Author's Reply:

Contrasting impact of different Mediterranean cyclones on the hydrological cycle and ocean heat content

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We thank the reviewers for their thorough review and helpful suggestions and appreciate their efforts to enhance the readability of the manuscript.

Below, we respond (in black font) to each comment (in blue) and describe the corresponding revisions.

Reviewer #1

Givon et al., (2025) leverages previous work on identifying distinct categories of Mediterranean cyclones to assess how these cyclones impact precipitation and evaporation rates across the Mediterranean basin. Through this analysis, the trend of precipitation and evaporation rates are found to differ across cyclone types, which can then inform future work on regional climate risk.

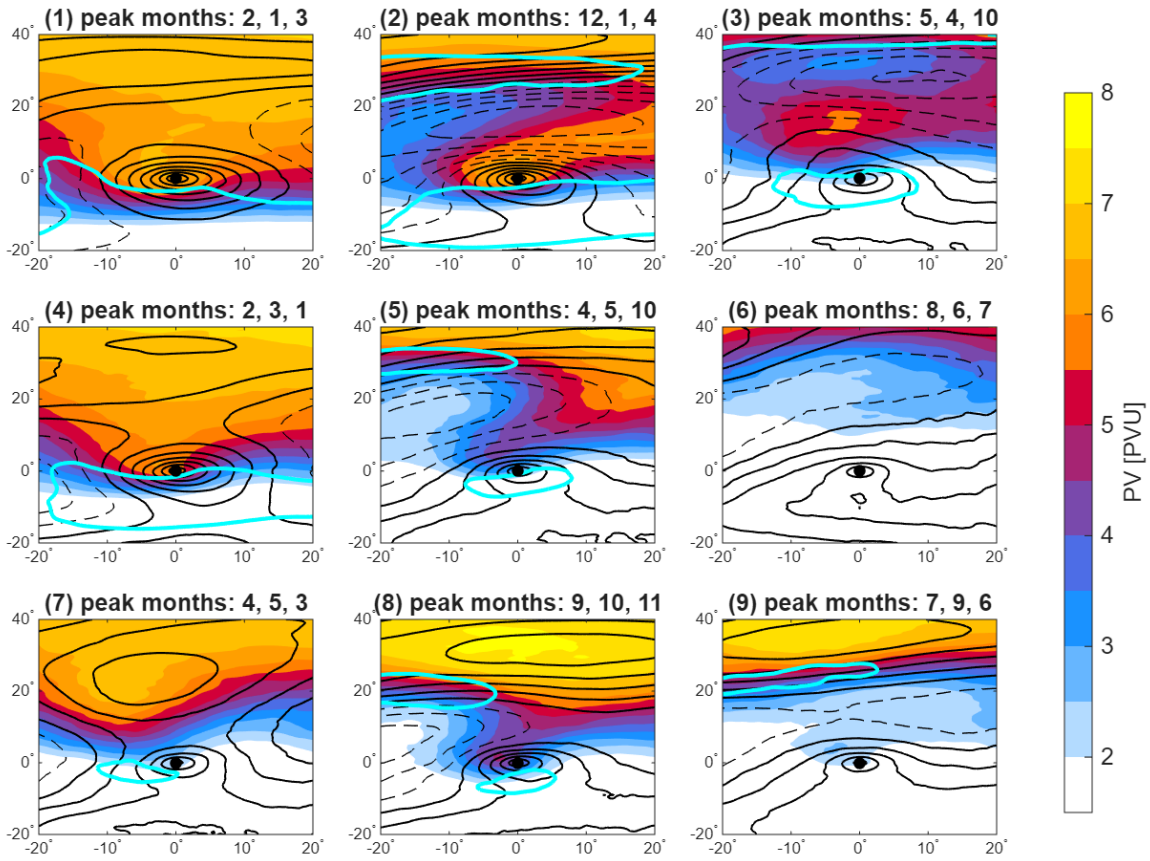
The figures and research are all high caliber and the paper flows as a coherent scientific story, for which I commend the authors. My main critique for this paper is with how much contextual information is not included to understand the discussion of this paper. I find this information necessary to interpret some of the main findings of the paper and to understand some of the points in the discussion section of the paper.

