We would like to warmly thank the Referee for their thorough review of our paper. The comments and suggestions provided will contribute significantly to improving the quality of our paper, making it more effective and clearer. Please find below our point-by-point responses (in red text).

General Comments

This study investigates the evolution of heavy precipitation events in the Mediterranean basin under a changing climate, focusing on the role of sea-atmosphere-orography interactions. Using both observational data and numerical simulations, the authors assess the trends in annual and maximum precipitation, with an emphasis on the Calabrian peninsula. They also explore how sea surface temperature changes might impact precipitation patterns, particularly during intense rainy seasons. The study emphasizes the importance of high-resolution, convection-permitting models to capture key processes. While the topic is relevant and the approach is promising, several aspects of the manuscript require clarification and improvement to strengthen the overall impact and scientific contribution.

We thank the Referee for the positive feedback and for highlighting the most important aspects of the research. We acknowledge that various aspects of the analysis require further strengthening. Below, we attempt to address the suggested concerns and comments.

Introduction

This section is overall well-written and provides relevant background. However, it does not clearly address the specific research gaps this study aims to fill. The authors should explicitly state the study's novelty (for example, the use of 20 real-case events at convection-permitting scale with calibrated SST perturbations) and how it builds on existing work.

The main aim of this paper is to investigate the extent to which Mediterranean Sea warming contributes to the seemingly counterintuitive increase in daily precipitation extremes in southern Europe, despite a general decline in annual precipitation. We agree with the Referee (and will modify the paper accordingly) that our aim to fill this research gap should be more clearly highlighted in the Introduction, along with the main strengths of the analysis performed (mainly, the convection-permitting resolution adopted in 20 real-world events with modified SST conditions, which allowed us to analyze in great detail the effect of sea-air-orography interactions on high precipitation features, and the special focus on overland precipitation, differently from the majority of the studies focusing on precipitation features over the whole domain).

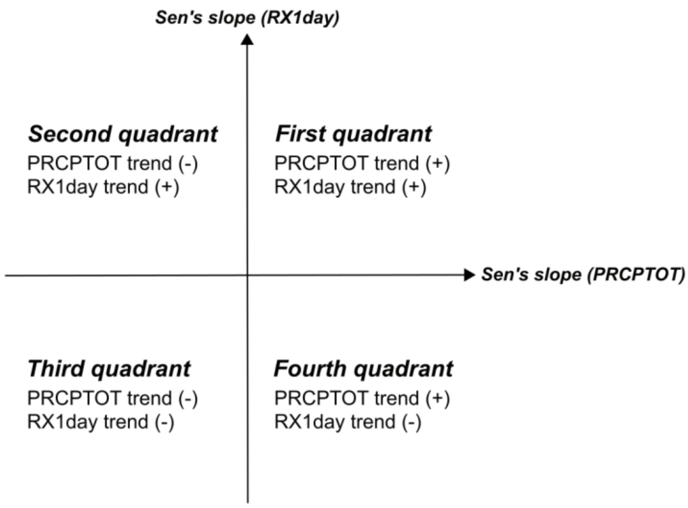
Data and Methods

1. This section would benefit from greater clarity and conciseness. The SST perturbation approach is not clearly explained, details on the magnitude, spatial pattern, and implementation (e.g., uniform or spatially varying changes) are missing.

In the Data and Methods section, we presented only the datasets used for the SST analysis (LL114-131), including the extent of the area analyzed (L120). Then, in the Results section, we provided more details about the perturbation approach (LL262-269), including the magnitude (from -1 °C to +3 °C compared to current conditions) and the spatial pattern and implementation (e.g., we claimed a homogeneous change). We agree that the SST perturbation approach can be presented in a clearer and more organic way in the Data and Section method, as pointed out also by the other Referees (e.g., Referee #3 comment no. 6).

2. The quadrant classification based on PRCPTOT and RX1day trends is also unclear, a clearer definition or simple diagram would help.

The quadrant classification is a method for highlighting the combined trends of two different variables. We will strive to explain it more clearly, even with a simple diagram like the one below. However, the representation of the results (Figs. 3 and 5) will be modified according to the comments of other referees (Referees #2, specific comments).



3. Additionally, the event identification method (starting the event the day before precipitation begins) is unconventional, as most studies define event onset based on a precipitation threshold or objective criteria, further justification is needed.

