We would like to warmly thank the Referee for their highly detailed 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

The manuscript “Increasing Daily Extreme and Declining Annual Precipitation in Southern Europe: A Modeling Study on the Effects of Mediterranean Warming?” by Senatore et al. addresses an important and timely issue of the apparent paradox between increasing event (or daily) rainfall amounts despite the overall drying of the Mediterranean. While this issue was previously discussed in many studies, at least since Alpert et al. (2002), a complete answer seems to be lacking from the literature, and studies that provide pieces of this puzzle are needed.

The authors use long-term ERA5-Land data to show the context of precipitation pattern change throughout the Mediterranean (and Europe), numerous gauge records from Calabria to show these changes over the region, and high resolution WRF model simulations to investigate the impact of changing sea surface temperature on precipitation. The combination of tools and analyses provides a potentially valuable contribution to the field; however, while the study, as it is contextualised, is ambitious, it suffers from several weaknesses that make this framing exaggerated. Specifically, (a) the manuscript lacks clear articulation of the knowledge gap it seeks to address (is it precipitation increase during extremes while total precipitation decreases? Is this clearly answered in the text?); (b) the authors invest significant effort in justifying the representativeness of both the Calabria region and the specific season chosen for simulation, suggesting that these are indicative of broader Mediterranean conditions and climate change trends. While these arguments are reasonable, the paper would remain valuable even if it were framed more narrowly — as a regional case study focused on Calabria and/or a specific season. Overextending the generalization to the entire Mediterranean may risk overstating the broader applicability of the results; (c) Lastly, the authors frame the research in the intensification of extreme precipitation while overall precipitation is decreased. However, their results suggest increase in total precipitation. While these are reasonable results given the framework of increased SST only, this should be explicitly mentioned, and the authors should explain whether their study shows an exception to the general behaviour, or it only addresses a specific part of climate change effects on precipitation.

Given these comments, in my view, the manuscript should undergo major revisions before it could be published in HESS. Further comments are written below.

We thank the Referee for the generally positive feedback. We particularly appreciate their acknowledgment that, about the issue addressed, “a complete answer seems to be lacking from the literature, and studies that provide pieces of this puzzle are needed”, which provides strength to the general purpose of our paper, and the potential added value of the combination of tools and analyses used. Concerning the three general comments that have arisen:

(a) 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 acknowledge that this main purpose could be more clearly addressed (as it has also been highlighted by other referees, e.g., Referee #1), and to this aim, we will reshape the Introduction section, taking up also the Referee’s suggestions and comments

(particularly, major comment no. 1). We firmly believe that the text contains useful insights concerning the topic, and will try making our answer clearer.

(b) We acknowledge the recalled risk of "overextending the generalization to the entire Mediterranean", which may overstate "the broader applicability of the results." On the other hand, we do not consider it correct to overlook the representativeness of the study area in the broader context of the Mediterranean, which, as the Referee states, is supported by reasonable arguments. Therefore, in the revised version of the manuscript, we will strive to balance these two aspects, defining the limits and boundaries of the results obtained more clearly.

(c) We assume that the conclusion of the Referee that our results "suggest increase in total precipitation" lies in the fact that, considering the precipitation events analyzed, the total amount is higher with SST+3 than with SST0. However, in the manuscript, we stated that "each event was considered as standing alone with respect to others" (L179) and "can be considered as isolated" (L347), while, among the information missing in our PGW scenario, "the expected frequency of cyclones, given the large-scale circulation dynamics changes induced by climate warming, is another relevant piece of information missing" (L349). Therefore, while we have proven that most of the events, individually analyzed, produce more rain under SST+3 conditions than under SST0, we can't prove a generalized increase in total precipitation because we can't provide information about the projected frequency of such events (and about other aspects, as pointed out by the Referee in their major comment no. 8). In the revised manuscript, we will include this discussion.

Finally, below we reply to the major and specific comments of the Referee.