Main/general comments

When describing some of your results, you allude to results from your previous paper discussing the clustering of your cyclones. Often there are references to seasonality or large-scale flow configurations that I cannot deduce without examining your previous paper. An examples where I wanted additional context is lines 244–257 (referencing clusters as summer or winter, or the double jet configuration not shown in this paper). I think figures 3 and 6 from Givon et al., (2024a), as well as a brief summary of the results of these figures, should be incorporated into the paper or included in the supplemental information to give the reader more context on your previous work.

We appreciate the suggestion and recognize that additional context from our previous paper was not sufficiently included in the submitted version. We now elaborate on the relevant cyclone-group features in the new appendix A. Specifically, we added a discussion of the new Figure A1, showing cyclone-centered cluster composites of PV, SLP, and 300 hPa wind speeds, and denoting the seasonal affiliation of the clusters.

In addition, the cluster's dominant season is qualitatively denoted in Fig. 3 of the revised manuscript, and a new paragraph discussing the link between the clusters and the influence of seasonality on our results is added to Section 4 (see also response to Reviewer 2).



New Figure A1: Cyclone-centered cluster composites of upper-level (320–340 K averaged) PV (PVU, shading) and SLP (black contours at 2 hPa intervals, dashed above 1015 hPa) at minimum SLP time. Overlaid is the corresponding composite of the 30 m s−1 isotach on the 300 hPa geopotential surface (cyan contour, roughly denoting the subpolar and\or subtropical jets). Titles denote cluster number, name, and the leading cluster months in a descending order.

I also found that several points included in the discussion were not properly supported by the research presented earlier in the text. Some examples include:

Line 365: referencing double jets when none are shown

The reader is now pointed to Appendix A for contextualization.

Line 382: non-linear PV dynamics are not discussed before, so the statement feels unsubstantiated

We thank the reviewer for pointing out the abrupt introduction of non-linear PV dynamics. Handling non-linear PV configurations was indeed one of the key aspects of the classification under discussion. In the new Appendix A we now clarify that this refers to the nonlinear modes of Rossby waves detected for each MC cluster (namely AWB, CWB and cut-off lows, each representing a different non-linear RWB mode).

Lines 394–395: "drivers" of the cyclones have not been discussed, nor have the ways in which those drivers may change with additional warming

I can understand the desire to place this work into a larger context, but as it stands currently, I find many of the claims related to the results of Givon et al., (2024a) rather than your current paper. I think these instances need to be rewritten to explicitly link back to your previous work.

Thank you for this important distinction. We admit that this terminology can be misleading. We changed the phrase as follows: "Each cyclogenetic process responds differently to warming, leading to opposing influences on both the MHC and OHC". In other locations we changed 'drivers' with 'types'.

Line by line comments

Line 54: "more of it" is ambiguous, is this referencing more frequent MCs? More precipitation generally?

Changed to "Specifically, regions prone to moistening are expected to get moister, and vice versa for regions prone to drying."

Line 266: I am not sure what you mean by "local effects" in this context.

Changed to: "The precipitation induced by cluster 2 again exhibits more localized hot spots, while the contributions of the other clusters are significantly weaker"

Citation: https://doi.org/10.5194/egusphere-2025-6061-RC1

Reviewer #2 :

The paper by Givon et al. details the contribution of different Mediterranean cyclone types (obtained using a potential-vorticity based classification) to the Mediterranean Hydrological Cycle (MHC) and to the Ocean Heat Content (OHC) of the same basin. The employed approach is particularly fruitful, as it allows to attribute trends in evaporation P, precipitation E and net precipitation (P-E) to the different frequency and intensity changes of each cyclone category. Of particular interest is the observation that the impact of Mediterranean cyclones on MHC and OHC can substantially vary according to the cyclone type. The results also elucidate some region-specific characteristics of the MHC, with implications for the understanding of climate-change related trends.

