

Interactive comment on “Atmospheric Conditions Leading to an Exceptional Fatal Flash Flood in the Negev Desert, Israel” by Uri Dayan et al.

Anonymous Referee #3

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Review of the manuscript “Atmospheric Conditions Leading to an Exceptional Fatal Flash Flood in the Negev Desert, Israel” by Uri Dayan, Itamar M. Lensky and Baruch Ziv

General

The paper presents a case study of an exceptional storm that not only had fatal impact, but was also a climatological outlier in the sense of its timing in the rain season, the distribution of precipitation relative to the regional orography, and the cyclone characteristics. The analysis involves a good combination of local observations and reanalysis data to infer the local instability conditions and the weather systems supporting the intense precipitation on the large-scale. Overall, the topic is important and the methodological approach is well designed. However, I have several reservations with regards

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to the data, diagnostics and interpretation, as detailed below, requiring major revision of the manuscript before it can be considered for publication. The text is in many places too thin and not accurate enough, or backed by sufficient evidence, as I elaborate in the specific comments. Enhancing the introduction is necessary to place this case in a climatological context and provide more solid background about spring season rainfall in the region and Mediterranean cutoff lows.

Major comments

1. Introduction: this section is too thin to support the understanding of the unique aspects of this storm. In my view, more substantial background and recent literature should be included before the specific research aims are outlined. For example, I strongly recommend to include information on the following missing aspects: weather systems conducive for precipitation in the region in the transition seasons; what is the typical precipitation distribution in spring storms versus Cyprus lows; sharav cyclones; tropical systems affecting precipitation in the region; what are the typical precipitation intensities and how common is severe convection in such storms in this season; how common are cut-off lows?
2. The paragraph describing the aim of the study (L46-48) should be clarified. It is currently not clear what the authors mean by “one of the latest spring severe events. . .” does “latest” refer to the most recent one? Or to severe precipitation occurring very late in the rain season? Especially when the introduction is sufficiently expanded, it should be more clearly outlined what is unique about this storm/flood. For example, how well was it forecasted? What is unusual about the distribution of precipitation? What is unusual about this cut-off low? What else is unusual beyond its fatal impact? What do the authors mean by “its unique features” (L47)? The authors should avoid using such general terms and be more specific.
3. The construction of the reference list of cases is not outlined with sufficient details. Are there objective quantitative criteria? Lines 58-59 and L 66-67 are still too general.

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Which streams are considered? What is the threshold for discharge? In how many stations? What is the reference region? Is the list restricted to cut-off lows? It will be good to reference Table 1 at this stage, and provide information on the precipitation in those reference storms, to then contrast the current storm in focus that produces a very different precipitation distribution.

4. There is a somewhat inconsistent usage of reanalysis datasets, with some fields taken from NCEP/NCAR, but PV and omega taken from ERA5. Why not analyse all fields from ERA5?

5. The motivation for examining and comparing the MCV values is not clearly revealed. Why not consider the shear vorticity as well and take the commonly-used relative vorticity as a measure for the intensity? Please justify this, especially given the fact that relative vorticity is anyway shown in Fig. 3. This clarification is again needed with regard to the list of reference cases. What do we learn from the high MCV? How do you interpret these differences?

Minor comments

1. L13: "one of the latest. . .3 decades" this is not clear and appears again throughout the manuscript. Please rephrase and clarify if you refer to the late timing in the season or to longer time scales.

2. L25: delete the mention of the temperature anomaly which is not shown, or add a section with this evidence to the results.

3. L43: what does "them" refer to?

4. L53: I suggest to replace "Material" by "Data and Methods"

5. L57: delete "to"

6. L58: what is meant by "maximum intensity"? it should be more accurate and clarify if it refers to precipitation/discharge/a vorticity measure/cyclone characteristic etc.

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7. Fig. 1 caption: indicate the times in UTC for the date range; add "the red start marks the location of . . ."

8. Equations: replace the cross sign with a dot, to not confuse with vector notation.

9. L83: how is the depth of the cyclone estimated? Please provide the accurate measure.

10. L92: add "temperature" after "mean".

11. L94: remove one S from SSI.

12. L89-103: for each index, mention which values indicate severe convection or thunderstorms.

13. Eq 4: What is meant by RH850,700? why a modification of the K-index is needed for the eastern Mediterranean?

14. L104: replace "also used" by "analysed". Add "as" before "if". Is PW based on ERA5?

15. L116: "which activated convection" – this statement is not backed by evidence at this stage and should be deleted.

16. L118-119: the term "precipitative elements" is not a clear.

17. L121-122: the statement is again not backed by evidence at this stage, and it is not clear how this conclusion is reached, especially since it appears in the beginning of the results section. Furthermore, here and in L184-188 and throughout the text, the relationship between dynamical factors / orographic effects / convection / thermodynamic factors should be more clearly defined and distinguished from one another. For example, omega in ERA5 incorporates mass fluxes from convection, so its attribution as a clear dynamical diagnostic is not accurate. Please readdress these definitions, and outline them with regard to the analysis you carry out in this work.

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18. L130-131: the transport of the dry air over the Levant is not consistent with the evolution of enhanced clouds at this stage.
19. Fig. 2: switch the locations of panels c and d; add “blue contours” after “m”, and “black contours” after “hPa”.
20. Fig. 3: replace “Course” by “Track”; add initials to the caption, e.g. “precipitable water (PW), CAPE (CA)” . . . ; replace here and throughout the manuscript (e.g., L200) “Km” with “km”; the blue text over the Med Sea is not visible; arrows in the late stages of the track are not visible;
21. Fig. 4 and accompanying text: it is unclear if this is PV or its anomaly (and how the anomaly is defined). I also recommend to switch the units to PVU and enlarge the domain.
22. L141-142: please add a reference to a climatology of such cutoff lows to demonstrate it is exceptional.
23. L165-166: This sentence is not clear. Can simplify by rewording to “. . . is expressed by enhanced easterly flow between the two vortices.”
24. Fig. 5: In my view, the figure belongs more naturally in the discussion, and clearly after Fig. 6. In the figure, the term “blocking L” is confusing and should be reworded to “cut-off L”.
25. Fig. 6: the arrows in (c) are not visible.
26. Fig. 7 caption: add in the end “, of 26 April 2018”; add units of MKI.
27. L 189: “cloud systems rotated cyclonically” – is there evidence for this advection as opposed to locally-produced clouds?
28. L187, 194: replace “ascendance” by “ascent”.
29. L201: change 3c to 2c.

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30. L202: add “reference” before “days”
31. Table 1: unclear how the depth is defined.
32. Section 3.3 and elsewhere: change “Kg” with “kg”
33. L226-227 “where it interacted with deep moist convection” this is a vague statement. Please clarify what you mean here.
34. L233: what is the evidence for “One is of tropical. . . at upper levels”?
35. L247: replace “one of the latest spring severe events” with “a severe storm occurring latest into spring”.
36. L257-258: cutoff lows are not typical midlatitude cyclones, but rather a particular case in which the high-PV air is separated horizontally from the stratospheric reservoir in the upper troposphere.
37. L268-269: Please comment on the timing. This is occurring one day before the flood.
38. L299: “-5 K temperature anomaly”. Please add more details on where is this anomaly located, at what vertical level, and add “not shown”.

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