Major comments

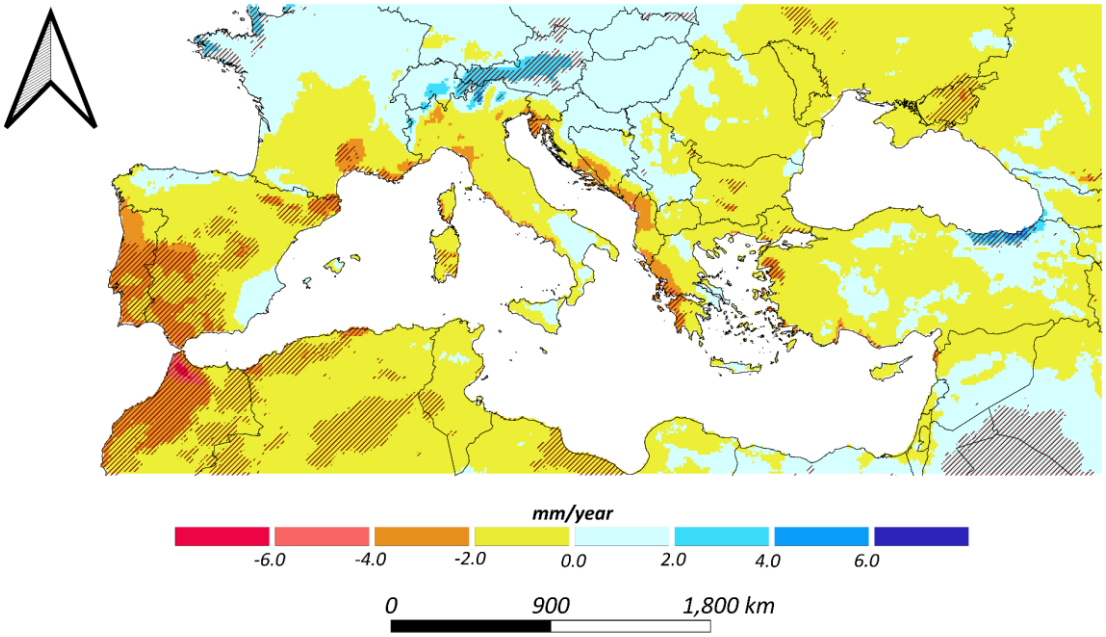
1) The last paragraph of the introduction provides both further motivation and background for the study (L64-67) and the task/object of the manuscript (L67-L84). In my view, the introduction lacks a clear description of the *need* for the work. After reading the first part of the introduction readers are likely familiar with some of the challenges and background related to understanding the impact of global warming on precipitation in the northwest Mediterranean. However, there is no direct statement clarifying what is not known and what is going to be resolved with this work. Please consider adding a description of the *need* and separate it from the *task* and *object*.

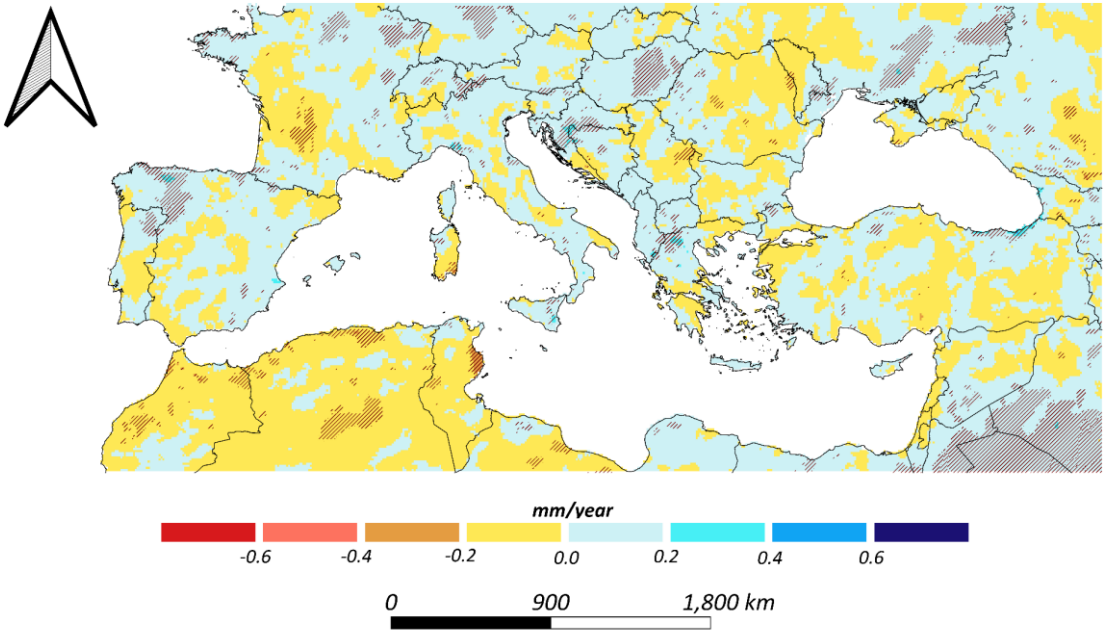
We thank the Referee very much for this valuable suggestion. As stated before (reply to general comment a), and as replied to other referees (particularly, Referee #1), in the revised Introduction we will strive first to clarify the research gap we aim to fill (the *need*) and then what results we aim to achieve and how (the *object* and the *task*), highlighting the peculiarities (strengths) of the analysis performed.