I enjoyed reading this manuscript, and my comments mostly concern suggestions to improve readability and clarity of the manuscript. There is only one point that I feel needs to be checked or discussed, which concerns the effect of the seasonal cycle on the results obtained.

Major comment

- This analysis considers annual frequency, but the characteristics of the land surface with respect to the Mediterranean Sea vary substantially across months. For instance, cyclones in the same cluster might lead to OHC enhancement in the warm season and OHC depletion in the cold season. How would the results change if the analysis were to be repeated separately for NDJFM and MJJAS? Isolating the effect of the seasonal cycle would help contextualizing interesting results, such as the capability of cyclones in increasing OHC.

We thank the reviewer for this insightful suggestion. Indeed, the seasonal cycle is embedded in our cluster separation, while some spread does exist around each cluster's favored season, allowing the clusters to seldom show "off-season" appearances. We repeated the analysis for E and P by cluster, separated into the seasons as suggested by the reviewer (Figs R1 and R2).

The following phrase was added to the discussion section:

The seasonal cycle is embedded in the cluster separation. To isolate the role of seasonality, the results were further separated into winter and summer halves (not shown). While summer E is weaker across all clusters, the relative impact of each cluster as well as the spatial patterns remain surprisingly similar in both sub-groups. Specifically, cluster 4 still shows the largest instantaneous fluxes, followed by clusters 2 and 1. Winter cluster 6 cyclones are especially rare, while cluster 8 is more evenly distributed between the seasons and shows similar E and P patterns (not shown). We conclude that a further seasonal breakdown does affect the results quantitatively, but not qualitatively with respect to the relative importance of each MC type. This shows that MC types exhibit consistent features across seasons.

