Response to Referee 1

We thank the referee for taking the time to review our manuscript. Please, find below your comments (CX) and our answers (AX), the latter highlighted in red.

Update:

In the submitted paper there was an error on how we computed ERA5 and ERA5 ensemble total precipitation, ERA5 convective precipitation, ERA5 large-scale precipitation and ERA5 snowfall. The error was that we computed the daily total amount by simply summing 6-hourly time-steps, instead of summing all the 1-hourly time-steps for ERA5 and 3-hourly steps for ERA5 ensemble. Therefore, we rerun all the scripts and updated all the figures with the correct datasets. The procedure used to compute the new precipitation datasets was the following: i) shift back by 1 hour (3 hours for ERA5 ensemble) the time-steps; ii) sum the 24 (8 for ERA5 ensemble) values at daily resolution. Our results did not change significantly compared to the submitted paper, except for convective and large-scale precipitation (Figure S8 in the submitted paper) since now both datasets show statistically significant results (see new Figure S8_rev below). Therefore, we removed the old Figure S8 but computed the same for convective available potential energy (CAPE, JKg^-1). Results for CAPE (see Figure S8_new) show significant positive anomalies over the Alps during JJA and therefore we link the wet anomalies (Figure 4e) with localised convective P events. In the revised paper we replaced the old Figure S8 with Figure S8_new.

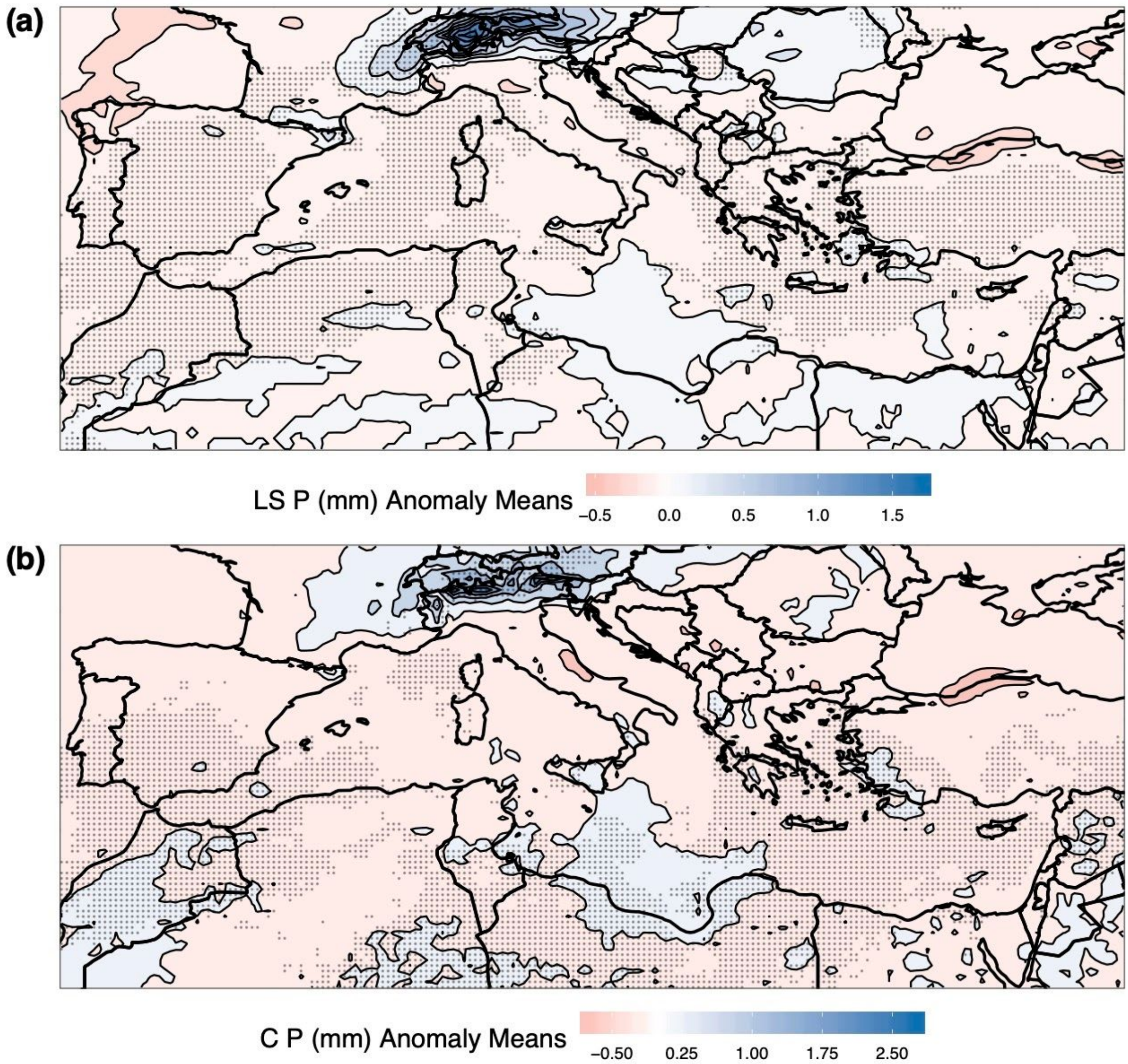

Figure S8_rev - As Figure 4e but for daily anomaly means of (a) large-scale total precipitation (mm) and (b) convective total precipitation (mm).

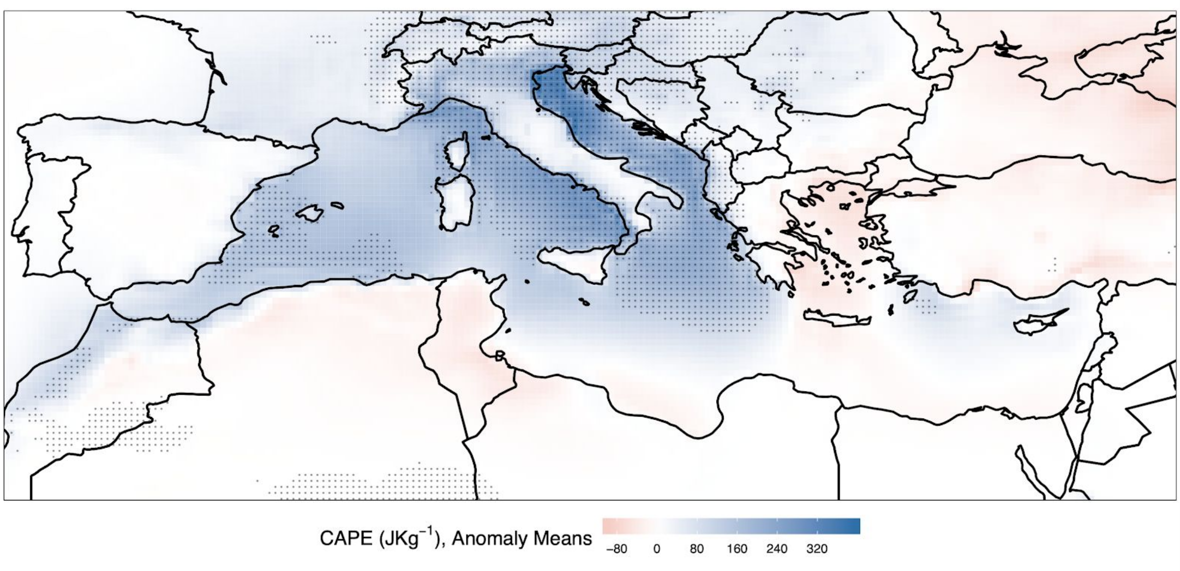

Figure S8_new - As Figure 4e but for daily anomaly means of convective available potential energy (CAPE, JKg^-1).