2) The use of EURO-CORDEX in the study is clear. However, presenting the results over the entire domain is not necessary. The authors should consider focusing this part of the results on the Mediterranean area only.

Thank you for this suggestion. Although we were somehow "charmed" by the latitudinal gradient achieved over Europe, especially concerning PRCPTOT trends, we agree that an area of interest more focused on the Mediterranean Basin can contribute to a more robust and understandable storyline. Furthermore, limiting the study area also helps in comparisons with

observations. Figure 2 of the manuscript will be replaced with the one below (upper panel for PRCPTOT and lower panel for RX1day). Additionally, Figure 3 (which will be further modified in accordance with other comments - e.g., the Referee's specific comment on Figure 3) will display the same spatial extent.





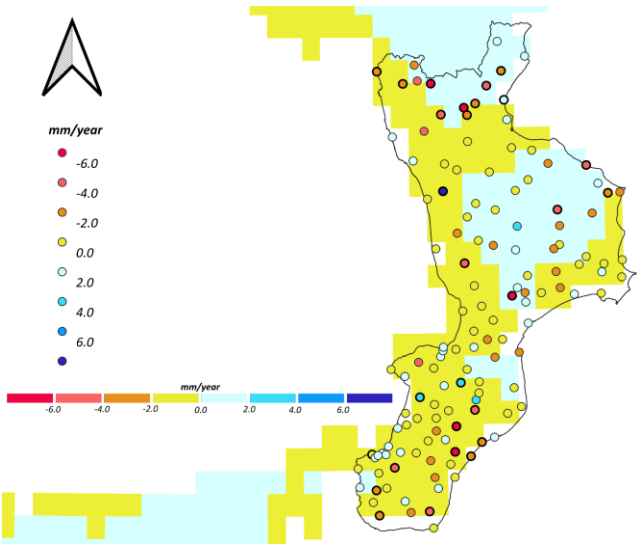
3) The Calabria region is at the focus of this study. The introduction lacks clear presentation and reader guidance about the region and why it serves as a case study, or why it is interesting to a broader readership unfamiliar with it.

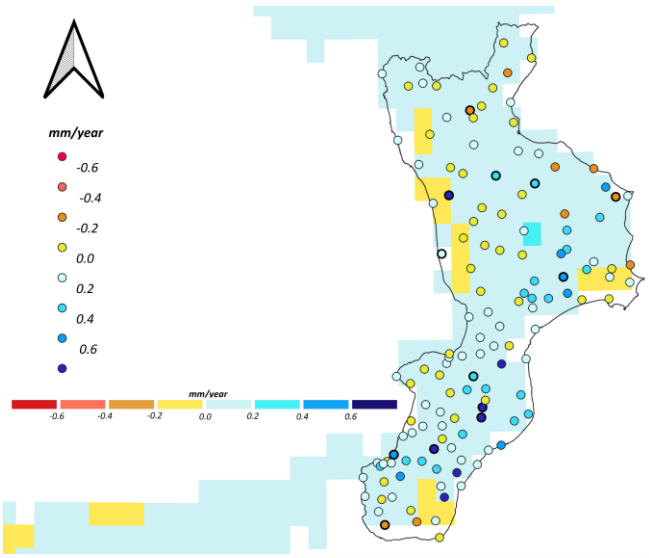
We will emphasize the importance of that study area in the context of the Mediterranean Basin and the selected period simulated in our high-resolution modeling framework, both in the Introduction and Data and Methods sections.

4) The comparison of ERA5-Land and gauge data seems insufficient to conclude that ERA5-Land "can catch the contrasting daily and annual precipitation trends" as well as the "results achieved along the entire northern Mediterranean coast". This is because: (a) no direct comparison was made. Could you provide a direct comparison of the results? For example, a scatter plot of gauge vs. ERA5-Land trends. This could either be done on a gauge-pixel basis, or interpolated gauge-to-pixel-pixel basis. (b) The fact that results are promising over one area (Calabria) does not mean they are like that, or should be like that, over extensive areas such as the northern Mediterranean. If this claim is something you would like to present, please show a wider comparison using gauges from other areas of the northern Mediterranean as well. Daily gauge records are rather easy to find, and thus, this analysis seems manageable. Similar comparisons could be made with other, gridded datasets, considering their limitations (e.g., L228-239).

