

### Reviewer 3

The manuscript does not have enough scientific merit to be published in the journal. It does not provide significantly new information which go beyond the current state of the art. It is descriptive and does not add new elements in current understanding of compound extremes in the area. Further a few parts are inconsistent and also not scientifically sound. Thus, I suggest rejection. A detailed review is provided below.

R: We do not agree with these comments. For sure there is room for improvement of the manuscript and we thank the reviewers for helping us in this respect, but we do not agree that our manuscript does not provide significantly new information which go beyond the current state of the art. We are going to support our argument by answering point by point the reviewer's comment below.

The title is misleading as it only deals with Romania, not Eastern Europe.

R: In the title of the manuscript it's very clearly stated that the manuscript is focused on Romania. Nevertheless, the title will be change to reflect the analyzed region.

The abstract mentions that compound extremes are considered. However, it only reports on changes in extreme temperature and precipitation/drought separately. There is a lot of methodological papers out in the literature that deal with compound extremes, how they are modelled using sophisticated methods.

R: We disagree with this comment. Both changes in temperature and precipitation are analyzed individually and also combined (see Section 3.3). Our aim was to analyze if there are any changes in the joint frequency of warm and dry spells, and this has been analyzed in detail in Section 3.3.

Nevertheless, in the revised version of the manuscript we are going to improve the methodology for computing the compound events. In this respect, in the revised version of the manuscript we are going to include lagged and in-phase correlation maps between the two indices (SPI and HWDI) to be able to better argue why the combination of different months in defining the compound events. Moreover in the revised version of the manuscript we are going to show also the probability of occurrence of risk, conditioned on SPI (SPEI) and HWDI.

Introduction: There is a lot of literature that deals with extremes in southeastern Europe, including Romania, for instance also in the form of reviews:

Kuglitsch, F. et al. 2010: Heat Wave Changes in the Eastern Mediterranean since 1960, *Geophys. Res. Lett.*, 37, L04802.

Ulbrich, U. et al. 2013: Climate of the Mediterranean: synoptic patterns, temperature, precipitation, winds and their extremes. Future Climate Projections. In: *Regional Assessment of Climate Change in the Mediterranean*: A. Navarra, L. Tubiana (eds.), Springer

R: We do not really think that the suggested papers are state of the art papers regarding the occurrence of extreme events over Romania. The papers indicated by the reviewer are rather old and do just a superficial analysis of the extreme events in Romania. For example the paper of Kuglitch et al., 2010 focuses on a period between 1960 - 2006. From a temporal point of view this is rather old, and new analysis is always indicated, especially due to the fact that most of the extreme years in term of extreme temperature and precipitation have occurred over the last 20 years. Moreover, in the aforementioned

study a limited number of station covering Romania are used, which do not really give a proper overview of the complex climatology of the country. Moreover, the aforementioned comment of the reviewer implies more or less that scientist should stop doing regional studies, just because there are continental and/or global studies.

The introduction does not provide a clear justification why this work is needed, does not show gaps in current understanding and does not formulate a clear hypothesis.

R: In the revised version of the manuscript we are going to improve the introduction part in order to make the justification of the paper more clear.

The choice of more than 5 days defining a heatwave is not objectively based (lines 109/110). My suggestion would be to consult the latest literature that deal with more objective measures how heatwaves are defined.

R: Our choice of 5 days was based on the recommended thresholds for the regions surrounding Romania. Moreover, we also followed the recommendations of the Expert Team on Climate Change Detection and Indices (ETCCDI).

Heatwave results reported (lines 149-150, 4 to 5 days) are in disagreement with the definition provided in lines 109/110 that state more than 5 days.

R: In the respective line we speak about an average at country level, meaning that the number is an average over a certain number of grid points. Some grid points might fulfil the heatwave criteria, some not, thus when you average them you will get a number averaged over a large region.

Lines 160/161, there is an overlap of having the year 1970 in both periods. In addition, the period 1970-1985 has a different length compared to others that makes the comparison difficult.

R: The text will be modified.

SPI is not the most appropriate measure for drought. For the area, SPEI is a better index that combines temperature and precipitation.

R: We do not really agree with this comment. In this study we compare a precipitation based index with a temperature based index. We have chosen SPI on purpose, because if we would have use SPEI it would have meant comparing a temperature based index with another temperature related index and we wanted to avoid this comparison. SPEI is strongly affect by the global warming signal, thus we have tried avoiding using it in our analysis. Since SPEI is highly correlated with temperature is to a certain degree also already an indicator for a compound event. Nevertheless, in the revised version of the manuscript we are going to perform the same analysis by considering also SPEI.

For a review please consult Raible et al. (2017): Drought indices revisited – improving and testing of drought indices in a simulation of the last two millennia for Europe, *Tellus A: Dynamic Meteorology and Oceanography*, 69, 1287492.

R: We do not think that this paper is actually relevant for giving a clear suggestion which index is optimal, mainly because it's a modeling study and most of the models have issues in properly represent the potential evapotranspiration which is an essential component in computing SPEI. We think the choice of the drought index should reflect what the authors want to analyze. Our aim was to analyze changes in extreme temperature and extreme precipitation and we have tried to identify the proper indices to do so. A more indicate paper in this respect would be Stagge et al., 2017. Based on the analysis of Stagge et al.(2017) there are no significant difference between SPEI and SPI over our analyzed region (Figure 2c in their paper). Nevertheless, following the suggestions of all the reviewers involved in the review process of our manuscript in the revised version we are going to perform and show the same analysis by considering also SPEI.

The comparison between EOBs and ROCADA does not provide new evidence, it could be skipped and the analysis could be concentrated on EOBs.

R: We have added also the results of ROCADA dataset mainly because in previous studies we got complain that EOBS might not be suitable to make studies in Romania. But in the revised version of the manuscript we are going to remove the information and figures regarding the ROCADA dataset.

Sentence on lines 122-123 is not clear.

R: The text will be modified and improved.

The manuscript states at various places “statistical significant” changes. However, no information on the underlying statistics to test significance is provided. A few maps show significant areas related to trends, however it is missing how those regions are calculated.

R: This was a misfortune from our part. In the revised version of the manuscript we are going to add in the Methodology text all the statistical test and the associated references used throughout the manuscript.

Further, the synoptical maps do not have a field significance information and thus they are difficult to interpret.

R: The anomaly maps for the case studies cannot have a significance field because it is just a snapshot for one event in time, thus we cannot perform any statistical significance.

For the composite maps the significance of the anomalies is actually plotted in the figures (see figures S11 and S12 for example). Again it was a misfortune from our side that we did not write that clearly in the figure caption.

Further, the maps are not unexpected and the processes that lead to drought or heat extremes are well documented in the literature elsewhere.

R: We really disagree with this comment. The fact that processes that lead to drought/heatwaves are well documented in literature, does not mean that scientist should stop doing this kind of research. Each region has it's own particularities. Yes, of course a heat wave will most probably be driven by a blocking system, but this doesn't mean that we have fixed and fully closed the issue of analyzing drivers of extreme events. Again, it seems to be a very accepted approach for most of the already published studies, but for the current study doesn't seem to be accepted. Moreover, we have added also the stability maps in our manuscript (Figure 13) to further add some new info regarding the relationship between heatwaves and

their drivers and we consider this is a new way to study this kind of relationship. Thus, we think this comment does not really reflect the findings and analysis from our manuscript.

The conclusions include information from the introduction and duplicate the results. As such, a lot of information is irrelevant and the last paragraph is not a conclusion from the analysis shown.

R: The text will be modified, improved and adjusted to the new figures which are going to be produced throughout the review process.