The authors investigate compound hot-dry and wet-cold events over the Mediterranean basin, employing a novel method based on dynamical systems theory. They use different reanalysis products and find a tendency towards an increasing coupling between temperature and precipitation over 1979-2018. The paper is well written and pleasant to read. I find the use/introduction of this approach interesting given that it offers a novel perspective for studying compound events. New approaches are always welcome as they can challenge or confirm previous findings. I recommend publishing the paper, but also to address some comments that follow. All of the comments are, of course, meant be constructive.

Thank you, please find our answers below.

C1: A main comment I have is about the definition of hot&dry and wet&cold conditions. I understand that they are defined based on positive/negative seasonal anomalies of precipitation and temperature. Although also non-extreme values of the contributing variables can lead to extreme impacts, the employed anomalies may be be particularly small. Given that the authors link the study directly to compound events and associated risks in the Mediterranean area, I think that some considerations are required. The authors could repeat some of the analyses based on higher anomalies (see comment below). Alternatively, I would recommend modifying the text in several parts, including abstract (e.g., line 7) and title, to avoid giving the impression of referring to, e.g., hot (and therefore extreme) events. In general, when possible some more physical interpretation would be welcome to guide the reader.

A1: We agree that within the text there is too much emphasis on extreme events, while we just considered compound anomalies. As suggested, we modified L7 and amended "hot-dry" with “warm-dry” (title included) in the revised paper. Regarding physical interpretation of our results, we identified and described within the text two main physical processes: i) thermodynamic changes driving the increasing alpha and persistence trends during summer; and ii) the formation of a Cyprus-low over the Eastern MED during DJF. In addition to this, in the revised paper (Section 4.2) we added the physical interpretation of wet P anomalies during JJA over the Alps, which we now link to convective P events (see **Update** at the start of this file and A21). We also mention (Section 5) that JJA SLP patterns may be a combination of different weather regimes (see also A20).

Specific comments

C2: L40, The paper from Manning et al. would definitely fit here (see also comment later): Manning C, Widmann M, Bevacqua E, Van Loon AF, Maraun D, Vrac M. Increased probability of compound long-duration dry and hot events in Europe during summer (1950–2013). Environmental Research Letters. 2019 Aug 29;14(9):094006.

A2: The study is indeed relevant and a reference to it has been added in Section 1 of the revised paper.

C3: L42, I would strongly suggest adding 2-3 sentences in this paragraph. You could explain, via examples and references, why wet-cold and dry-hot events can lead to impacts (e.g., wildfire, vegetation issues etc), i.e. why they are important.

A3: As suggested, two sentences have been added in Section 1 of the revised paper highlighting hot-dry and cold-wet impacts.

C4: From line 61 onward. Overall, the explanation is very easy to understand. However, would it be possible to add 1-2 equations to guide some type of readers?

A4: We added the derivation of the metrics in Section 2.1 of the revised paper.

C5: L68, Does the persistence depend on any used threshold to define the close range dx (intorno) around Zeta_x? Or is it propriety of the system in Zeta_x that you somehow obtain based on some limits? Please, clarify.

A5: Yes, the persistence (and co-persistence) metric quantifies the mean residence time of the recurrences around a given state of interest Zeta_x (Zeta). Therefore, the persistence - as well as others dynamical indicators introduced in the manuscript - depends on the threshold (q) used to compute the recurrences. Such threshold refers to the size of the radius of the hyper-ball centred on Zeta_x. The higher the threshold q, the smaller the radius of the hyper-ball. In Section 2.1 of the revised paper we clarified this. The dependence of the results on the specific threshold used is discussed in several articles in the preceding literature (Faranda et al. 2011 J Stat Phys, Lucarini et al. 2014 J Stat Phys, Faranda et al 2017 Sci. Rep, Faranda et al 2019 Nature Comm). In this and those studies q=0.98 value was used. This value has provided good estimates of the dynamical indicators for two reasons: on one hand it is high enough to ensure to select only genuine recurrences of Zeta, on the other it ensures a sufficient large sample of recurrences to perform statistical fits. In previous studies little sensitivity of the results to the threshold is found in the range $0.95<q<0.99$.

C6: L72, Also for the co-recurrence ratio: is this obtained based on (empirical) counting of the states and therefore it depends on the values dx and dy used to the define the close range around Zeta_x and Zeta_y? I agree that a full description of the theory should not be given but, in my opinion, a few sentences to guide the reader are needed here.

A6: Yes, the co-recurrence ratio is quantified by considering the joint number of recurrences between variable_x and variable_y. As for persistence, to compute recurrences we make use of thresholds for both variable_x and variable_y. Note that the thresholds for x and y are not the same, as we decided to used a fix a quantile of the recurrences for each variable. The same set of thresholds is used to compute persistences. In Section 2.1 of the revised paper we provide the derivation of the co-recurrence ratio.

C7: “Compound dynamical extremes”, would it be better to use compound dynamical events? “Extremes” might be misleading.

A7: We appreciate the suggestion, however we would like to keep “compound dynamical extremes”, since both recurrences and high-alpha values are computed by applying a high-quantile threshold to the time-series, and indeed their calculation issues directly from extreme value theory.

C8: L92, anomalies relative to JJA means

A8: We amended the text as suggested in the revised manuscript for both JJA and DJF definitions (Section 2.2).

C9: L96, Could you please explain why these slopes are preferred to usual linear regression. For example, would the trend in Fig. 1c-d be non-significant with a linear regression? Please, discuss this.

A9: We make use of the Mann-Kendall test to compute slopes and p-values because of its robustness to outliers and suitability for non-normal distributions. However, we tested some of our trends with linear regressions and confirm that both slopes and p-values reflect the ones computed using the Mann-Kendall test. Indeed, for JJA alpha trend (i) ERA5 (Figure 1a), (ii) ERA-Interim and (iii) ERA5 ensemble (Figure S1a) we find the following slopes and p-values with linear regressions: (i) $p<0.01$ slope=0.001; (ii) $p<0.01$ slope=0.0008; and (iii) $p<0.01$ slope=0.0008.

C10: L110, Fig. 1a, Are you computing alpha for every JJA day and then computing the yearly average? Make this clearer, please (in the caption is not fully clear in my opinion).

A10: Yes, alpha (and persistence) has been computed first for the entire period 1979-2018, then filtered by JJA days and eventually JJA yearly means were computed. We amended the caption of Fig. 1a in the revised paper.

C11: L110, Guiding the reader to see what is happening in the (T,P) space would help here. For example, in summer, would you expect to find a similar trend using Tmin and P (or, in winter, Tmax and P)?

A11: Yes, similar trends are observed in winter when computing alpha with Tmax. Indeed, in a very early stage of the analysis we computed alpha for Tmin and P during JJA and found similar trends as for Tmax and P (see Figures R_1 and R_2). However, we specifically selected Tmax in summer and Tmin in winter to reflect our desired compound events (i.e. warm-dry and cold-wet). Such similarity between trends may be due to temperature, which, in both Tmax and Tmin cases is continuing increasing over the MED. We expanded the relevant sentence in the revised paper (Section 3).

