Response to Referee 3

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:
After checking carefully the ERA5 datasets used in the analysis we found an error when computing total precipitation. We therefore corrected the error and created new figures. No significant changes, compared to the submitted version of the paper are observed, except for Figure S8 which has now been removed and replaced with Figure S8_new. Please see the **Update** description in "Response to Referee 1" for more details.

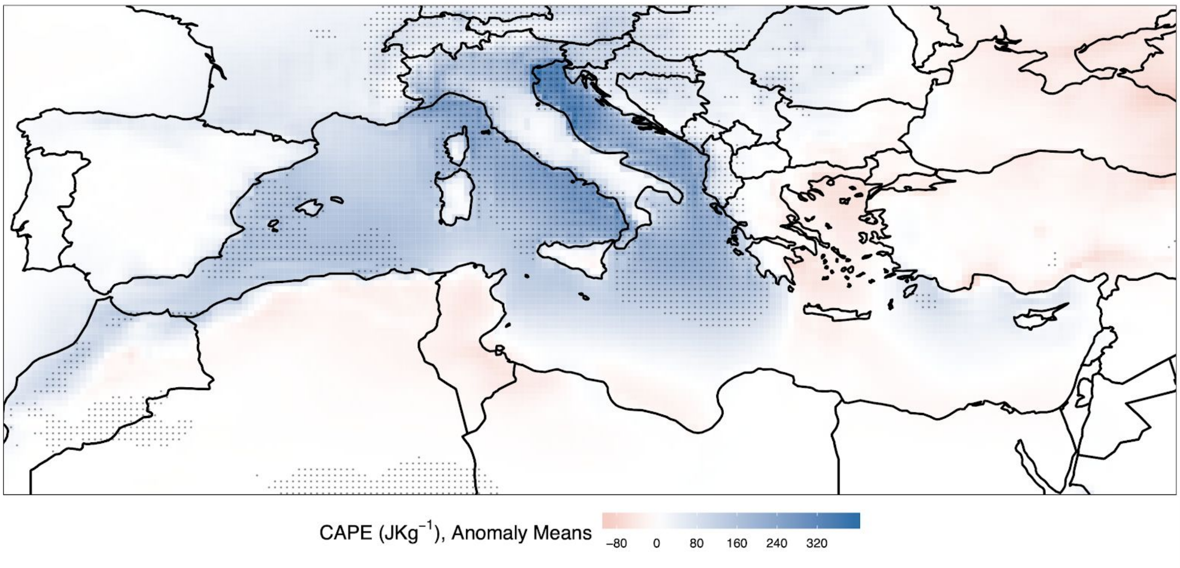

Figure S8_new - As Figure 4e but for daily anomaly means of convective available potential energy (CAPE, JKg^-1).

GENERAL COMMENTS

The authors analyse hot-dry as well as cold-wet dynamical extremes over the Mediterranean region in ERA reanalysis data sets over the period 1979-2018. They use a novel method based on dynamical systems metrics and extreme value theory to select and analyse so-called "compound dynamical extremes." The study is mainly based on two indicators, termed the co-recurrence ratio α and the co-persistence θˆ(-1). They estimate these indicators for joint occurrences of daily maximum temperature and total precipitation, as well as of daily minimum temperature and total precipitation. They define the events with α>90th quantile of the whole α distribution as compound dynamical extremes. The authors find a positive trend in the co-recurrence and co-persistence of hot-dry events during summer (JJA), whereas no trend can be found in case of the co-recurrence of cold-wet events in winter (DJF). Thus, they conclude that long-term warming strengthens the coupling between temperature and precipitation, leading to more intense hot-dry compound events. They also analyse spatial fields of sea level pressure, temperature and precipitation during compound dynamical extremes, as well as spatial maps of the co-recurrence ratio α.

The paper is well written, with a clear, fluent and concise language and a well organised structure. I think that this new method based on dynamical systems metrics can provide new insights into understanding the mechanisms behind compound events. Hence, my assessment of the manuscript is overall positive. However, I have to point out some deficiencies, which need to be fixed before publication:

Thank you, please find our answers below.

C1: 1. The computation of α and especially of θˆ(-1) is not described clearly and precisely, and I think it should not be substituted by merely a reference to another publication. The manuscript should contain the basic equations for the two main indicators (at least in the supplement) since these represent the core of the whole analysis. To assure the reproducibility of the results a precise description of the computational steps for α and θˆ(-1) is required.