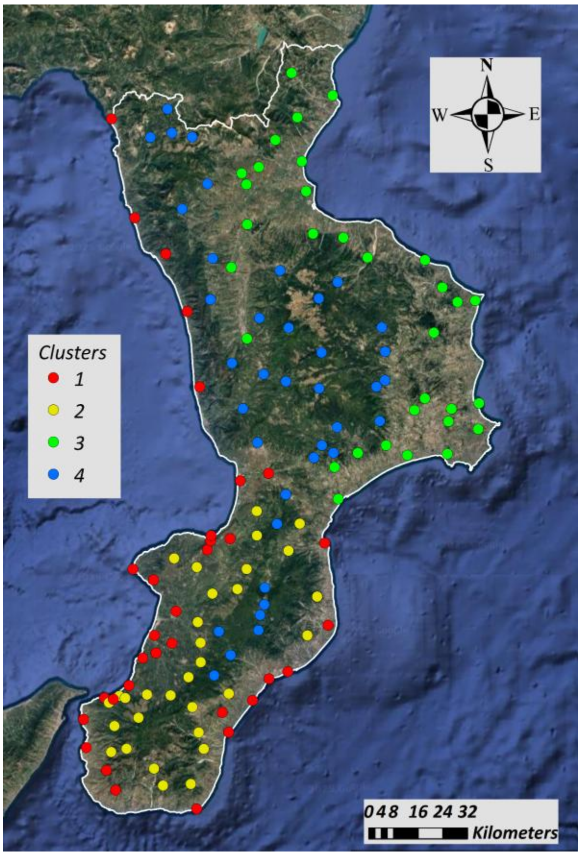
We sincerely thank the Referee for this comment, which points out a wrong statement in the manuscript, 'survived' from previous versions (it was related to some spin-up issues). Indeed, event selection was made objectively. Specifically, we used a fixed threshold of 0.8 mm on the average daily precipitation values simulated by the WRF model (SST0 simulation) over the Calabria region. Indirect confirmation of the use of this approach is given by Figure 9, from which it is clear that the day-before-precipitation approach was not used. We opted for this choice, rather than detecting the event based on the observations, because (a) as shown in Figure 9, the SST0 simulation performs very well in terms of timing, and (b) this choice makes it easier to further compare with the SST+3 and SST-1 simulations.

Results and Discussion

Overall, the results and discussion sections offer valuable insights, but they lack clear physical explanations to support the findings.

1. In the Trend Analysis section, the authors present an analysis of observed rainfall trends in Calabria but fail to adequately link these results to the region's orographic features. While the orography is mentioned multiple times, there is no attempt to explain how it might be influencing the observed rainfall patterns, particularly in terms of total maximum rainfall. This lack of connection makes it harder to understand why different trends are observed.

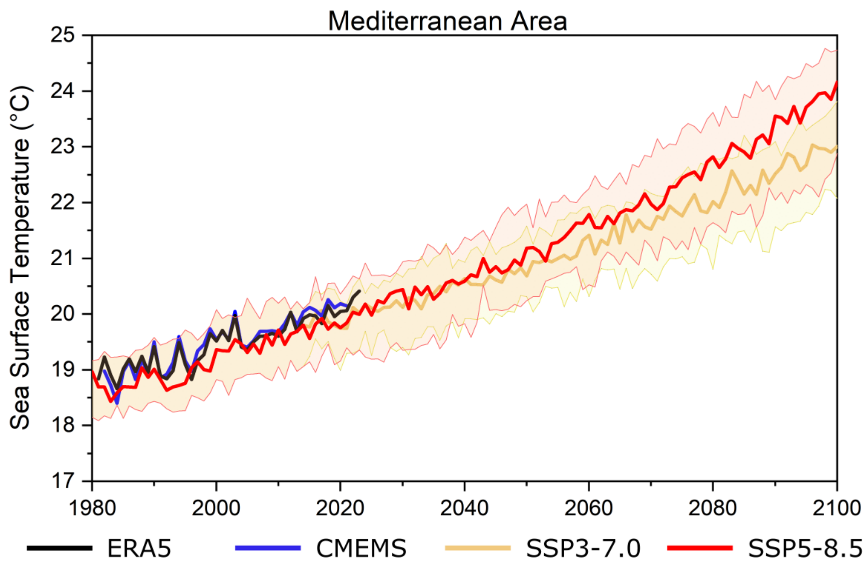
While several studies are available concerning features of precipitation spatial patterns in Calabria, which will be duly cited, a new analysis will be proposed and discussed based on the available dataset, in which geographical and orographic features like latitude (N), longitude (E), elevation, slope, aspect, and distance from the sea are analyzed to study how effectively they affect observed average annual precipitation (Precp_avg), as well as trends of PRCPTOT and RX1day in terms of Sen's slope. By applying the k-means method within the framework of Principal Component Analysis (PCA), the area of interest is preliminarily divided into four homogeneous clusters, as shown in the figure below.

