

Once more, we warmly thank the Editor and Reviewers for thoroughly reviewing our paper. Please find below our point-by-point replies (red text). All page and line numbers refer to the revised version of the manuscript unless otherwise specified.

Editor

Dear Alfonso Senatore,

I have now received the reports from three reviewers (two who reviewed the original submission and one additional reviewer). Two reviewers recommend acceptance, whereas one recommends rejection. The negative recommendation primarily concerns the trend analysis, which the reviewer considers requires substantial additional work to reach an appropriate scientific standard (see the detailed comments). Upon re-reading the manuscript, I share these concerns regarding the trend-analysis component. In addition, the link between the trend analysis and the WRF numerical experiments, which are the core of the manuscript, is currently not sufficiently clear, and it is not evident that the trend analysis is necessary for the central message of the paper.

I am therefore inviting a moderate revision. I strongly encourage you to refocus the manuscript on the WRF analysis and to substantially reduce, or remove entirely, the trend-analysis component. This would better align the work with the current title and scope. The revised manuscript will be evaluated again by an external reviewer and by me. Please also address the technical/minor issues raised by the other reviewers.

I look forward to receiving your revised submission.

Sincerely,
Nadav Peleg

Dear Editor,

Thank you very much for your valuable suggestions about how to refocus the manuscript. We understand both your and the Referee #5 concerns, and agree that a clearer emphasis on the main topic of the study, which is the interrelationship between increasing SST and heavy rainfall events, will make our paper clearer, more direct, and, hopefully, more impactful. To this aim, we have substantially reduced the section on trend analysis, which, properly strengthened in accordance with Referee #5's comments, will be the subject of future work. The main parts of the paper that were changed/removed can be summarized as follows:

- The Abstract and Introduction were modified to reflect the substantial removal of the trend analysis;
- Section 2.1 “Datasets and study area” (now “Study area and datasets”) was cleaned up of any reference to datasets used only for the trend analysis (mainly, ERA5-Land);
- Previous Sections 2.2, 3.1, and 4.1, all named “Trend Analysis”, in the Methods, Results, and Discussion sections, respectively, were removed;
- Some related sentences in the previous section 4.2 “Mediterranean Sea warming effect on precipitation” were also removed;
- Conclusions were also modified to exclude any reference to the trend analysis;
- Finally, the Supplementary material was also modified accordingly, removing previous Figs. S2-S4 and Table 1.

On the other hand, we decided to leave a 'small trace' of the trend analysis in the Appendix (Fig. A1). It concerns the Mann-Kendall and Sen's slope tests for PRCPTOT at the annual scale and RX1day at the wet-season scale in Calabria. While we smoothed and contextualized our findings as much as possible, we believe they are relevant for informing the selection of our study area and for shaping the framework within which we conducted our modelling experiments.

Finally, we have added all the references suggested by Referee #5, which we considered very appropriate, and replied to the technical issues raised by Referee #2.

We believe these further changes definitively rebalance the paper's structure, focusing on the main message of our research, and we look forward to your feedback.

Anonymous Referee #2

I offer the following comments to help further refine the manuscript.

L107: The phrase "for the first time" is not clear to me in this context. If the meaning is that it is the first time 20 real-world events were simulated, the authors had already proved differently ("To the authors' knowledge, only Armon et al. (2022) performed a systematic analysis by identifying 41 extreme events..."). Please explain or revise.

We were referring to the study area. However, for the sake of clarity, we removed the phrase.

L211: The phrase "New experiments" is not clear to me in this context. New compared with what?

We changed the phrase in "The results of these additional experiments" (L178), referring to the experiments performed applying spectral nudging to the outer domain (D01) on the geopotential (ERA5 source) at heights above 500 m.

L254–260: Since the EURO-CORDEX is now not the focus, can the authors change the text such that it only refers to the EURO-Med region?

The paragraph to which this comment refers has been removed from the current version of the paper.

L280–283: "The results... according to other studies" — so is it shown in the results or in other studies? Not clear. "Instead" of what? Can you either choose a different word here or be more clear about it?

The paragraph to which this comment refers has been removed from the current version of the paper.