A1: In the revised paper (Section 2.1) the complete derivation of θˆ(-1) and α has been added as suggested.

C2: 2. Approaches based on dynamical systems and extreme value theory are developed under certain assumptions, which are usually not entirely fulfilled in case of applications to geophysical data. These assumptions, together with their possible consequences to the results of the analysis, are not mentioned in the manuscript. Furthermore, the manuscript lacks a critical discussion related to the advantages and disadvantages of the applied method. The authors should discuss these very important points in the paper.

A2: We agree that the limitations and the hypothesis of the theory should be specified in the present study. In Section 2.1 of the revised paper we have therefore added: “The recurrence approach for the computation of the dynamical indicators is based on the following assumptions: 1) the existence of an underlying chaotic attractor for the dynamics (Freitas et al. 2010), 2) the quasi-stationarity of the dynamics: the method can handle dynamics where weak nonstationarities are present in the dynamics (see e.g. Faranda et al. 2019 Nature Comm). The method cannot be used when the nonstationarities lead to bifurcations of the system.” As for the advantages: “with respect to statistical techniques, the dynamical indicators provide information on the underlying nature of the dynamics, i.e. the fact that co-recurrences are associated to high or low values of persistence and predictability connect co-recurrences to specific points of the phase space (unstable orbits, periodic points) (Faranda et al. 2020 Clim Dyn).”

C3: 3. In the conclusion, the authors write that their results are in correspondence with previous studies. However, they do not point out clearly enough the scientific gain based on this new work. What do we learn here we have not known before? This should be discussed thoroughly in the paper.

A3: Thank you for raising this point. In the Section 5 of the revised paper we added three main points highlighting the scientific gain obtained from our work. These are: i) the coupling between temperature and precipitation at large scale is driven by specific regions and processes (e.g. Cyprus-low) and therefore it does not always reflect the whole region under study; ii) the coupling results are highly sensitive even to non-extreme events, thus providing added value to conventional climatological analyses; and iii) our results provide information on specific factors that are driving the changes in the coupling of the variables being analysed (e.g. surface warming).

C4: I would also welcome some comments about choosing α=90th quantile as threshold for defining compound dynamical extremes. How robust are the obtained results against changes of this threshold? It would be also interesting to know what the authors think about the effect of the horizontal grid resolution on α and θ^(-1).

A4: In the initial stage of the research part of the analysis was performed with three α thresholds to test the sensitivity of the results: i) Q90th; ii) Q95th; and iii) Q99th. We found no significant differences when changing the α threshold and we make use of Q90th because a larger number of CDEs are available (compared to Q95th and Q99th), which can eventually provide more robust results when composited with Tmax (Tmin), P and SLP anomalies. We added two sentences referring to this in Section 2.1 of the revised paper.

Please, see below Figures R_1-R_3 which reproduce Figures 4, S7 and S9 in the submitted paper but for anomaly means computed from alpha extremes >95th quantile. As you can see, when changing the alpha threshold the synoptic patterns are kept and the only (expected) difference is the size of the anomaly means, which in the case of alpha>95th quantile are larger compared to alpha>90th quantile. We added a sentence specifying this in Section 4.2 of the revised paper (see also A20 of Referee 1).