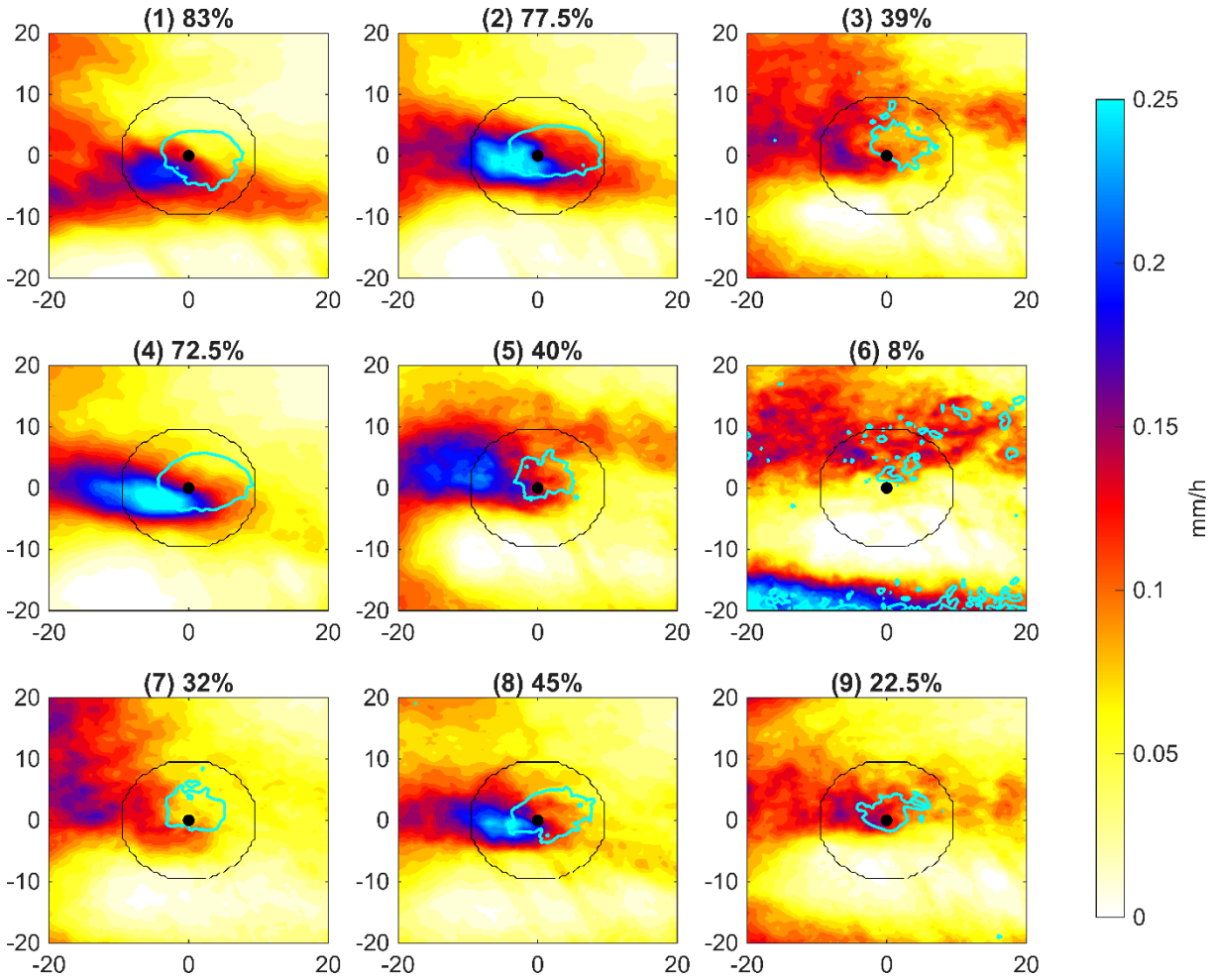


Figure R1: as Figure 3 in the main text but only for winter (October through March). The title denotes the cluster number and the fraction of cluster tracks included in the sub-group.