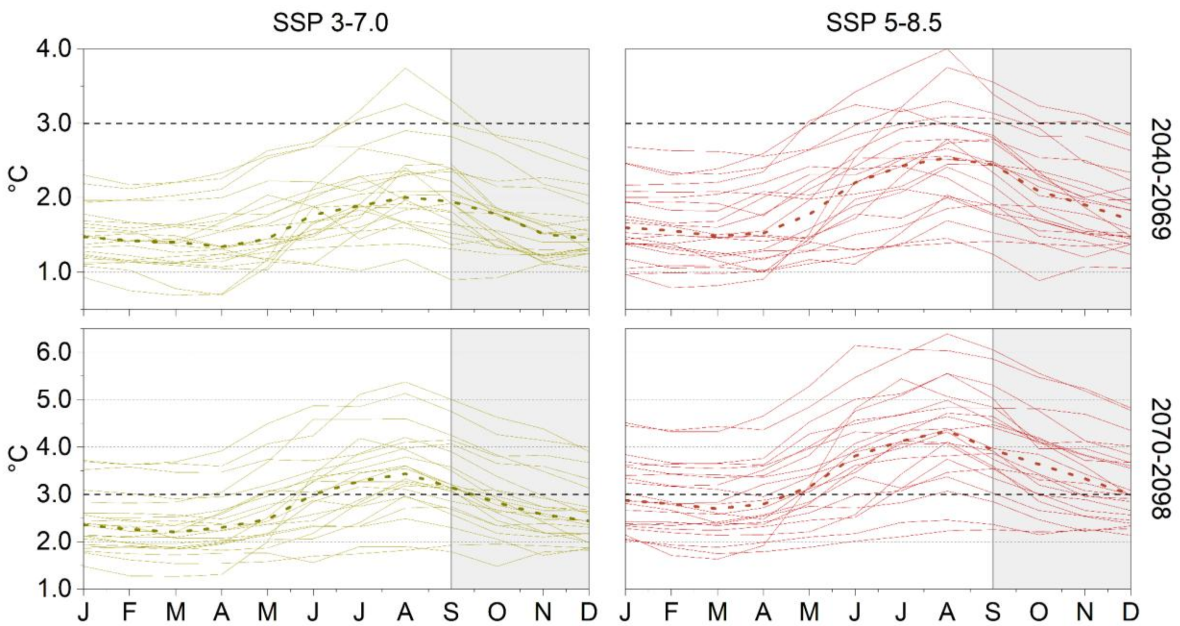


The findings achieved so far highlight that orography is more closely associated with the spatial pattern of observed precipitation than with the temporal trends of PRCPTOT and RX1day.

2. In the Observed and Projected SST Warming section, the authors choose SSP changes of -1 and +3°C for their simulations, but the rationale behind these choices is unclear. Were these values based on region-specific data, or were they generalized from the broader Mediterranean basin? It would also be more logical for the authors to focus more on their study area (Domain 3) rather than a larger region.

We believe that the rationale for the choices is explained in this section. However, as also noted in our reply to the previous Data and Methods comment no. 1, we'll strive to present the entire approach in a more straightforward and organic manner. The spatial extent of the analyzed region corresponds to the IPCC Atlas Mediterranean region (L120). However, we agree that, to avoid confusion among different domains and study areas, it is more straightforward to focus on the external domain (D01) of the WRF simulation, so we changed Figure 6 and replaced Figures 7 and 8 with a new one, following also suggestions provided by Referee #2 (comment no. 5). The new figures are shown below (in the spaghetti graphs, the dotted line represents the median behavior). We observed a slight increase in projected SST. Further details will be provided in the revised text.





3. For the WRF Simulation Evaluation, the authors chose to spatially interpolate the observations. However, it's unclear whether this is the most appropriate method for comparison. Interpolating observations can introduce uncertainty and might not accurately

reflect the spatial variability of the actual observations. It would be useful for the authors to justify why they chose this method.