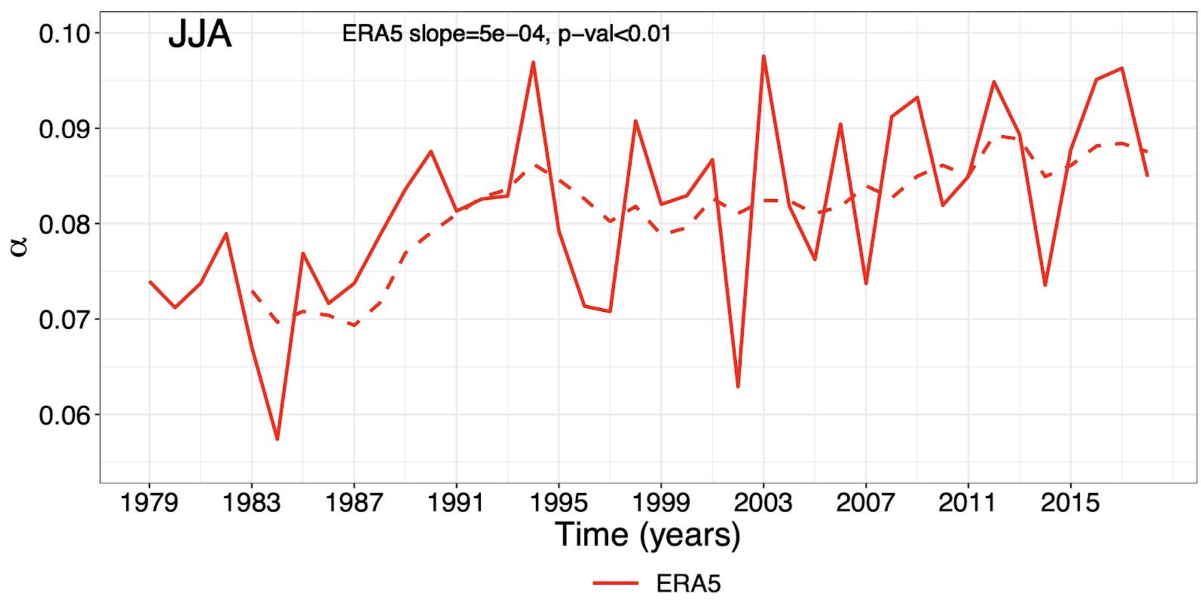

Figure R_1 - Alpha JJA trends computed using Tmin (instead of Tmax) and P for ERA5 reanalysis. Compare the figure with Figure 1a in the main paper.

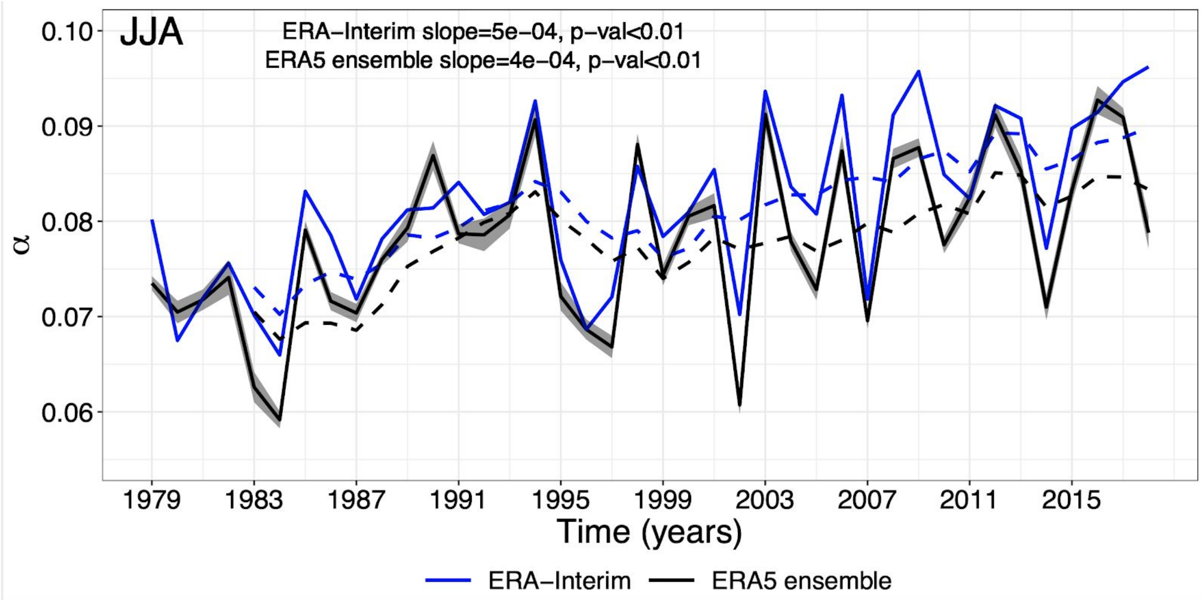

Figure R_2 - Alpha JJA trends computed using Tmin (instead of Tmax) and P for ERA-Interim (blue) and ERA5 ensemble (black) reanalyses. Compare the figure with Figure S1a in the main paper.

C12: L112, Would in any way carrying out the analysis after detrended the time series of the temperature help to better understand the physical drivers of the trends?

A12: Thanks for the comment. We opt for not showing such type of analysis since the detrending distorts the dynamical system's recurrences and would not satisfy the statistical assumptions underlying our approach. What we instead did was to explain the summer trends by plotting alpha and persistence ranked in ascending order by average Tmax over the 1979-2018 period.

C13: L 120, is there a correlation between co-persistence and alpha?

A13: We confirm that the correlation between JJA ERA5 alpha and persistence is positive (rho=0.75, Spearman's test) and significant ($p<0.001$). We mentioned this in Section 3 of the revised paper.

C14: L121, how are, in this regional case, hot and dry days subsampled?

A14: Here, the persistence trends have been computed *for each grid-point* and then averaged by considering only persistence daily values recorded during compound warm-dry events, instead of using the full time-series. We clarified the sentence also according to C2 by Referee 2 (Section 3).

C15: Section 4.1, Do also the univariate persistences show a similar seasonality? If so, is it possible to interpret this in relation to the seasonality in alpha?

A15: Thanks for the comment. We checked the seasonal trends of univariate persistences (Figures R_3-R_4) and co-persistence (Figure R_5) extremes (>90th quantile) for all three reanalysis products by making use of total precipitation (mm) and maximum temperature (K) data. We note that none of these plots agree with Figure 3a, and hence we do not find a marked and distinct peak in persistence extremes during July and August months, except for precipitation (Figure R_3), which may be linked to the prevailing dry conditions over the region. We added a sentence referring to this in Section 4.1 of the revised paper.

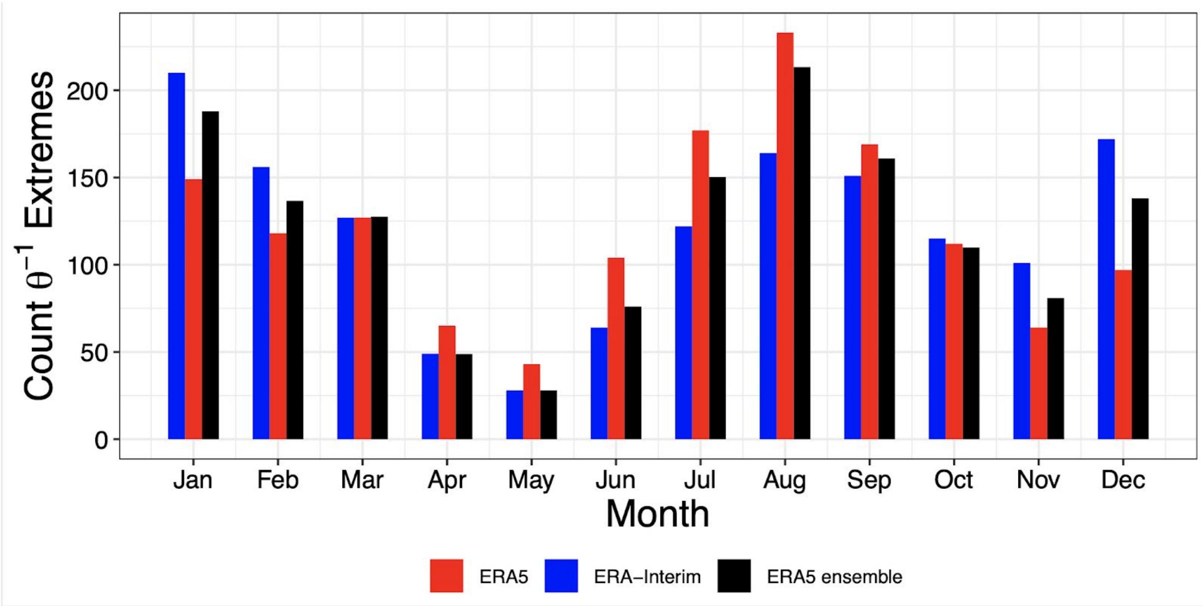

Figure R_3 - Univariate total precipitation (mm) persistence extremes (>90th quantile) computed for the three reanalysis products within the 1979-2018 period.