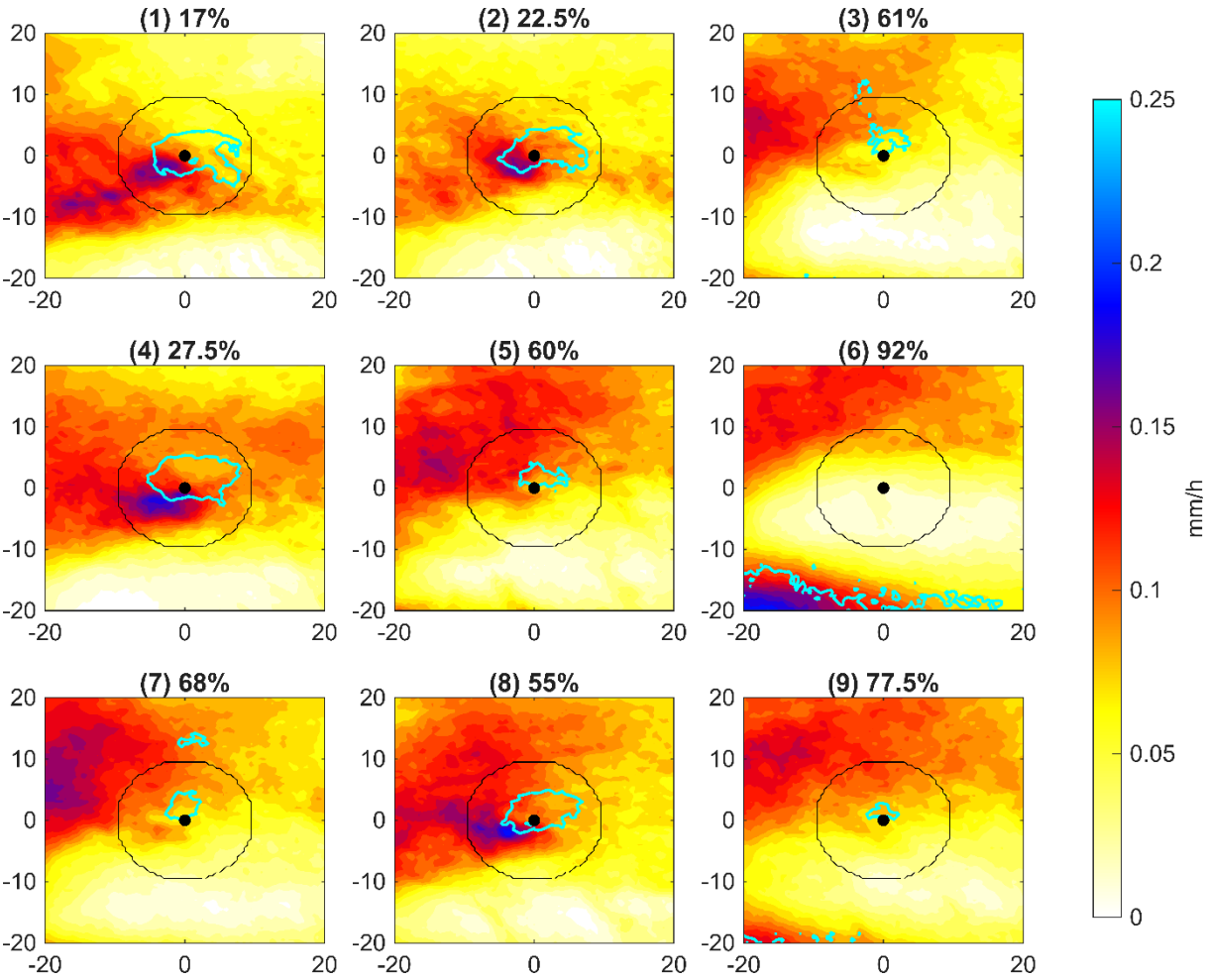


Figure R2: as in Figure R1 but for summer (April through September).

Minor comments

Line 56: A terminology clarification: as water uptake from the sea is determined by the dryness of the overlying air mass and not by its air temperature, I would suggest to replace "increased water-vapor uptake" with "increased water content" (unless the authors meant the increase in sea-surface temperatures with global warming, in that case it is correct but it would be better to specify it)

Thank you, we indeed refer to the latter. The sentence was changed as follows: "This phenomenon is often referred to as the "wet gets wetter - dry gets drier" mechanism, or the "Mediterranean precipitation paradox", often explained by a rise in MC-associated P due to increased water-vapor uptake caused by the rise in temperatures under global warming, and a drying effect as MCs grow less frequent in the southern and eastern Mediterranean due to the poleward shift of the Atlantic storm track (Chericoni et al., 2025)."

Line 58: "meridional shift" is general, can the authors specify whether poleward or equatorward and provide references? Unless the Mediterranean is in a "optimum" state for which any meridional shift would result in a reduction in precipitation.

Changed to "poleward shift of the Atlantic storm track"

Line 61-62: I don't fully understand the point about the necessity of strong E to sustain cyclones, do the authors refer to tropical cyclones here? Maybe a citation would also help to clarify the argument being made.

Indeed, the sentence reads: "While in the tropics enhanced E rates are deemed necessary to sustain cyclones and generate P, in the extra-tropics, extreme evaporation rates may arise preferentially in their cold sector" – implying

to the difference between tropical and extra-tropical cyclones. This distinction is highly relevant for the Mediterranean, which hosts a wide variety of cyclones on a spectrum ranging from extratropical to tropical-like cyclones.

Line 64-65: the authors could consider adding the paper by Papritz and Aemisegger on evaporation hot spots.

The proposed citation was added.

Line 68: I do not understand the use of the formulation "extratropical-like cyclones (most comparable to extratropical cyclones)" in particular what would be their implied difference with respect to extratropical cyclones?

Changed to: "While the frequency of MCs is expected to decrease...".

Line 160-161: which frequency is referred to here? The normalized one or the annual one?

The sentence is changed to explicitly refer to the annual frequency.

Lines 228-229, 236: these sentences (and others in the paragraph) imply some knowledge of the geographical distribution of the clusters, that needs to be gained by reading previous work -- Givon et al. (2024a). To improve readability, I suggest exchanging the discussion of Fig. 4 and Fig. 3.

