

Response to Reviewer 2 Comments

Manuscript 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. Many of the points raised highlight areas where the manuscript can be clarified or strengthened, and we believe that addressing them will improve the clarity and overall quality of the study. Reviewer comments are shown in italics, and author responses are provided in blue plain text.

1. General comments

Comment 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. 2a (sic 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.

Thank you for this insight and for highlighting the limitations of Fig2a. We agree that, in its current form, the discussion of jet superposition is not developed sufficiently to support its prominence in the manuscript.

To address this, we will include cross sections of the jet structure following the approach of Winters and Martin (2017; doi.org/10.1175/MWR-D-16-0262.1). These cross sections will extend beyond the domain shown in Fig. 2a and will allow us to better illustrate how the polar and subtropical jet cores evolve and become superimposed.

We will also revise the text to make the scope of this analysis clearer. In the Introduction, we will avoid wording that suggests formal quantification and instead frame the study as a process-based case analysis. Our goal is to show how jet interaction may have contributed to the event, rather than to define or apply a quantitative JSP metric.

In the Results and Discussion, we will expand the explanation of the physical mechanisms involved. This will include a more explicit link between the jet interaction and the resulting upper-level wind maxima, divergence, synoptic ascent, and frontal structure, and how these processes may have supported the organization of hail-producing convection. Overall, this revision aims to make the argument more physically grounded and clearly supported by the diagnostics.

Comment 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.

Thank you for highlighting the lack of a 500-hPa geopotential height contour. We will add the isolines to the necessary figures and refer to these figures in support of our statements. In several places, the discussion refers to processes that are not directly shown in the figures, which makes it difficult for the reader to follow or verify the interpretation.

We will include 500 hPa geopotential height fields to explicitly show the cut-off low, and we will add additional diagnostics where needed to support statements related to upper-level divergence, baroclinic structure, cyclogenesis, and forcing for ascent.

At the same time, we will review the wording throughout the manuscript. Where the available diagnostics do not fully support a given interpretation, we will either add the necessary fields or adjust the wording to keep the statements consistent with what is actually shown. This should make the analysis more transparent and address the concern that some statements currently appear broader than the supporting evidence.

Comment 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.

We appreciate this candid feedback. We will carry out a substantial editorial revision of the manuscript to improve conciseness, sharpen the scientific focus, and reduce overly descriptive or repetitive material. This revision will involve removing sentences and paragraphs that do not directly support the central analysis, condensing repeated explanations, and tightening passages that currently discuss mechanisms in broad terms without sufficient connection to the actual findings.

Comment 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.

Thank you for raising this point. We will expand the discussion by incorporating additional literature (e.g., Varble et al., 2023; Brennan and Wilhelm, 2024) and by more clearly acknowledging the complexity of aerosol–convection interactions. We will also explicitly state that assessing the microphysical role of dust in hail formation would require convection-permitting simulations, which are beyond the scope of this study.

In addition, we will clarify the limitations of the CAMS dataset, particularly its coarse resolution and its limited ability to capture boundary-layer dust processes such as haboobs.

Finally, we will revise the wording in the Discussion and Conclusion to frame dust more cautiously, as a possible large-scale environmental factor, rather than as a directly demonstrated driver of storm evolution or hail production.

Comment 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.

Thank you for pointing this out. We will strengthen the literature review to include work on cut-off lows, PV structures, Red Sea trough dynamics, moisture transport, and convective events over the Arabian Peninsula and the wider Middle East.

These additions will be made mainly in the Introduction and Discussion, and also where relevant in the Results when previous findings are referenced. We expect the additional citations in the Introduction will not extend the section beyond reasonable length and should enhance the manuscript.

2. Specific comments

Comment 6: Line 8. “Coupling between the polar and subtropical jet streams” reads a bit abstract. Should this be interpreted as the jet streams being merged?

Yes, merged/superimposed/single jet core. We will edit accordingly.

Comment 7: Line 9. “Cut-Off Low” does not need to be capitalized.

We will correct “Cut-Off Low” to “cut-off low” throughout the manuscript for consistency.

Comment 8: Lines 16-17. What kind of “atmospheric instability”?