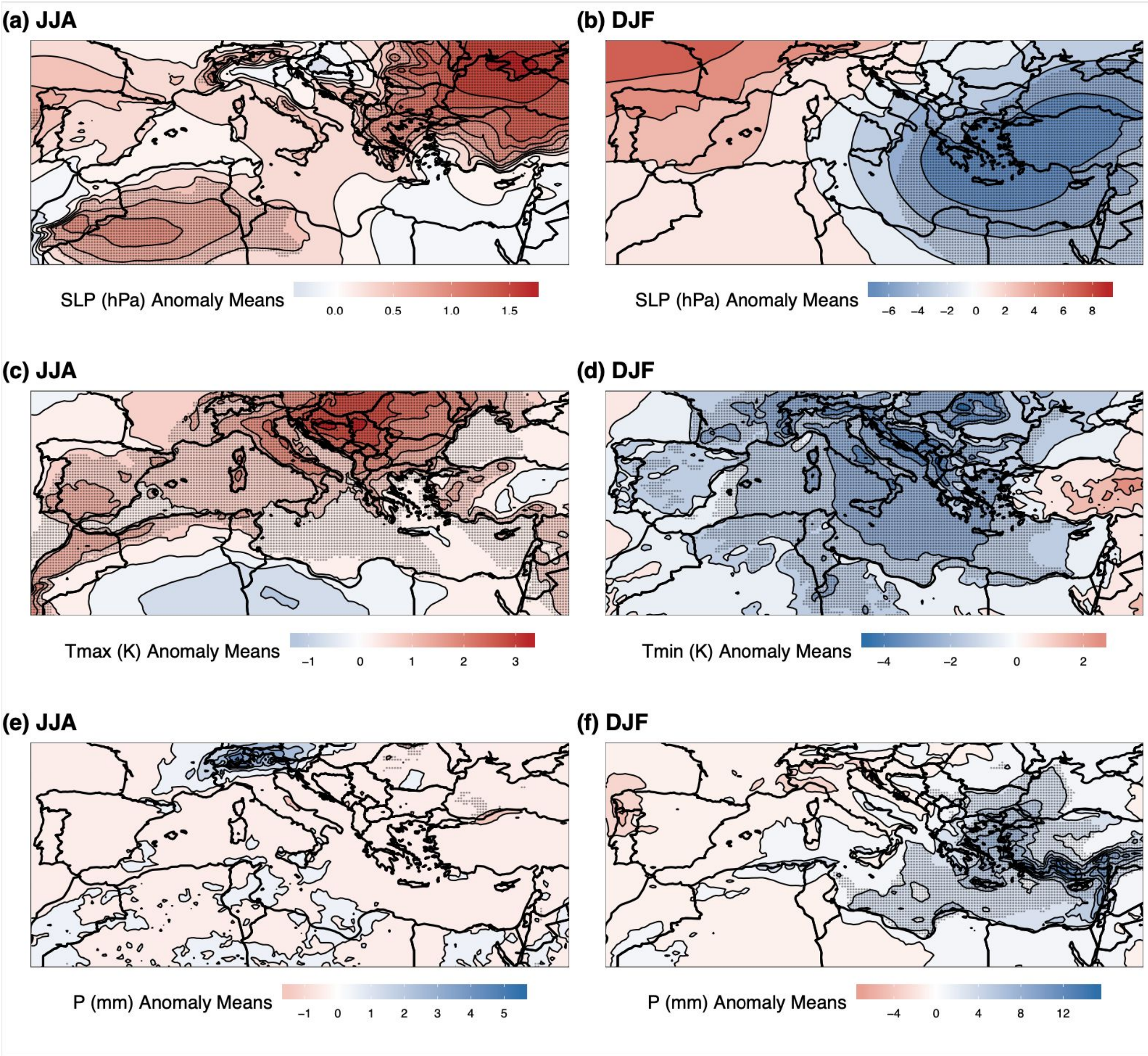

Figure R_1 - As Figure 4 but for alpha extremes > 95th quantile.