The geographical distribution of the clusters is presented in figures 4 and 5 in black contours. Fig. 3 represents the link between the previous classification work, which is anchored in a cyclone-centered perspective, to this work, which puts more emphasis on the geographical distribution of the clusters. We therefore prefer to show Fig. 3 prior to the geographical projections of the cyclone-centered clusters. To improve readability, we now infer to the geographical distribution in Fig. 4 earlier on.

Line 255: this is the first time that the "double-jet" is introduced, there is a risk of confusion with the same "double jet" patterns related to heatwaves by recent work (e.g., Rousi et al. 2022). Consider avoiding this terminology or explaining it briefly with respect to the concept of "AWB+CWB".

We agree that this terminology may cause confusion. The following phrase was added for clarification, as well as the discussion in Appendix A:

"Cluster 2 MCs are denoted by a unique combination of AWB and CWB simultaneously deforming the same PV streamer, that extends from the anti-cyclonic shear zone of the sub-polar jet to the cyclonic shear zone of the sub-tropical jet (Figure A1)."

Lines 267-269: interesting, indeed. Are these cyclones related to cut-off lows? There is plenty of literature connecting Rossby wave breaking to precipitation extremes in the subtropics (e.g., the work of Andres de Vries), maybe it would deserve a citation to contextualize a result that would look otherwise quite "off".

We thank the reviewer for providing this relevant reference, which was added to the main text:

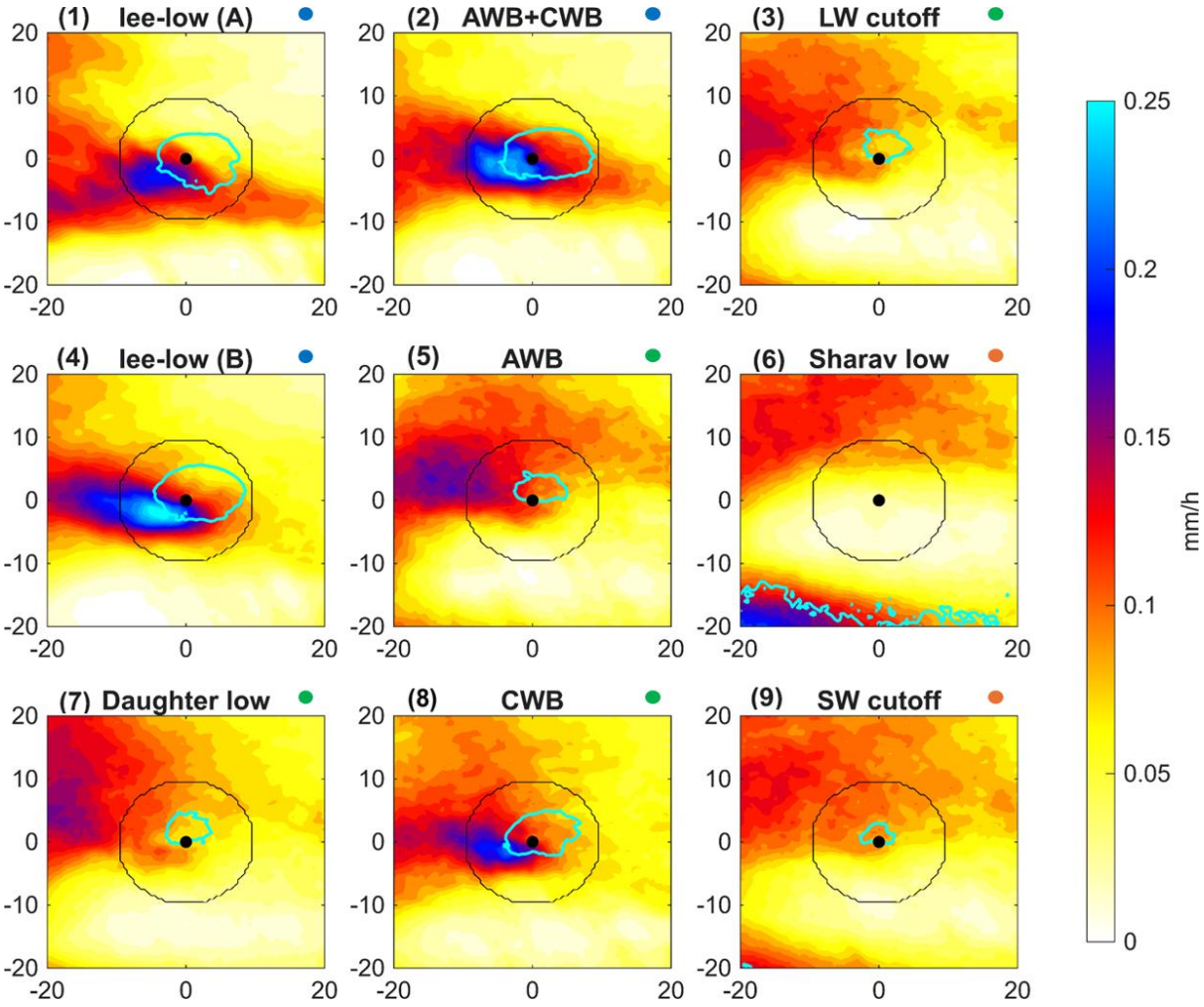
"The tight connection between RWB patterns and extreme precipitation were recently highlighted by de Vries (2021), especially for semi-arid areas such as the eastern-Mediterranean."