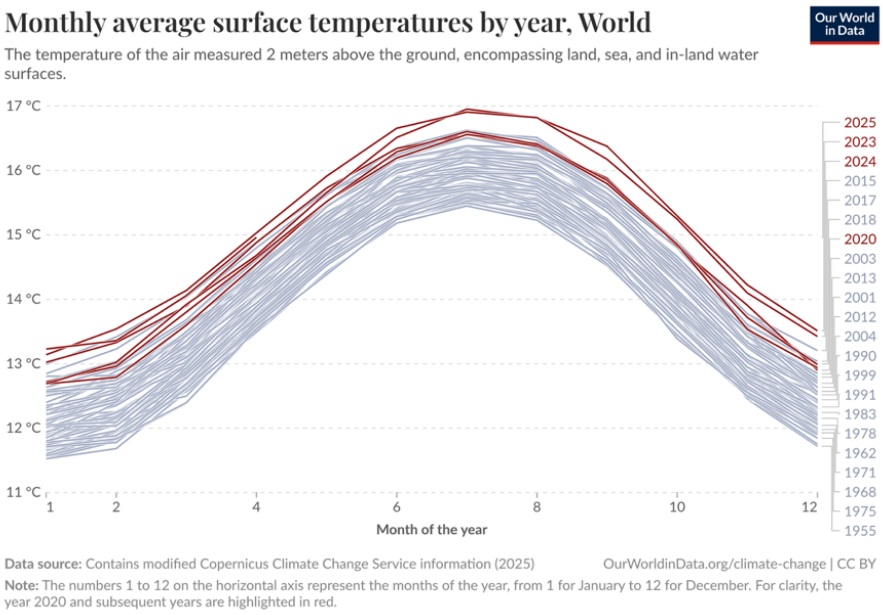
While the analysis suggested in (a) can be easily performed, that suggested in (b) goes well beyond the scope of our paper and requires a specific study devoted to it. We claimed that the results in Calabria “further strengthen the reliability of the results achieved along the entire northern Mediterranean coast”, meaning that Calabria is a case in which the reliability of the ERA5-Land dataset in reproducing trends is demonstrated. We acknowledge that this sentence is misleading and will be modified. Additionally, we will provide more literature results, such as those by Gomis-Cebolla et al. (2023), Oukaddour et al. (2025), and the very recent Beranová et al. (2025), and investigate the possibility of using the recently published dataset from Vicente-Serrano et al. (2025) for comparison, specifically concerning annual precipitation.

As a preview of what will be shown in response to (a), below the overlays of the Sen's slope extracted from both datasets for PRCPTOT (upper panel) and RX1day (lower panel) are shown. Of course, the gauges capture more localized variations (not necessarily representative of an entire ERA5-Land grid cell), and the ERA5-Land dataset provides more smoothed precipitation time series, suggesting the potential for a hybrid approach in precipitation monitoring, which utilizes both gauge data for localized accuracy and gridded data for regional assessments.