L383: " In the Fig.," should probably be "In Figure 12" or "In the Figure."

Thanks for noting that. We changed with "In the Figure".

Anonymous Referee #5

This manuscript by Senatore et al. aims to improve our understanding of past and future changes in extreme and total precipitation in the western Mediterranean, a region critically affected by anthropogenic hydrological changes. The authors have undertaken a comprehensive study, integrating diverse observational and modelling datasets to address a scientifically important and timely question. My main concern is that the manuscript contains an excessive amount of information, which dilute the focus and make it difficult for the reader to identify the key messages. The manuscript appears to present two distinct bodies of work: trend analysis and modelling experiments. This tends to undermine the valuable modelling experiments designed to improve our understanding of how extreme precipitation may have changed and will respond to warming sea surface temperatures. Conversely, I believe the trend analysis section requires further consolidation before it is ready for publication.

At this stage, and considering the numerous reviews already provided, I recommend rejecting the manuscript. However, I believe that the work based on the WRF simulations is very valuable for the community and should be worthy of publication after major modifications are done to this current manuscript. I suggest that the trend analysis component be further consolidated and ideally separated from the modelling work to improve clarity and focus.

We thank Referee #5 for their clear recommendations. According to them, and in agreement also with the Editor's suggestions, we almost totally removed the parts of the manuscript related to trend analysis, emphasizing the main focus of the paper related to the interrelation between warming sea surface and heavy/extreme precipitation:

- The Abstract and Introduction were modified to reflect the substantial removal of the trend analysis;
- Section 2.1 "Datasets and study area" (now "Study area and datasets") was cleaned up of any reference to datasets used only for the trend analysis (mainly, ERA5-Land);
- Previous Sections 2.2, 3.1, and 4.1, all named "Trend Analysis", in the Methods, Results, and Discussion sections, respectively, were removed;
- Some related sentences in the previous section 4.2 "Mediterranean Sea warming effect on precipitation" were also removed;
- Conclusions were also modified to exclude any reference to the trend analysis;
- Finally, the Supplementary material was also modified accordingly, removing previous Figs. S2-S4 and Table 1.

However, we decided to leave a 'small trace' of the trend analysis in the Appendix (Fig. A1). It concerns the Mann-Kendall and Sen's slope tests for PRCPTOT at the annual scale and RX1day at the (wet) seasonal scale in Calabria. While we smoothed and contextualized the results as much as possible, we believe they remain relevant for informing the selection of our study area and for shaping the framework within which we contextualized our modelling experiments.

Given the paper's new structure, the Referee's comments on the removed sections were not addressed. However, they will be duly considered for further research, as we plan to include trend analysis in future work.

Below are some major comments that I hope will assist the authors in refining their manuscript for resubmission and ultimately achieving publication:

Modelling experiments. The approach of perturbing sea surface temperatures to represent past (SST-1), present (SST0), and future (SST+3) conditions is a particularly effective way to capture the impact of observed and projected changes in the regional sea surface temperature on precipitation in the surrounding land. However, it remains unclear how the fall 2019 season examined in this study compares with climatological values of extreme precipitation indices such as RX1DAY. How extreme were this observed sequence of events relative to present-day climatological conditions? Providing a clear discussion on this point would strengthen the study's impact by emphasizing the relevance and significance of the findings. This clarification is particularly important given that the study in its current states aims to compare results with past observed PRCPTOT and RX1DAY values.

The fall 2019 season was not characterized by particular precipitation records, neither in terms of PRCPTOT nor RX1day. In the new version of the manuscript (LL127-133) we more clearly explain that that season was chosen as an "example" (L127) because of its notable meteorological variability, with two well-reported events by the Regional Environmental Agency (<https://www.cfd.calabria.it/index.php/pubblicazioni/voce-2>), and its relative proximity in time, which allowed for a considerable number of measuring stations available (150 gauges, roughly a gauge per 100 km²). Of course, in the new version of the paper, the comparison with climatological PRCPTOT and RX1day no longer holds, as the objective is to assess the sensitivity of the events to varying SST boundary conditions.