Figures 7,8, and lines 306-307: consider depicting with (1) significant upward trend and with (-1) significant downward trends in quantities to further improve readability and allow to assess immediately the contributions of individual P and E trends to P-E.

While we understand that such a presentation can be more intuitive, we wish to avoid showing "negative significance", which may also be misinterpreted or confusing. We therefore decide to settle for the binary result of the MK test, followed by the slope of the trend with the correct sign for positive and negative trends, which we hope comes across more clearly.

Lines 311-314: would it be possible to highlight in some way, in some of the plots, which clusters are the winter and summer ones? This would avoid again the reader the trip to Givon et al. (2024a) or to a Supplementary Information (that this paper, miraculously, does not have yet).

We thank the reviewer for raising awareness of this issue. The seasonal distribution of the clusters is now included in Appendix A, Figure A1. Figure 3 was changed to highlight cluster-dominant season:



New Figure 3: cyclone-centered cluster composites of E (shading) and P (cyan contours, 0.25 mm/h) at classification time (minimum SLP time of each MC track). The black circles denote the 10° radius impact area considered for cyclone-induced fluxes. Colored dots indicate the dominant season for each cluster: blue for winter, orange for summer, and green for spring/autumn.

Table 1: the first column tells that Mediterranean cyclones (at least in the classification used here) only occur in 13.4% of the considered days, correct? But still, they contribute to 24.1% of the P-E in the basin. Wouldn't those numbers be a bit low, especially for what concerns the precipitation?

This is a subtle point that we now clarify further in the captions of Table 1: "*Note that the spatio-temporal frequencies include the fraction of MC-affected grid-points to all grid-points in the domain, as well as their mean temporal frequencies.*".

Thus, Table 1 quantifies the total spatial and temporal frequency of MCs corresponding to the fraction of days and of total area that is affected by an MC. Therefore, these numbers suggest that the contribution from MCs to the MHC roughly doubles their spatio-temporal frequency. We do not interpret these numbers as "low", even for precipitation, since many other processes can still contribute to this year-round, long-term, Mediterranean-wide analysis. Still, the large difference between frequency and MHC contribution is consistent with the large impact of MCs on the regional MHC.

Lines 340-342: would it be possible to see analogous composites of radiative and turbulent fluxes comparing a OHC-enhancing and a OHC-depleting cluster? This analysis might complement and show nicely this interesting feature of summer cyclones.

We thank the reviewer for pointing out this interesting prospect. It is possible, and the authors intend to further decompose the MHC and OHC responses to MCs and their variability into the different components in a future study dedicated to the coupling between MC-induced fluxes and the OHC. We find this elaboration deserves its own publication, and beyond the scope of the current manuscript.