Thank you for pointing this out. In the revised manuscript, we will specify that we are referring to elevated conditional instability associated with moist layers above a stable boundary layer. At the same time, we recognize that the reviewer may be referring to baroclinic versus barotropic instability in the synoptic context.

To avoid this ambiguity, we will revise the wording so that the type of instability is clearly defined in each case, distinguishing between thermodynamic (convective) instability and the large-scale baroclinic environment.

Comment 9: 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.

We will add relevant regional references on cut-off lows and heavy-precipitation events to better contextualize the study.

Comment 10: Lines 40-41. The sentence refers to a role of the vertical alignment of the jets, while Fig. 2 (sic Fig. 1) seems to show a merging of the jets in the horizontal plane instead of any vertical cross section.

Thank you, this is a helpful observation. We will revise the sentence to better reflect what is displayed, focusing on the horizontal interaction between the jets rather than implying a vertical structure that is not illustrated.

Comment 11: Lines 45-48. These two sentences seem to repeat the few foregoing sentences.

Noted. We will remove or consolidate these repeated lines to improve conciseness as part of our editorial process (see response to Comment 3).

Comment 12: 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.

We will replace it with more direct and physically explicit wording so that the intended process is clearly defined and tied to the analysis.

Comment 13: Line 58. “Diurnal conditions”; probably, “daytime conditions” is meant?

We will revise this wording to a more precise expression, such as “daytime surface-heating conditions.”

Comment 14: Lines 61-64. This sentence seems to repeat the topic of the foregoing paragraph.

Noted. We will remove or consolidate these repeated lines to improve conciseness as part of our editorial process (see response to Comment 3).

Comment 15: Lines 64-66. It remains unclear how dust can affect processes relevant to convective storms and hail.

Thank you for highlighting this. Please see response to comment 4. We will introduce the role of dust in hail formation in the introduction and refer to it here.

Comment 16: 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?

We will revise this wording for clarity and use more direct observational terminology.

Comment 17: 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.

We will revise or relocate it, and include appropriate references if the statement is retained.

Comment 18: Line 81. It is not clear how addressing the influence of dust can “enhance predictive capability”.

We will revise or relocate this statement and add citations where needed.

Comment 19: Line 92. In the title “Study area and overview”, overview, of what?

We will revise to “Study Area” or “Study Area and Literature Review”

Comment 20: 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.

Thank you. We will review whether it should be removed or expanded.

Comment 21: Section 2.1. I have generally the feeling that much of this subsection would also fit well in the introduction.

Thank you for this observation. We plan to retain this subsection within the Data and Methods section, but we will review it carefully to ensure it stays focused on methodological context and does not become overly descriptive.

Comment 22: 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.

We will remove this paragraph or relocate any essential content to a more appropriate section.

Comment 23: Lines 259-275. Would these sentences fit better in the corresponding parts of section 2.2?

Yes. We will move these sentences to the relevant subsections in Section 2.2 for better structure.

Comment 24: Line 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.

The wording here can indeed be misleading in the context of convective development. We will revise this part to better reflect the role of upper-level dynamics, without implying conditions that contradict the observed convective environment.

Comment 25: Section 3.1 is an example of a paragraph that is full of inferences to atmospheric processes and proposed linkages which are not shown.

This is an important comment. We will revise Section 3.1 to tie the discussion more directly to the displayed diagnostics and reduce unsupported inferences.

Comment 26: Line 361. “increases static instability” seems incorrect as any arising static instability would immediately be eliminated by convective overturning.

We agree that the phrasing is not physically accurate in this context. Will replace this wording with a more accurate description, such as steepening of mid-tropospheric lapse rates.

Comment 27: Line 378. The title mentions “cyclone evolution” but the text speaks about a cut-off low, which is not the same.

We will revise the terminology to distinguish more clearly between cut-off low evolution and cyclone development.

Comment 28: Line 380-381. The sentence “This feature .. trough intensification”, is a rather strong statement without that supporting evidence is provided.

Thank you, this is a fair concern. The statement is currently stronger than what is directly supported by the figures.

We will revise this part to include additional diagnostics, such as 500 hPa relative vorticity and 1000–500 hPa thickness, to better support the interpretation. At the same time, the wording will be adjusted so that it remains consistent with the available evidence.

Comment 29: Line 382-383. The definition of a cut-off low is correct, and it would strengthen the study to actually show this.

