. The definition of a cut-off low is correct, and it would strengthen the study to actually show this.

Line 384. I couldn't find the meaning of the acronym "AG".

Line 385. Why is upward motion and the surface cyclone not shown? Overlaying sea level pressure contours over satellite imagery and dust loading could be helpful for interpreting these figures. The same is valid for 500-hPa geopotential height and PV contours in these figures when relevant for drawing inferences.

Line 398. The inferences to moist and dry air masses are likely true, but it isn't shown.

Lines 415-419. The RST isn't shown in the figures, and therefore, these arguments aren't supported.

Lines 422-425. This is another example of statements that lack any support by findings in figures and computations.

Line 427. “documented in previous studies”; it would strengthen the manuscript to add the citations specifically.

Line 429. The sentence “while .. from the Arabian Sea Basin” is speculative.

Lines 436-439. Is there any evidence that can support these statements?

Lines 450-454. Also here, the statements lack evidence.

Line 494-495. The statement could be better supported by showing the cut-off low in the figure.

Section 3.5 is an example of text with many statements without that evidence is shown when it comes to linking observed dust with processes in the circulation

Lines 586-595, 606-611. Would these parts better fit in the methods and data section as the text discusses motivation for used thresholds?

Lines 696-696. The references to “a tropical anticyclonic PV anomaly” and “associated PV streamer structure” come out of the blue and seems not supported.

Lines 718-720. The discussed processes have been documented before in other studies and need to be cited adequately, in contrast to the included citation which seems very general.

Lines 760-762. It seems inaccurate to pose that sounding data is used to diagnose “jet-level forcing and IVT” when the study doesn’t do this.