Line 360 (and see my comment about Table 1): here values around 50% and 70% are mentioned, it sounds a bit confusing. Please consider specifying (here or at Table 1) the values with respect to which those percentages refer.

The following clarification was added: "(locally, see Fig. 2)" in addition to the clarification in the Table caption.

Line 380-382: the general linkage between nonlinear PV dynamics and OHC is not immediately obvious, could the authors further elaborate on it?

This link stems from the dominance of nonlinear PV dynamics on the MC clusters under discussion. Following comments from both reviewers, the sentence now reads: "We show that certain MCs, primarily of RWB patterns and cut-off lows, may even add heat to the Mediterranean instead of extracting it, stressing the importance of non-linear PV dynamics to the OHC."

Line 384: this is the first time that the "Mediterranean precipitation paradox" is mentioned here, what is it? Please introduce this (seemingly complex) conundrum in the Introduction.

This phenomenon is addressed in the introduction, L 55-58 in the revised manuscript: "This phenomenon is often referred to as the "wet gets wetter - dry gets drier" mechanism, or the "Mediterranean precipitation paradox", often explained by a rise in MC-associated P due to increased water-vapor uptake caused by the rise in temperatures under global warming, and a drying effect as MCs grow less frequent in the southern and eastern Mediterranean due to the meridional shift of the Atlantic storm track (Chericoni et al., 2025)."

Lines 389-392: please give more details here or in the Introduction about the problem of this "all-quartile increase", which might not need be known to the reader.

The phrase now reads: "The impact of MCs on the MHC agrees with the trends of total regional P derived by André et al., (2024). The "all quartiles increase" pattern, describing an increase in all precipitation quartiles (i.e., extreme and moderate values alike, not restricted to the occurrence of MCs) suggested for the European continent can be related to the increased P of clusters 1 and 4 often affecting the area. However, the decrease in clusters 2, 6 and 9 partially explain the reduction in P intensity across the central, eastern, and southern parts of the domain."

Line 394: "Each driver", do the authors mean "Each cyclone type", or something more than that?

The sentence now reads: "Each cyclogenetic process responds differently to warming, leading to opposing influences on both the MHC and OHC", referring to the dominant processes denoted by each cluster (thermal lows, daughter lows, lee-lows (A) and (B), AWB, CWB, etc.)

Line 395: "warming buffer": this term is introduced here for the first time, do the authors mean that MC act to counteract global warming, in the sense of reduction of temperature?

At the basin scale MCs primarily act as heat-extractors from the ocean, and generators of excess precipitation, as discussed in the manuscript. Both of these contributions act to indirectly mediate regional warming.

Line 396: this is the first mention of "nonlinearity", to what nonlinearity do the authors refer? I am rather sure that you would like to bring forward an interesting point, but I encourage them (see also previous comments) to outline

their reasoning in a slightly less minimalistic way also for readers who are not familiar with MHC-specific jargon or issues.

The sentence was altered to read: “Moreover, changes in MCs impact on the MHC could develop nonlinearly in the future, as shifts in both frequency and intensity align with ongoing warming for several MC types. E.g., winter cyclones do not only drop in frequency or intensity – they are being replaced by MCs with an opposing sign of MHC impact.”

Technical/Typos

Line 57: “grow less” -> “become less”

Changed as suggested

Line 79: “are associated”

Changed as suggested

Line 90: “were revealed” in the aforementioned studies, or “are/will be revealed” in the current study?

Changed to read “are revealed”, referring to the current manuscript.

Line 104: in which sense “impair the climatological role”?

We are referring to the known ability of MCs to cool the ocean’s surface. The sentence now reads: “Revealing subtle changes in the heat extraction capacity of MCs is important, as these changes may compromise their climatological role as cooling agents in the oceanic system”

Line 215: consider adding short sub-titles next to (a) (b), etc… to increase readability of the figure.

Sub-titles were added