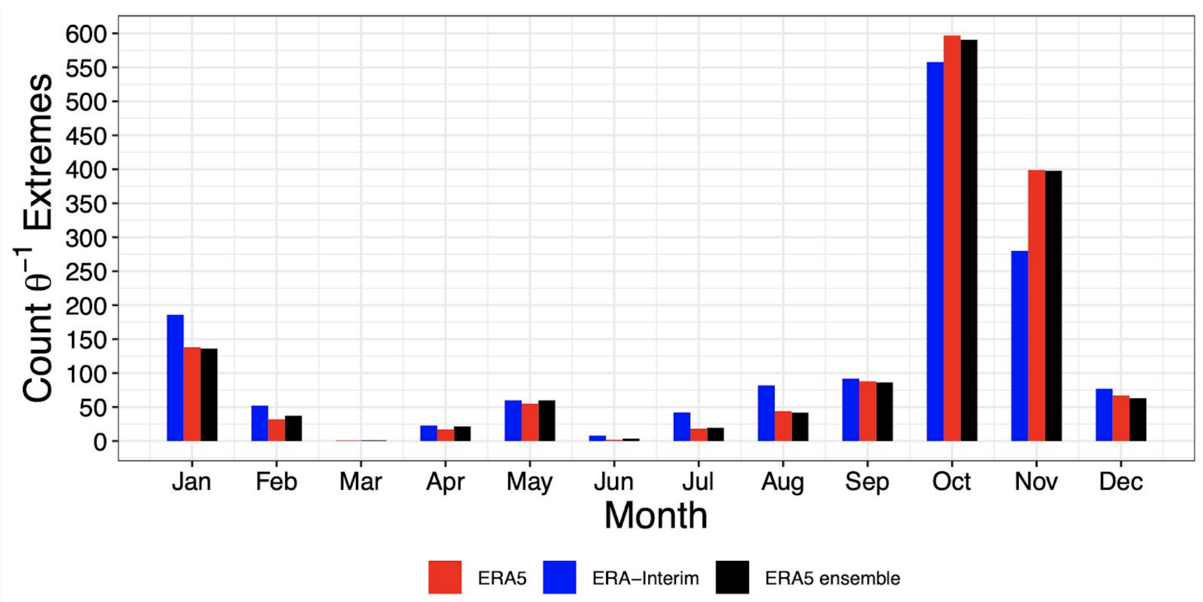

Figure R_4 - Univariate maximum temperature (K) persistence extremes (>90th quantile) computed for the three reanalysis products within the 1979-2018 period.

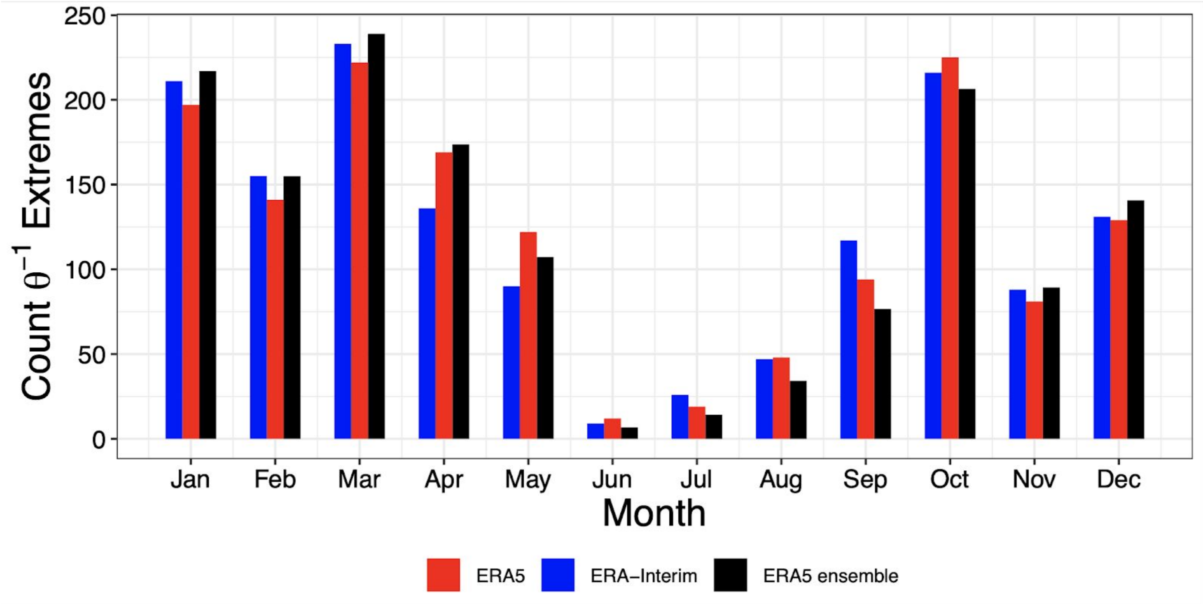

Figure R_5 - Co-persistence extremes (>90th quantile) computed for the three reanalysis products within the 1979-2018 period by making use of total precipitation (mm) and maximum temperature (K) data.

C16: Section 4.2 and the following sections. The authors could consider whether moving these results before the trends would help or not. Being aware of what alpha depicts from a physical point of view may help to interpret the trends more easily.

A16: Thank you for the suggestion. We would like to keep the structure of the paper as it is now. The reason is that Section 3 shows trends only for JJA, whereas Sections 4.2-4.4 show both JJA and DJF and therefore the overall flow of the paper may be disrupted if we were to move these sections before Section 3.

L141 (paragraph)

C17: 1) In general, a discussion on why one would expect to capture anomalies would be important to help the reader.

A17: At the end of Section 5 we indeed discuss the link between CDEs and compound events. However, to strengthen our point a new sentence has been added at the end of Section 5 of the revised paper, stating that “a link between CDEs and compound events is expected because in both cases the data reflect anomalous (or high-coupled) conditions for the same atmospheric variables”.

C18: 2) I understand that the anomalies are computed relative to the seasonal mean, please specify it.

A18: In Section 2.2 of the revised paper we made clear that anomalies have been computed from seasonal means.

C19: 3) To strengthen the conclusions, you could highlight that small anomalies are expected over the sea due to water inertia, even during heatwaves.

A19: Thanks for raising a good point. A sentence specifying this has been added in Section 5 of the revised paper.

C20: 4) Overall, the SLP field picked up by high alpha values in winter appears associated with a more defined atmospheric configuration compared to summer. Is it possible that you pick up different weather circulations within the subsample of extreme alpha in summer? - I see you have stippling in Fig. 4. How large is the average anomaly during days with large alpha compared to the standard deviation*? *computed based on the daily anomaly data. - How do these maps change when using, e.g., the 95th percentile to define extreme alpha?