We thank the reviewer for the comment. Spatial interpolation was performed using an Inverse Distance Weighting (IDW) technique (i.e., an exact interpolator), as specified at L111-113 in the original manuscript, with a total of 150 stations (134 “historical” and 16 “recent” gauges), resulting in a density of approximately 1 station per 100 km^2. While we acknowledge that using an interpolation method (which inherently means adding another model) adds uncertainty, we observe that: (a) a gauge is not necessarily entirely representative of the calculation cell of the atmospheric model in which it falls, especially in the numerous steep areas of the region, therefore also gauge-to-pixel direct comparison can be affected by some bias; (b) on the other hand, the density of the monitoring network in the region is high, preventing significant misinterpretations of the spatial patterns of precipitation; and (c) while literature is plenty of studies based on gridded datasets of questionable quality (the topic is partially addressed also in this paper, but for southern Italy please refer, e.g., to Cammalleri et al., 2024), in this case we have a fully validated spatially dense observational dataset which is a perfect candidate for the development of a reliable gridded dataset. We will add these comments to the text to justify our choice.

4. The Figure 12 showing the eastward shift of extreme rainfall events is visually appealing, but the authors do not explain the underlying mechanisms driving this shift. While they focus on a single event, there is no physical explanation for the observed trend, which limits the depth of the physical understanding gained from the analysis.

In the manuscript, we provide details of the underlying mechanisms of the shift through the example of event no. 12 (LL 327-337 and Fig. 13). However, considering also comments from other Referees (Referee #2 comment no. 7, and Referee #4 comment no. 6), we acknowledge that the explanation should be more general and straightforward and we will strive to provide more details in the revised version of the manuscript.

5. In the Comparison with Previous Studies section, the authors compare their findings to studies that account for global warming, rather than focusing only on SST. This comparison could lead to misleading conclusions because the underlying drivers of rainfall changes could differ between global warming and SST warming alone. More clarity on the boundaries of their analysis and comparison with relevant studies focusing on SST would enhance the discussion.

We thank the Referee for this comment. This Section will be expanded, providing more room for studies based solely on SST warming, to which we previously only hinted (LL340-341), adding others (e.g., Lin et al., 2023; Dutheil et al., 2022), and highlighting the boundaries of our analysis even more effectively. However, it is also of interest to us to refer to “complete” global warming scenarios to provide the whole picture, also because we are currently working on convection-permitting climate simulations in the study area (https://doi.org/10.5194/egusphere-egu25-15936), and they represent an area of future investigation for us (L411).

6. Lastly, Figures 7 and 8 present an overwhelming amount of information, making them difficult to interpret. The authors might consider finding an alternative way to present these results, perhaps by simplifying the figures or breaking them down into more digestible parts.

We agree with the Referee. This concern was also raised by other referees (Referee #2, comment no.5, Referee #3, comment no.5, and, partially, Referee #4, minor comment no. 15). Following Referee #2's suggestion, the representation of projected SST increase will be changed entirely, using only one multipanel figure with four spaghetti graphs, showing the SST increase compared to 1985-2014 in the periods 2040-2069 and 2070-2098, under the SSP3-7.0 and SSP5-8.5 scenarios, respectively. Additionally, according to comment no. 2 concerning the Results and Discussion section, the area on which we will base our calculations will no longer be the entire Mediterranean basin, but the external domain D01 (please refer to our reply to comment no. 2).

Conclusions

1. The conclusions section lacks impact and doesn’t clearly tie the study’s findings together. It doesn't explain how the research advances our current understanding. While the authors summarize their results, they could better highlight the practical implications of their work. For example, they could link their findings to how the study might help predict or mitigate future storms. It would also be useful to mention the key implications of these trends for climate adaptation or urban planning, especially in terms of how extreme precipitation affects flood risks.

We thank the Referee for their several valuable suggestions, which will be given due consideration. In particular, we will highlight the advancements in current understanding made possible by our high-resolution simulations and our focus on changes in overland precipitation, as well as how our research can contribute to mitigating flood risk. Indeed, this aspect was already hinted at in the last paragraph of the manuscript (LL412-415), but we will elaborate on it further.

2. Lastly, the statement that "only high-resolution, convection-permitting analyses can accurately capture key processes" is too strong and could benefit from further context.

We agree that this sentence is sharp and will strive to contextualize it better, while (anyway) avoiding making the concluding paragraph excessively long.

References

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