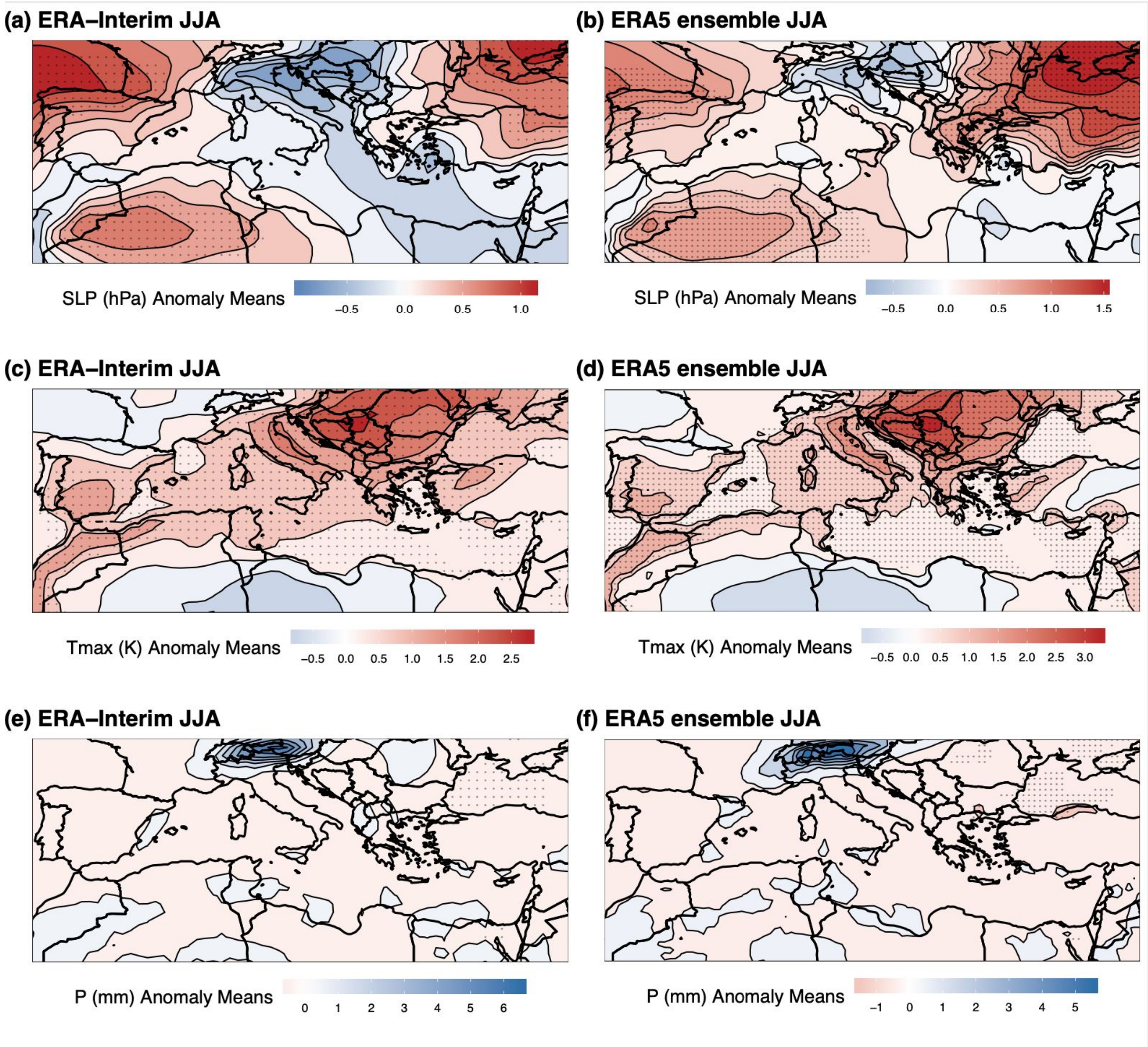

Figure R_2 - As Figure S7 but for alpha extremes > 95th quantile.