A20: Yes, during winter the SLP synoptic patterns clearly reflect a well documented phenomenon over the eastern MED (i.e. the Cyprus-low), whereas the SLP patterns in summer do not match any large-configuration we know of. Therefore, it may be indeed possible that alpha extremes in summer capture a different set of weather regimes. Before submitting the manuscript, we tried to investigate the matter with a Self Organising Maps (SOMs) analysis. However, the SOMs results did not point to any plausible physical mechanism, so that we do not show them. Nonetheless, we now specify in the revised manuscript (Section 5) that results obtained for summer might show a composition of different weather circulations that we are unable to isolate clearly. Please find below in Figure R_6 the ratio between anomaly means and SDs corresponding to Figure 4. We note that the JJA season displays similar ratio values, albeit with different spatial patterns, when compared to DJF.

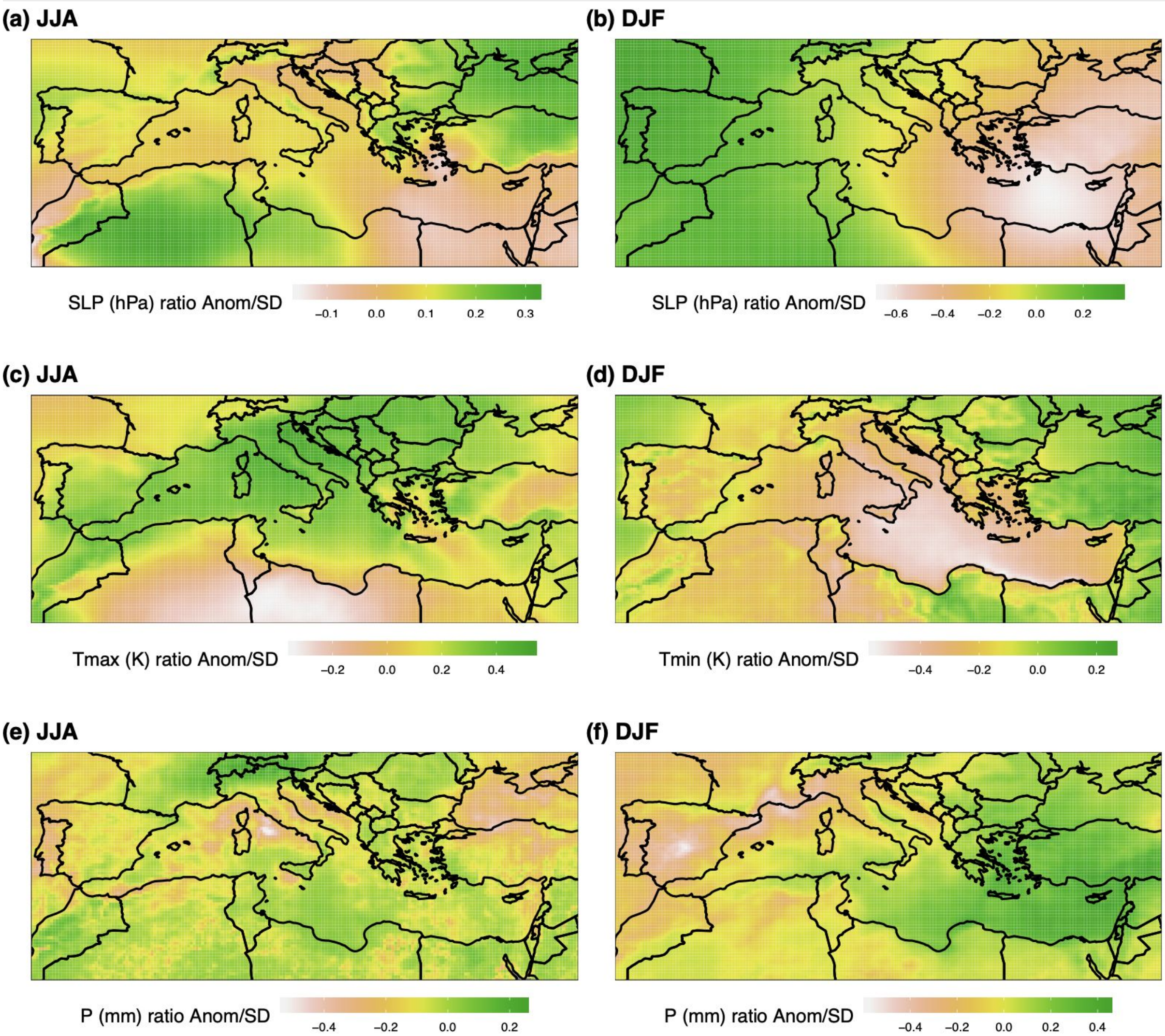

Figure R_6 - As Figure 4 in the main manuscript but for the ratio between anomaly means and their SDs.

Figures R_7-R_9 below, reproduce Figures 4, S7 and S9 of the submitted paper but with anomaly means computed from alpha extremes >95th quantile. In general, the figures are in agreement with the ones in the submitted paper for all variables (i.e. SLP, Tmax, Tmin and P), seasons and reanalysis products. The only difference are the anomaly values which in Figures R_7-R_9 are larger compared to Figures 4, S7 and S9 (see colorbars), but this is somehow expected since we now compute anomaly means based on CDEs derived from a higher threshold (95th quantile). In Figure R_10 we also show the ratio between anomalies and SDs of Figure R_7. As you see the ratio spatial patterns between Figure R_6 (alpha>90th quantile) and Figure R_10 (alpha>95th quantile) are in agreement, although the ones in Figure R_6 are weaker. This suggests that our main results are not overly sensitive to the alpha threshold used. We added new sentences mentioning this in Section 2.1 and Section 4.2 of the revised paper (see also A4 of Referee 3).