We agree that the cut-off low should be explicitly demonstrated in the figure set rather than only described in the text. To address this, we plan to overlay 500 hPa geopotential height on the relevant synoptic figure so that the closed circulation associated with the cut-off low is clearly visible. The corresponding text will then be revised to refer directly to this field when describing the cut-off low structure and evolution.

Comment 30: Line 384. I couldn't find the meaning of the acronym "AG".

Thank you for noting this. We will replace "AG" with "Arabian Gulf."

Comment 31: 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.

We will revise the figure overlays to better show the surface cyclone and upper-level structure, and we will moderate the wording where ascent is not directly shown.

Comment 32: Line 398. The inferences to moist and dry air masses are likely true, but it isn't shown.

We will add or reference a moisture diagnostic to directly support this interpretation.

Comment 33: Lines 415-419. The RST isn't shown in the figures, and therefore, these arguments aren't supported.

This is a valid point. The discussion currently references the Red Sea Trough without clearly linking it to the figures.

We will revise this part to explicitly connect the interpretation to the low-level wind and moisture fields.

Comment 34: Lines 422-425. This is another example of statements that lack any support by findings in figures and computations.

We will revise this paragraph so that each interpretive statement is explicitly tied to the corresponding figures and diagnostics. Where the discussion refers to enhanced moisture availability, thermodynamic support, or conditions favorable for convective development, we will connect these points directly to the relevant fields, such as equivalent potential temperature, moisture, or wind diagnostics, depending on the final figure arrangement. We will also moderate the wording where needed to avoid conclusions that extend beyond the available evidence.

Comment 35: Line 427. "documented in previous studies"; it would strengthen the manuscript to add the citations specifically.

We will include the appropriate references at this location.

Comment 36: Line 429. The sentence "while .. from the Arabian Sea Basin" is speculative.

We will revise this statement to make it more cautious and more clearly grounded in the analyzed circulation fields. Rather than presenting the Arabian Sea Basin as a definitive moisture source, the revised text will indicate that the mid-tropospheric flow pattern is consistent with, or suggests, a contribution from that region.

Comment 37: Lines 436-439. Is there any evidence that can support these statements?

Thank you for raising this. We will reassess this passage carefully and revise it so that only statements supported by the presented diagnostics are retained. If the discussion concerns weakening of convection, reduced moisture supply, or changes in the surrounding thermodynamic environment, we will tie those points directly to the relevant moisture and wind fields. Any elements that we will not be able to support adequately by the existing analysis will either be removed or rephrased in more cautious terms.

Comment 38: Lines 450-454. Also here, the statements lack evidence.

This is consistent with earlier comments. We will revise the paragraph to better align the interpretation with the figures and avoid unsupported inferences.

Comment 39: Line 494-495. The statement could be better supported by showing the cut-off low in the figure.

This is a good suggestion. We will ensure that the cut-off low is clearly shown in the figures and referenced explicitly in the text.

Comment 40: 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

Thank you for this observation. Please see response to comment 4. We will revise Section 3.5 to make the linkage between the observed dust distribution and the large-scale circulation more explicit and more carefully constrained. In particular, we will ensure that references to transport pathways, synoptic positioning, or circulation-related dust accumulation are directly tied to the relevant upper-level and surface fields shown in the figures. At the same time, we will moderate the language so that the manuscript clearly distinguishes between observed dust patterns and inferred radiative or microphysical effects.

Comment 41: Lines 586-595, 606-611. Would these parts better fit in the methods and data section as the text discusses motivation for used thresholds?

Yes. We will move these passages to the Data and Methods section.

Comment 42: 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.

Thank you for pointing this out. We agree that the terminology here is introduced without sufficient support in the figures. We will revise this part to refer only to structures that are explicitly shown and avoid introducing unsupported concepts.

Comment 43: 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.

We will add more specific and regionally relevant references at this location.

Comment 44: 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.

This is a helpful clarification. We will clarify that the radiosonde data are used to characterize the vertical thermodynamic and kinematic structure of the environment, including stability, moisture depth, and vertical wind shear. The diagnosis of jet-level forcing, large-scale dynamical structure, and moisture transport will instead be attributed explicitly to the ECMWF-derived fields.