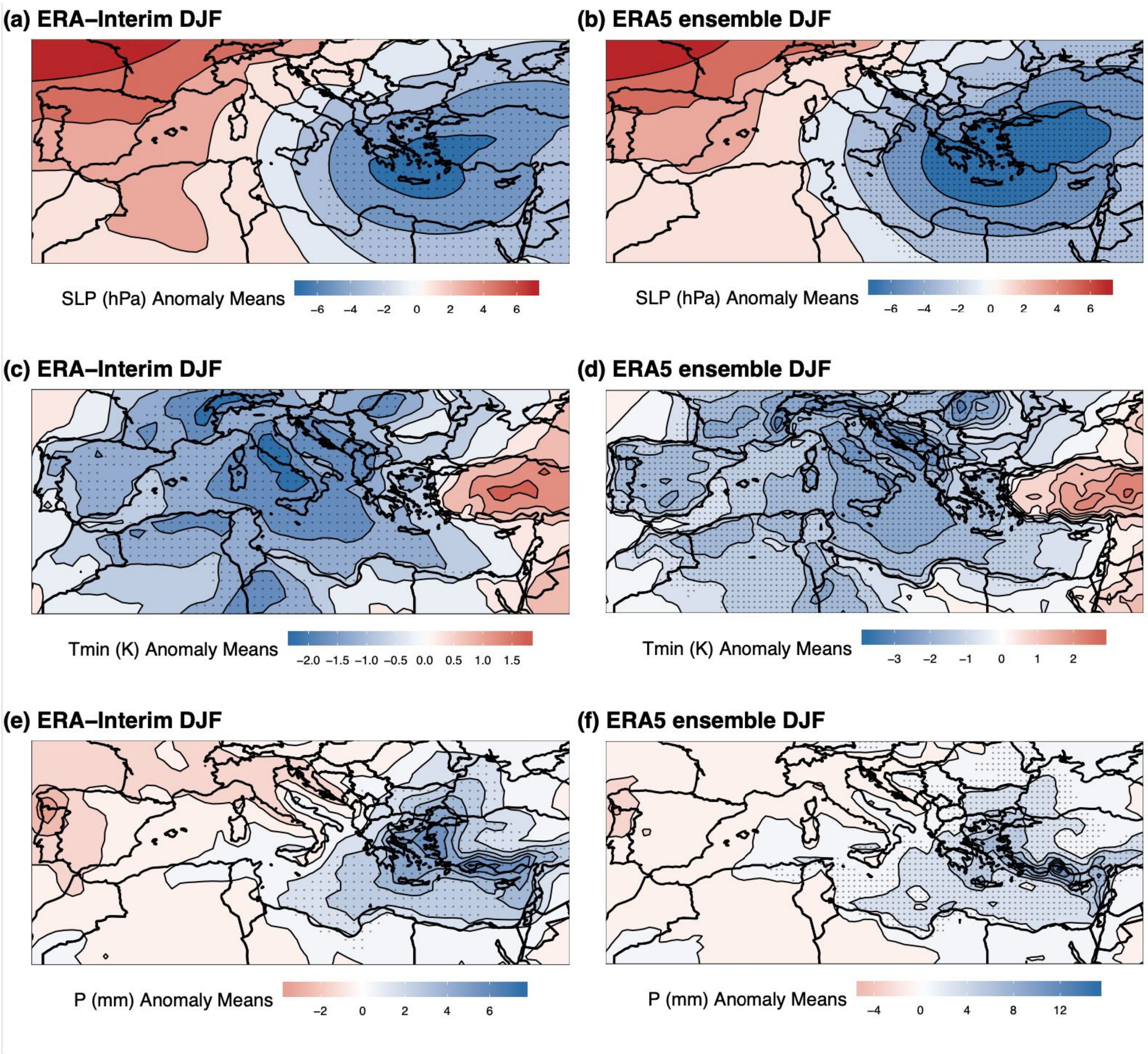

Figure R_3 - As Figure S9 but for alpha extremes > 95th quantile.

Horizontal grid resolution does not change significantly the α and θˆ(-1) values. This is confirmed in our work by looking at ERA-Interim (0.75deg) and ERA5 ensemble (0.5deg) results in the Supplementary Material. Such datasets have a coarser horizontal resolution compared to ERA5 (0.25deg) but still their trends, composites and spatial compound maps are in agreement with the ERA5 ones. We highlight this point when presenting the results for ERA5 throughout the text and also added two sentences in the revised paper (Section 5).

C5: A more detailed discussion of the spatial patterns of α and their possible connection to the atmospheric circulation would increase the quality of the paper as well.

A5: In DJF we found that SLP patterns composited on CDEs show a Cyprus-low over the eastern MED. This has been mentioned and discussed in the submitted paper. During JJA, the SLP patterns do not point to any documented synoptic configuration. Before submission we verified whether this may have depended on the presence of multiple, distinct clusters of configurations by using Self-Organising Maps (SOMs). However, for JJA results we provide a physical explanation of the α trends, which we found are driven by surface MED warming and prove that wet P anomalies are driven by convective P events (see **Update** at the start of this file). We also computed the standard deviations of the anomalies observed in Figure 4 (Figure R_4). See also answer A20 of Referee 1 and answer A6 of Referee 2.