Trend Analysis. I find the interpretation of the results based on ERA5-Land somewhat misleading. The observations reveal significant trends only in a limited portion of the region, which suggests that conclusions should be drawn cautiously. The authors discuss trend patterns that are not statistically significant. While this might be acceptable if supported by clear evidence from the literature, such a connection has not been convincingly demonstrated in the current manuscript. The authors further support their trend analysis using in situ data, and I acknowledge the effort to incorporate multiple observational sources to complement the ERA5-Land dataset. However, the significance of these results remains limited, and overall, the findings are not very convincing. For example, the spatial distribution of quadrants in Figure 6 appears patchy, and most of the trends for both PRCPTOT and RX1DAY are not statistically significant. The authors should interpret these results with caution and focus their discussion on regions and patterns supported by a substantial number of stations or grid cells exhibiting significant trends. Although there is a discussion of this limitation around lines 468–478, it is insufficient, and more importantly, some conclusions are made without sufficient support. Additionally, within the context of the manuscript in its current form, it would be more appropriate to focus on trends specifically during the fall season, aligning with the period of interest in the modelling experiments section. This further underscore the challenge of combining trend analysis and modelling work within a single manuscript, reinforcing my recommendation to split them into two separate studies for improved clarity and coherence. I also suggest investigating seasonal trends (in addition to annual trends) given evidence that future changes in mean and extreme precipitation tend to scale more reliably at the seasonal level rather than annually in the Mediterranean region (Bador and Alexander, 2022).

According to the Referee's recommendation, the trend analysis was almost totally removed and will be finalized in a separate study. For this reason, most of the comments raised by the Referee in this paragraph were not addressed, although they will be duly considered for the future. However, as already described, we decided to leave a 'small trace' of the trend analysis in the Appendix (Fig. A1 and related text). Specifically, we present the Mann-Kendall and Sen's slope tests for PRCPTOT at the annual scale and, following the Referee's suggestion, for RX1day at the seasonal scale in Calabria. This choice is to strengthen and add value to the choice of the study area.

Throughout the manuscript, it is not always clear whether the authors aim to investigate changes in the intensity or frequency of precipitation. In the trend analysis section, the focus appears to be on past changes in the intensity of both total and extreme precipitation. However, the objectives for the rest of the study remain unclear. What are the main hypotheses regarding how changes in sea surface temperatures might affect precipitation? This should be explicitly clarified, especially since the author reproduce past observed events under perturbed climate conditions representing idealized past and future SST conditions while the synoptic patterns leading to the occurrence of the events remain unchanged. Clarifying these expectations and explaining how the findings address them would greatly improve the impact of the work.

We hope that the new structure of the paper helps clarify the highlighted lack of clarity regarding the paper's main aim. Results address both SST-related changes in precipitation frequency and intensity, but with the peculiarity of focusing on overland impacts (e.g., LL404-408). Related to that, changes in precipitation spatial patterns were investigated. While some basic hypotheses were already drawn in the previous version of the manuscript (LL67-77 in the new version), the main hypothesis, uncertainties, and expectations regarding the analysed process are now more explicitly expressed (LL89-92).

Finally, several recent and important papers relevant for comparison appear to be missing from both the introduction and the broader discussion of the implications of this work: González-Alemán et al. (2023), Ginesta et al. (2023), Bador et al. (2025).

References:

- Bador, M., & Alexander, L. V. (2022). Future seasonal changes in extreme precipitation scale with changes in the mean. *Earth's Future*, 10(12), e2022EF002979.
- Bador, M., Boé, J., Caillaud, C., Terray, L., Moine, M. P., & Alias, A. (2025). Cooler than observed sea surface could have reduced impacts of storm Alex and induced mediterranean heavy precipitation event in France. *Climate Dynamics*, 63(12), 460.
- Ginesta, M., Yiou, P., Messori, G., & Faranda, D. (2023). A methodology for attributing severe extratropical cyclones to climate change based on reanalysis data: the case study of storm Alex 2020. *Climate Dynamics*, 61(1), 229-253.

We thank the Referee for the suggested references. We found all four of them appropriate and added them to the new version of the manuscript.