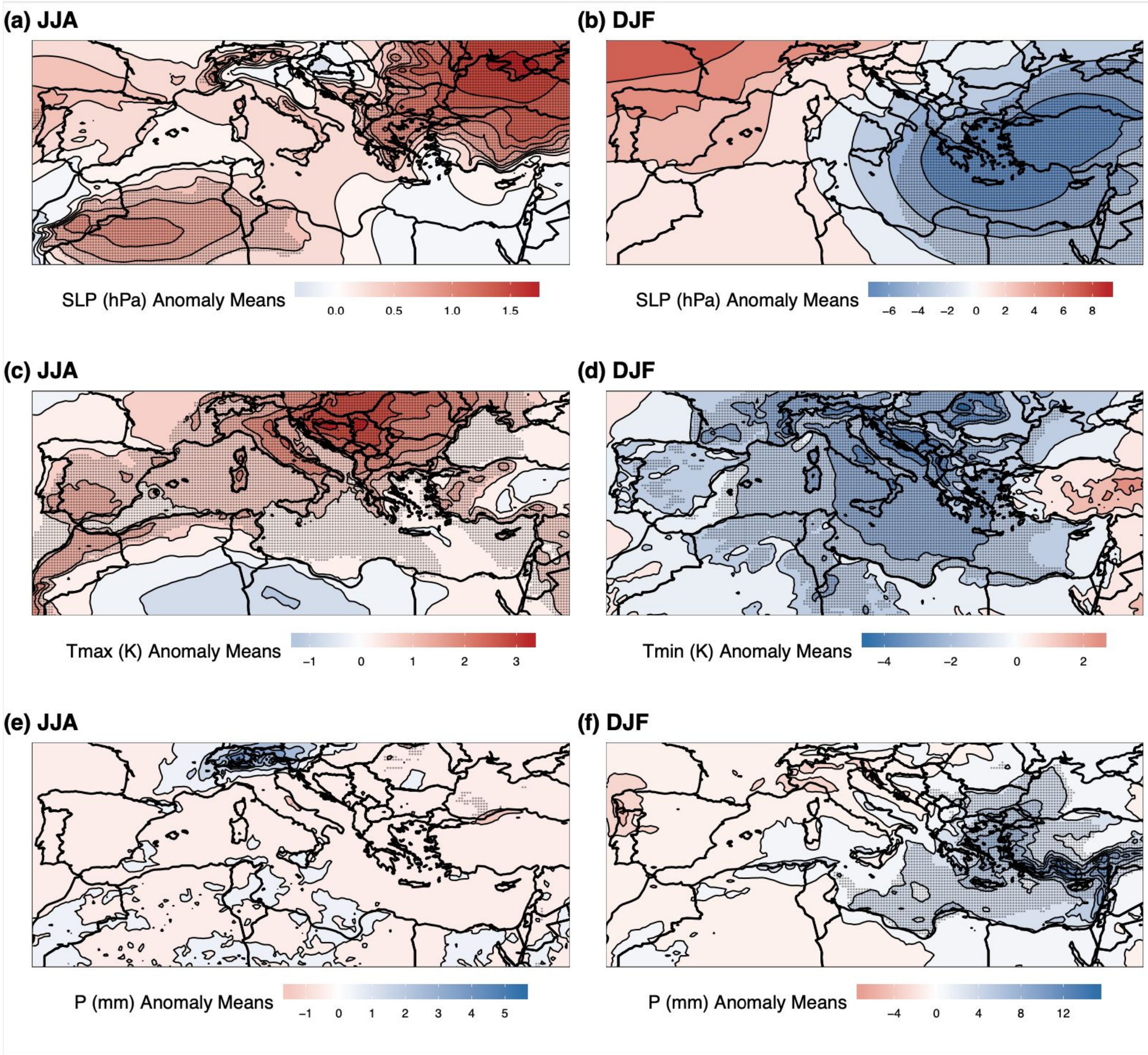

Figure R_7 - As Figure 4 but for alpha extremes > 95th quantile.

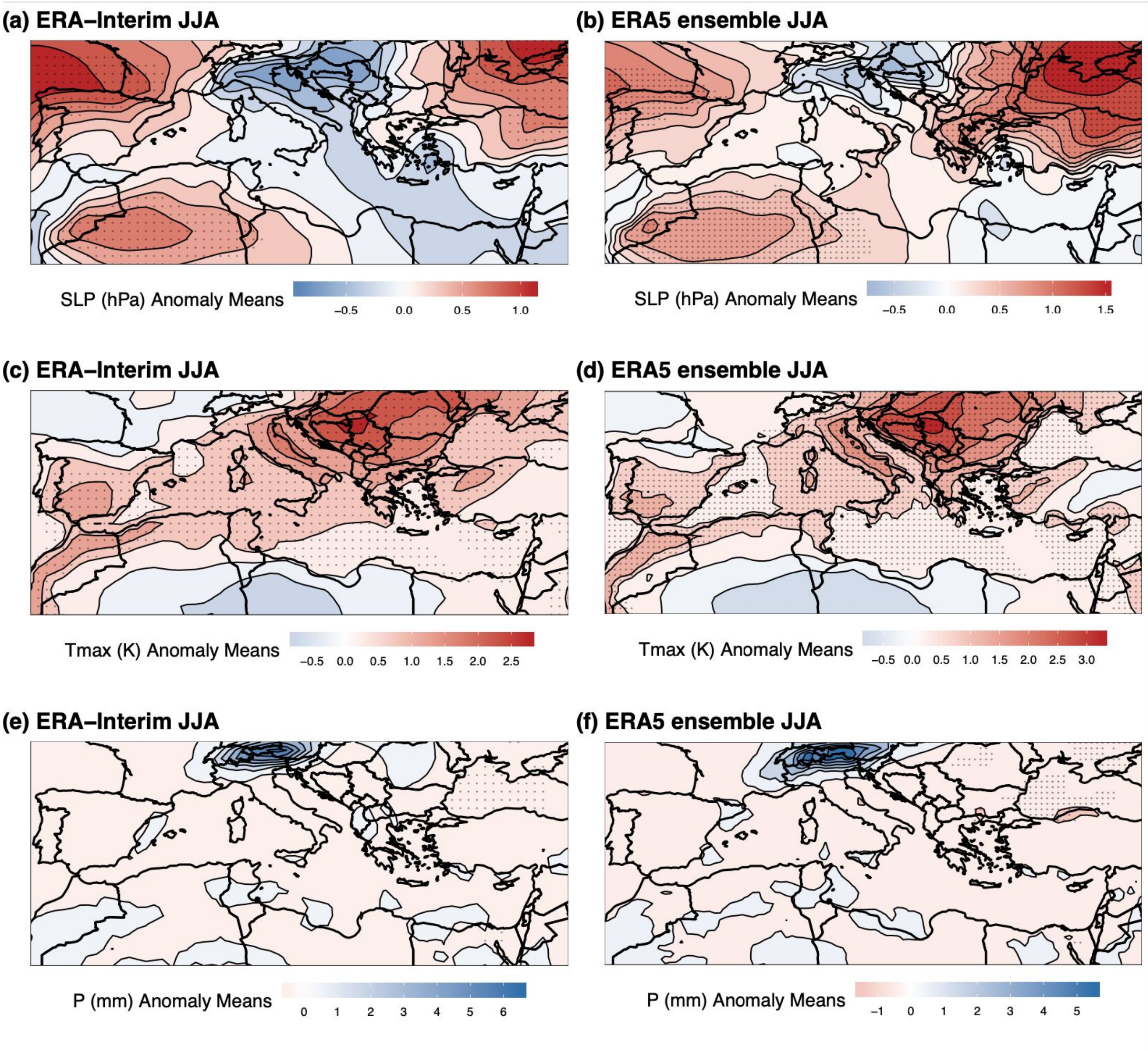

Figure R_8 - As Figure S7 but for alpha extremes > 95th quantile.