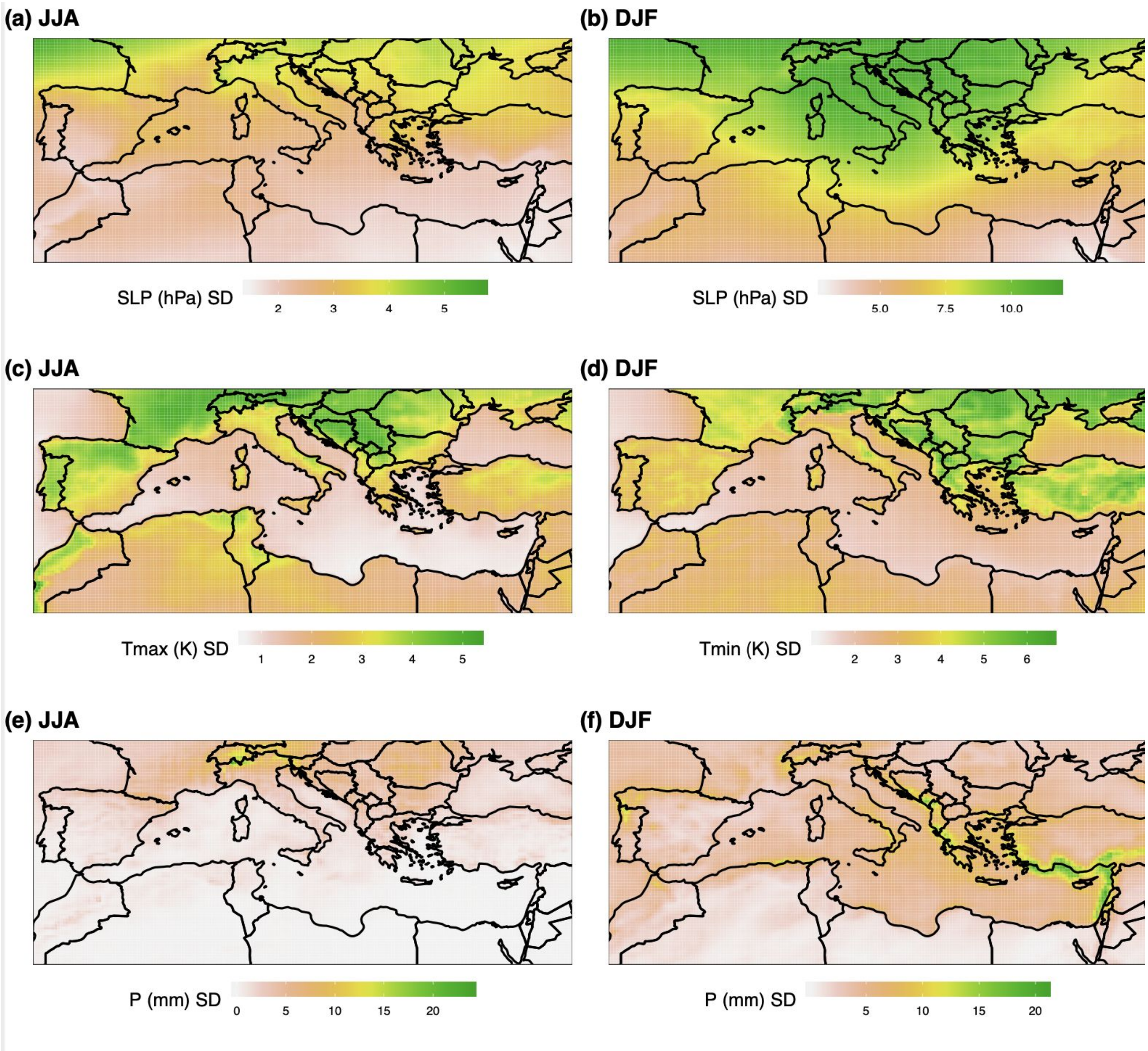

Figure R_4 - Standard deviations (SDs) computed for the anomaly means of Figure 4 in the main text.

SPECIFIC COMMENTS

C6: It is difficult to compare the results for the different reanalysis products, because of the different axis or colour scale limits. For example, Fig. 4 – S7 / S9, Fig. 5 – S11 / S12, Fig. 6 – S13 / S14.

A6: In the revised paper we redid the axes of the colorbars for all the maps so that a comparison between the different reanalysis products can be made.

C7: Fig.1(c) and 2(b): There seems to be no trend in $\theta\_(Tmax,P)\hat{}(-1)$ and in $\theta\_P\hat{}(-1)$ after 1995.

A7: Thank you for the observation. In this work we assessed the full trends from 1979 to 2018 and would like to keep sub-trend analysis, which may be more closely linked to low-frequency modes of climate variability, for further work.

TECHNICAL CORRECTIONS

C8: P4-L103: . . . higher (lower) than those observed. . .

A8: Text amended.

C9: FIG1: It is hard to see the difference between the two red lines.

A9: In all the figures showing trends we amended the thin lines (5-year moving averages) with dashed lines.

C10: P2-L7 Suppl.Data: "Tmax, Tmin and P" instead of "Tmax, Tmin and TP".

A10: Text amended.

C11: FIGS2 caption: "Tmin and P" instead of "Tmin and TP".

A11: Text amended.