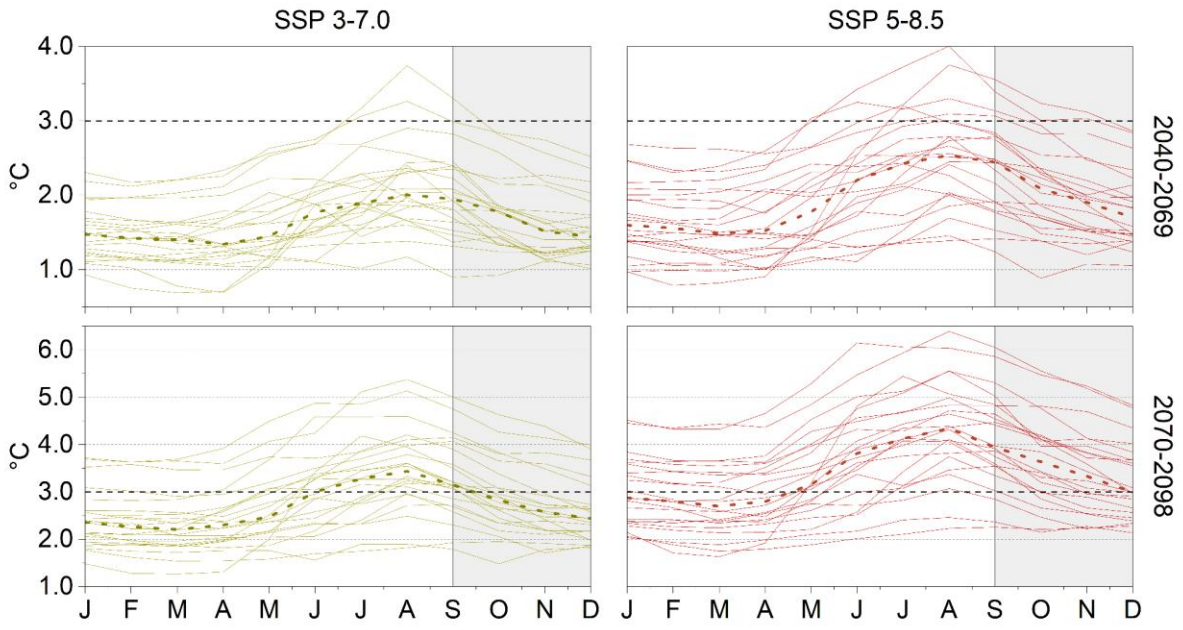


5) Sect. 3.2 and Figures 6 and 7: Given that there is no description of the use of specific CMIP models over specific months, in my view, the details provided in Figures 6 and 7 are excessive. You could provide, for example, a plot like the one below, showing the changes throughout the year. If the specific models are needed you could plot them as spaghettis, and if they are not needed, a shaded area would be more helpful. This could be done for the actual values and for the difference ("delta") values, and if it looks alright you could even provide the two SSP scenarios on the same plot, thus, minimising the figures to only two panels. More generally, it seems like the whole purpose of this section is to justify the use of the specific numbers which were then simulated, which is alright, but you could provide these numbers with a symbol on a graph (e.g., add the 2019 values and the -1, 0 and +3 values to the "delta" panel plot) and make the text much shorter (one paragraph).



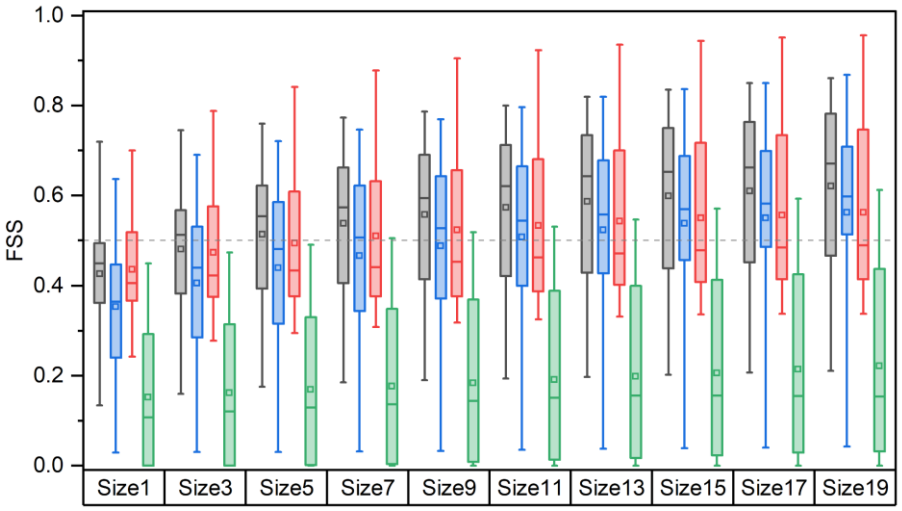
We sincerely thank the Referee for this suggestion. This concern was also raised by the other referees (Referee #1, Results and Discussion comment no.6, Referee #3, comment no.5, and,

partially, Referee #4, minor comment no. 15). Following Referee'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, considering SSP3-7.0 and SSP5-8.5 scenarios, respectively. Additionally, according to Referee #1's comment, the area on which we will base our calculations is no longer the entire Mediterranean basin, but the external domain D01. The new figure is 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.