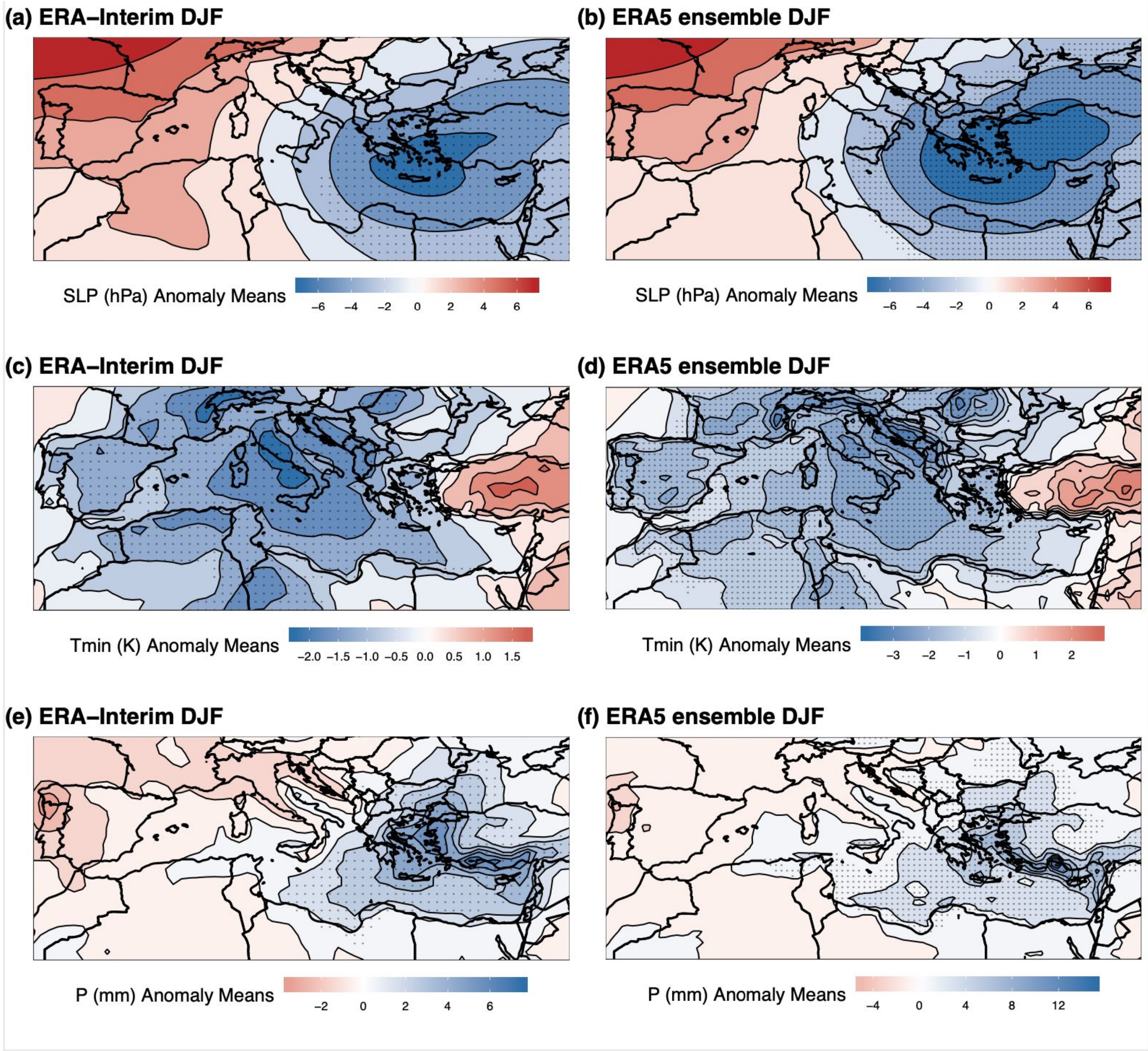

Figure R_9 - As Figure S9 but for alpha extremes > 95th quantile.

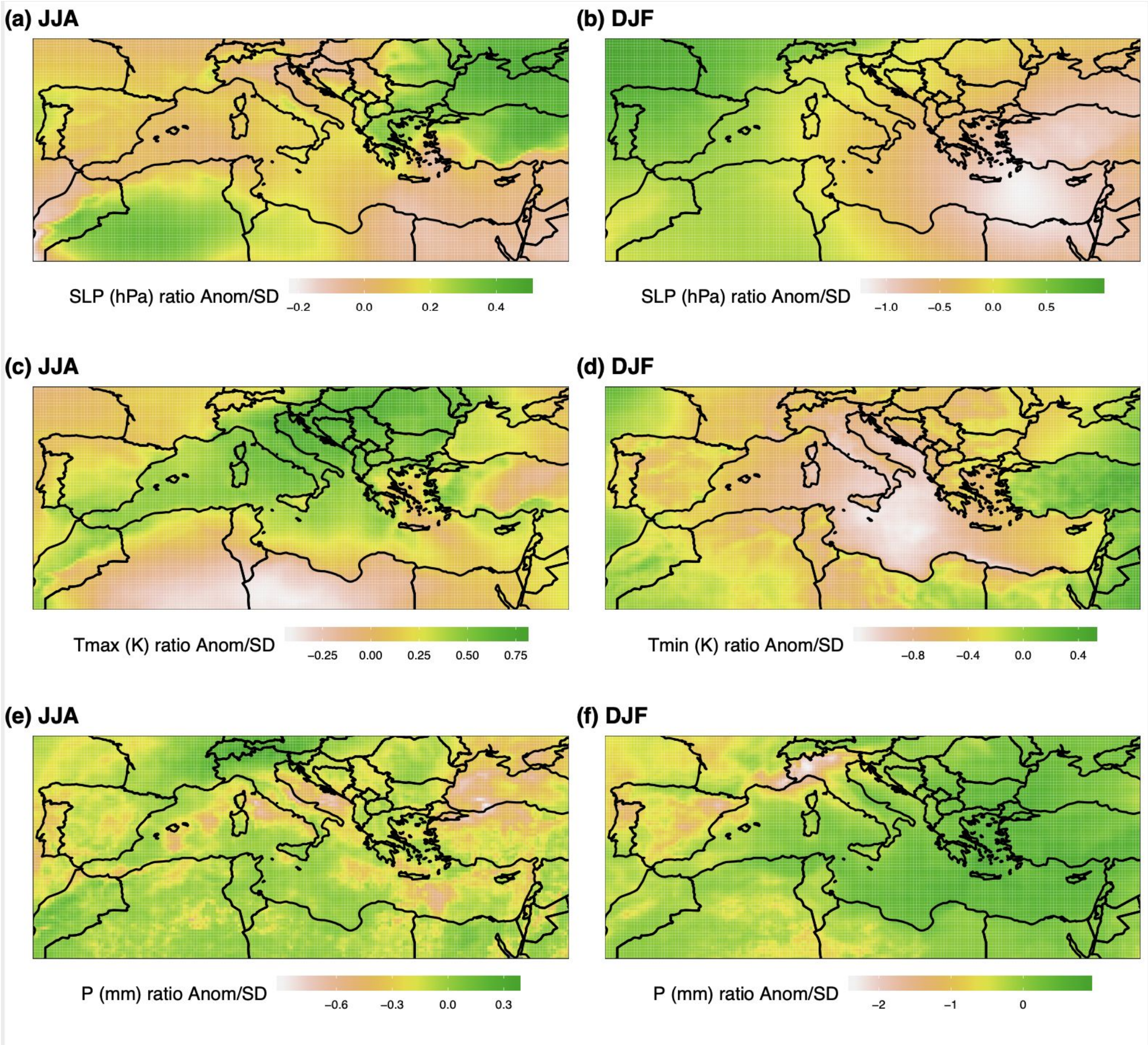

Figure R_10 - As Figure R_6 but for ratios computed from alpha extremes >95th quantile.

C21: 5) L146, The latter correspond…: Personally, I would rephrase given that although the significant anomaly is all associated with the large-scale component of the precipitation, also the convective part is relevant. The reasoning in the next sentence would still work, maybe saying these anomalies are *mainly* linked to SLP.

A21: We removed the sentence in L146-147 along with Figure S8 (see **Update** at the start of this file). In addition, we computed ERA5 JJA anomaly means of convective available potential energy (CAPE, JKg^-1) occurring during alpha extremes and found positive and significant CAPE anomaly means over the Alps (Figure S8_new). Therefore, we conclude that the JJA wet P anomalies observed over the Alps (Figure 4e) are driven by localised convective P events. In the revised paper we replaced the old Figure S8 with Figure S8_new and added a sentence specifying the findings in Section 4.2.