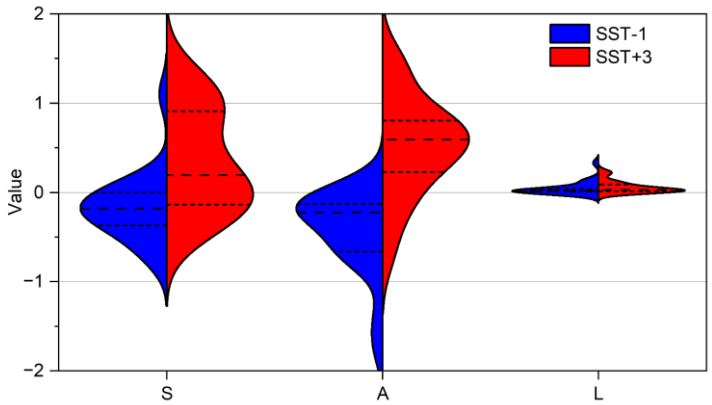


6) WRF event simulation validation. In the FSS analysis, you use the 95th percentile, which is quite high, and get rather low values. While you mention that 0.5 "typically indicates valuable skill", actually it is 0.5 + the occurrence of half of a random forecast (Roberts and Lean, 2008), meaning that almost none of your forecasts are "useful" as termed by Roberts and Lean (2008). Even if we take the small portion of useful forecasts, they lie mainly in the larger averaging domains (e.g., "Size 19"), so the forecast is useful for 180X180 km and more, which is quite a bad statement. Please consider using the FSS with a lower threshold, unless you're specifically interested in pinpointing the really high precipitation accumulation values. You could, for example, take the 50th percentile out of the "rainy" pixels (e.g., where rainfall accumulation is greater than 1) or something similar. That's just an example for a threshold that I see as valuable but other thresholds could also be used. The thing is that taking the 95th percentile means you are trying to forecast extreme values and you actually show that this prediction is bad. Additionally, when describing FSS and CSI scores for the SST-1 and SST+3 simulations you are showing validation of events which we do not expect to be similar to the observed. Therefore, the meaning of the validation of this forecast is vague. You can, however, suggest that lower scores indicate rainfall patterns had changed between the events, but this is sort of a weird way to examine it. A simpler way would be an application of a metric that measures similarity between precipitation fields, for example the SAL analysis (Wernli et al., 2008) where SST0 serves as the "truth" while each of the other scenarios is examined against it.

We thank the Referee for pointing out several aspects of the verification strategy. Our statement of FSS value greater than 0.5 could be misleading, as it should be a valuable skill above 0.5 + the occurrence of half of a random forecast. Indeed, how to use the FSS at its best is an issue still under investigation (e.g., Antonio and Aitchison, 2025). Following the Referee's suggestion, we have relaxed the percentile thresholds by using the 90th percentile, which was also employed by Senatore et al. (2020) and Antonio and Aitchison (2025). However, since we consider 20 different events, we will just highlight the 0.5 threshold by inserting a dashed line in the new figure (which is shown below). The revised figure highlights that even with relatively small window sizes (i.e., not only size 19), SST0 forecasting is skillful, and the importance of both proper SST and high-resolution representations is also critical (i.e., results with SST-1, SST+3, and ERA5-Land are poorer).

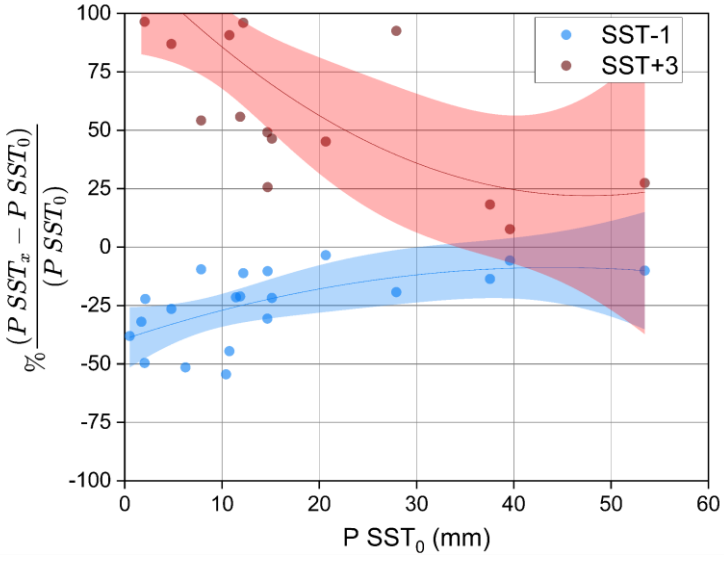


While we believe it makes sense showing SST-1 and SST+3 results compared to observations (for the reasons exposed above), taking up the Referee's suggestion, we conducted also a SAL analysis (Figure below, where the dashed line indicates the median, while the dotted lines highlight the 1st and 3rd quartiles, respectively). Since the reference simulation is the SST0, and this analysis highlights the effect of increasing or decreasing the SST, we will add such a graph to Figure 12 to emphasize the effect of the SST scenarios.