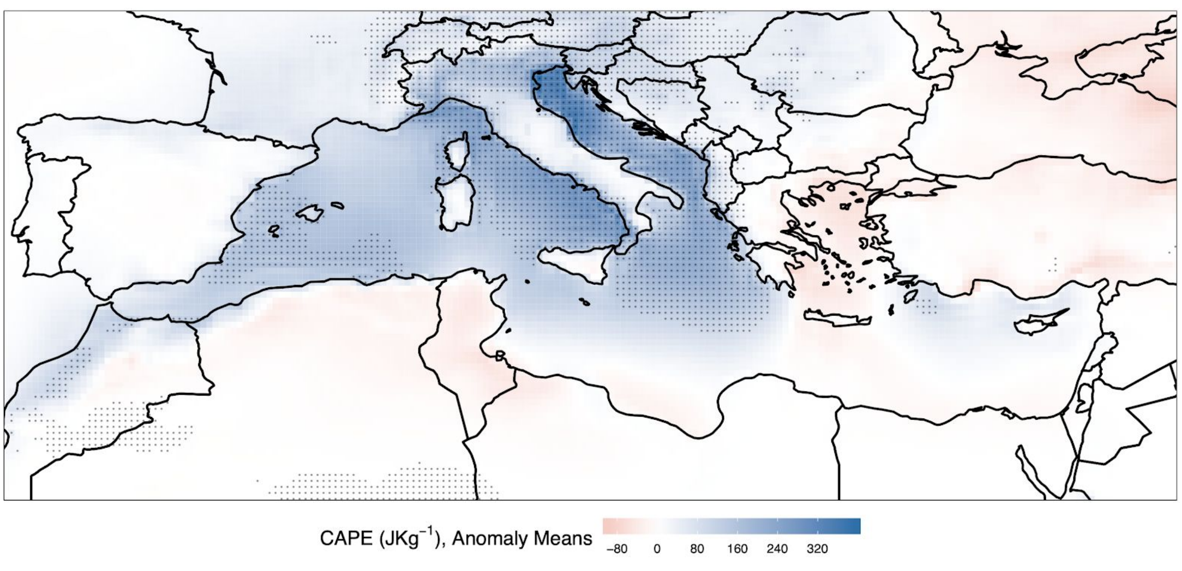

Figure S8_new - As Figure 4e but for daily anomaly means of convective available potential energy (CAPE, JKg^-1).

C22: L158, The sentence is correct. But it suggests that these events can occur everywhere over the analysed domain, while, especially the P anomalies, suggest that this is the case mainly over the eastern domain and along the Italian areas exposed to cold-air advection from northern Europe.

A22: We clarified that CDEs match compound cold-wet events “over the eastern MED” in the revised paper (Section 4.2).

C23: L160, I understand that hot-dry and cold-wet events are defined based on positive/negative anomalies from the seasonal average. Would the main conclusions be similar if using larger anomalies to define, hot/cold and wet/dry conditions? For example, one could use +/-2 standard deviations from 0 to define larger anomalies.

A23: Thank you for the comment. In the next revision (after Editor’s comments) we will try to replicate some of our figures with *extreme* anomalies as you suggested.

C24: L167, Could you relate to the numbers above, i.e., does this also imply that also the values 84% and 77% are significantly large?

A24: These percentage values reflect the cumulative distribution functions (CDFs) of anomaly means of interest for all grid-points (Figure 5a-b). In other words, 84% of Tmax anomaly means are warm (>0 K) and 77% of P anomaly means are dry (<0 mm). The same (but for cold-wet events) applies to DJF percentage values (Figure 5c-d). Here we checked the statistical significance between distributions of anomaly means during alpha and non-alpha extremes (Section 2.3 and 4.3) and found statistically significant p-values, therefore we intuitively expect that the percentage values are significant too. Note that in the revised manuscript the percentage values changed slightly because we made use of the correct ERA5 total precipitation dataset (see **Update** at the start of this file).

C25: L162, How are these numbers computed: Are all CDE days and grid points pulled together? Please, clarify.

A25: Please see comment C24 above.

C26: L168, consistently with Fig. 4f, the distribution of the precipitation is peaked around zero in fig. 5d. I am wondering whether (maybe for future work) the authors would see any added value in focussing some of these statistics only over the eastern part of the domain, where the framework is able to better capture anomalous conditions. If so, this could be discussed.

A26: Thanks for the comment and suggestion. It would be definitely interesting to compute the dynamical systems metrics only for the eastern MED subregion, since at least in DJF it is the one showing a clear and well documented synoptic pattern. At present, works by Hochman et al. partly address your question, but for future work we see our interests moving towards a larger-scale dynamical systems analysis instead of focussing on smaller regions. We specified this at the end of Section 5 in the revised paper.

C27: Section 4.4, It is a bit difficult to read the values in fig. 6 given that the palette has continuous values. Aren't these values depending on the percentiles (here 90th) used to define the CDE events? Therefore, the reader should be helped to interpret these numbers. They should be compared to what expected under a certain null hypothesis. For example, one could easily compute the probability of getting concurrent CDE and hot&dry days assuming that the CDE events are randomly distributed during the year (if this is a reasonable assumption).

A27: Thanks for your comment. As suggested, in the next revision (after Editor's comments) we will try to compute the statistical significance for Figures 6, S13-S14.

C28: Discussion: Could you add 1-2 sentences about the expected sensitivity of the results from the size of the analysed domain? This is relevant for the reader...

A28: These have been added in the revised paper.

C29: L190, Do you think that re-computing the trends in the two metrics obtained based on maps of (1) land surface only and of (2) sea surface only could somehow allow for speculating more safely about this? Or, more in general, could this allow for disentangling a higher signal of the increasing coupling on land?

A29: Yes, computing the dynamical systems metrics based only on land-surface (and/or sea-surface) data may help in providing more understanding about the physical processes at play during summer. Temperature-precipitation coupling may change significantly between land and sea, due to the very different thermal inertiae of the underlying surfaces, since in the former many components of the earth's surface affect the coupling (e.g. vegetation, orography, built environment and freshwater systems), whereas in the latter the Clausius-Clapeyron relation is followed with no (or little) disturbances. In the next revision (after Editor's comments) we will try to replicate Figure 1 for land- and sea-only data.

C30: I understand that the trends you found in the regional coupling can have different meaning depending on the areas of the domain so I understand that they should be interpreted bearing this in mind. Could you discuss more explicitly on this from an impact perspective?

A30: A sentence with a practical examples has been added in the revised paper.

C31: Around L205, I would suggest to interpret and discuss the results also in relation to results found by Manning et al. (paper cited above).

A31: A new sentence discussing the results of Manning et al. (2019) has been added in the revised paper.

C32: L215, see my main comment about the definition of hot days.

A32: We amended 'hot' with 'warm' throughout the text and in the title of the revised manuscript.

C33: The authors could consider expanding the discussion, very briefly, to highlights the potential benefits of their approach for the part of the compound event community that is focussing on impact assessments.

A33: We expanded the discussion in the revised paper. See also comment A30 above.

More technical comments

We thank the Referee for spotting these typos and small errors which had escaped us.

C34: L10, discussing first the winter results may allow avoiding some repetitions.

A34: Thank you. The abstract has been amended as suggested.

C35: L17, add space

A35: Done.

C36: L19, I would probably start talking of atmospheric circulation changes which are more intuitive than "dynamical changes" for a non-expert reader.

A36: The word has been amended as suggested.

C37: L20, may expect based on?

A37: "as a consequence of global warming". We amended the sentence.

C38: L27, an increase in "daily" or "episodic" precipitation extremes?

A38: To be more generic, we amended the sentence with "heavy precipitation events" in the revised paper.

C39: L56, "[...]. The metric theta^{-1}..."

A39: This has been added in the sentence.

C40: L79, each daily timestep

A40: "daily" added in the sentence.

C41: L80, with -> characterized by…;

A41: Amended.

C42: L81, could be written slightly better

A42: Thanks. The sentence has been rephrased.

C43: Fig.1, caption, L4, 5 year centered moving [...]

A43: "centered" added in the caption.

C44: L200, Please, check that it is ok. I assume you did not talk of Tmax *and negative P* anomalies on purpose.

A44: In the revised paper we clarified the sentence by adding "and negative P anomalies" when referring to JJA.

C45: L 211, it is probably more correct to write "concurrent cold spells and heavy…"

A45: "concurrent" has been added in the sentence.

C46: L212, "e.g" in the parenthesis

A46: Thanks. This has been corrected.

C47: L212, "accordance" a better word? It might not look in accordance with the decrease found in the other studies.

A47: We amended the word with “in line” to reduce the emphasis of the sentence.