7) L327–337 and Fig. 13: Event #12 shows quite a lot of precipitation over the Tyrrhenian Sea and even more over the Calabrian peninsula. From the accumulated precipitation map (Fig. S12) it seems like there is a significant orographic enhancement of rainfall during the event. At the same time, it seems like there is quite a low correlation between the CAPE values presented (from the night before the peak of the event) to the accumulated precipitation map. This requires elaboration: either the night before is not a good representative time step, or CAPE is not such an important indicator for precipitation during the "actual" event. If the latter is the case, why should CAPE be a good indicator for the changes occurring between the three different scenarios? Did you consider examining the moisture flux perpendicular to the coastline/mountain ranges? Could you think of other mechanisms explaining this shift towards the Ionian Sea? How about the rainfall across the entire domain, does it go up or down in this event? A graph would help, so please consider adding another panel to Fig. 11 with precipitation over the entire domain. Can this shift be arbitrary? From Fig. 12 it seems like the shift is consistent, but the explanation is not clear to me. Can it be related to a shift of the cyclone center toward the Ionian Sea? If so, why? In my view, to better explain this part, a more detailed analysis is needed. Last note about this topic, "Negative values of omega, related to vorticity advection (e.g., Lenderink et al (2017))" — such values are not necessarily the result of vorticity advection, and are important indicator for precipitation even without vorticity advection. Also, if I remember correctly, the vorticity advection topic is not discussed in Lenderink et al (2017).

We agree that the orographic enhancement is a key factor in the precipitation development for event #12. We believe that the increasing CAPE shown in Figure 13 effectively highlights both the thermodynamical and dynamical aspects of our simulation results, emphasizing the increasing atmospheric instability over the Ionian Sea, with the humid air mass coming from the south and being fed by the increasingly warmer sea surface, changing significantly the cyclone track and the spatial pattern of precipitation. We first reported this result in a paper published more than 10 years ago (Senatore et al., 2014). However, we will strive to strengthen our findings further and explore the suggested analyses by computing the vertical moisture flux perpendicular to the mountain ranges for the three scenarios. Concerning rainfall across the entire domain, of course, it also increases for event #12, as is the case for all the analyzed events. This outcome is already shown in Table A1 and further illustrated in the figure below (which will be included in the paper, as suggested).



Finally, regarding omega, we agree with the reviewer that the phrase “related to vorticity advection” is misleading and will be removed. Additionally, we agree that the reference to Lenderink et al. (2017) is incorrect in this context.

8) Increase in extreme precipitation despite overall drying. The authors claim to investigate the intensification of heavy precipitation in the context of overall drying over the Mediterranean. However, the analysis seems to show that precipitation is increased across almost all rain events within the examined season in SST+3 and the opposite for SST-1. This raises questions about the apparent contradiction between their results and the broader drying trend they show and refer to. I would like to see some discussion explaining why this is the case and whether the results actually show the intensification of extremes, or rather they show a general intensification which is related to rising SST. If the latter is the case, this should be explained, and the discussion could describe the contradiction by suggesting alternative reasons for the overall drying, like reduced cyclone frequency (Zappa et al., 2015), shortening of the rain season (Hochman et al., 2018), decreasing land-sea gradients (Tuel and Eltahir, 2020) and a decrease in the area and duration of rain events (Armon et al., 2022).

This comment appears to be related to the general comment (c). We reiterate here that, in the manuscript, we stated that “each event was considered as standing alone with respect to others” (L179) and “can be considered as isolated” (L347), while, among the information missing in our PGW scenario, “the expected frequency of cyclones, given the large-scale circulation dynamics changes induced by climate warming, is another relevant piece of information missing” (L349). Therefore, our results support a scenario with “the intensification of extremes”, but cannot support “a general intensification which is related to rising SST”. Besides the reduced cyclone frequency, all the alternative/complementary reasons listed by the Referee, accompanied by the respective literature references, are valuable suggestions for us to enhance our discussion.

Specific comments

L18-L28: The first paragraph is very general on the one hand, but puts a lot of emphasis on the expansion of the Hadley Cell on the other hand. While this is nice as a general introduction, it does not fully lead the reader to a better understanding of the background related to this specific study. I would suggest either being more specific — i.e. what is special about the climate change of the Mediterranean, or adding other examples to what governs climate change effects over the region in addition to the expansion of the Hadley Cell.

We thank the reviewer for this comment. While we (as suggested) will keep the issue of the expansion of the Hadley Cell, we will be more specific about climate change issues in the Mediterranean Basin.

L34-36: The claim for increased cyclonic activity is **not supported** by the cited reference. Aragao and Porcu's (2022) claim is that their algorithm produces 40% more cyclones compared with other cyclone detection algorithms. Please revise. Please correct also the connection to medicanes. Medicanes are indeed formed in the Mediterranean and lead to destruction, but this is not related to the fact the number of cyclones in the Mediterranean is increasing (which, in any case, is not supported by the reference cited).

We thank the Referee for this very timely comment. The reference to Aragao and Porcù (2022) will be removed, and the entire sentence rewritten.

L105-106: Did the authors apply the mentioned techniques? If so, would you please elaborate? If not, please describe better the procedure and mention who is responsible for it. Could this procedure change the value of extreme events?

Yes, we applied the mentioned techniques to estimate missing values and reconstruct detected outliers in a large dataset concerning both temperature and precipitation in Calabria. To be precise, concerning precipitation, we only used linear regression; therefore, the text will be modified accordingly. The technique was applied as follows:

- First, daily rainfall data were acquired for each station from 1955 to 2023. If less than 15 days per year were missing (approximately 4%, such as proposed in several studies, e.g., Aguilar et al. 2005, Donat et al. 2013 and Stephenson et al. 2014) the PRCPTOT and RX1day values were calculated for that year.
- If more than 15 days of data were missing for a given station in a specific year, we performed a linear regression between the available data of that station and those of a nearby station whose data were highly correlated (in all cases, we achieved *r* values greater than 0.8, often much greater). Then, we used the linear regression equation achieved to fill the gaps.

Although this procedure could potentially influence the values of extreme events, it was approached with careful consideration of the data's characteristics.

L123: To my view, there is not much of a comprehensive overview of the dataset in Table 1. Rather, there is a list. Please delete the text in the parentheses except for the words "Table 1".

We will modify the manuscript according to the Referee’s comment.

L133-134: " Non-parametric trend tests like the... were employed" — please be specific. Are these the only tests or are there others?

We used only these tests. The sentence will be modified to make the text clearer: “The non-parametric trend tests of Mann-Kendall (identifying significance at a 5% level) and Sen’s slope estimator (determining the trend slope per year) were employed to analyze…”

L195 (and L208): " resulting in more probable flooding challenges across Europe (Fig. 2b)." This claim is problematic since greater RX1day does not necessarily mean more probable flooding challenges, as floods come in different flavours. See for example Bloschl et al. (2019).

We understand the point raised by the Referee and will make these sentences smoother. Regarding Bloschl et al. (2019), it is noteworthy that the analysis is limited in several Mediterranean areas due to the (sadly chronic) lack of observations. E.g., the graph of the uncertainty of the river flood discharge trends in terms of standard deviation (Extended Data Fig. 2b) exhibits a sharp latitudinal gradient in Greece and southern Italy.

L196: " These results are largely consistent with previous literature." Is there literature showing ERA5-land trends over the EURO-CORDEX region? If there is, this should have been mentioned in the introduction. If there is not, it is better to explicitly mention the "previous literature". In that case, the introduction misses a description of what was not done before which you are showing here (e.g., calculating trend in ERA5-Land versus trends in other models).

We referred to previous literature regarding the trends achieved, either by using or not using ERA5-Land. We considered the reference to the 6th IPCC Assessment Report sufficient since it is a sort of summary of many other works. However, we will contextualize the sentence better, both here and in the introduction. Interestingly, a very recent paper from Beranová et al. (2025) provides results very close to ours concerning winter and summer precipitation trends in Europe in the period 1961-2010 (by the way, another novelty of our research is the significantly updated dataset used, with 2023 as the final year).

Figure 3: Could you add axes labels to the small quadrant graph? This would make it much easier to read (rather than remember) what every zone represents. A different approach would be to incorporate all zones into one graph but vary the colours. Could you try this approach in order to minimise the number of panels for this figure?

We sincerely thank the Referee for this brilliant idea. Figure 3 will be updated as shown below, with different colours representing the four quadrants. Additionally, as noted in comment no. 2, we will focus on the Mediterranean. Finally, a specific figure in the Methods section will be added to explain the meaning of the four zones (please refer to our reply to Referee #1, Data and Methods comment no. 2).

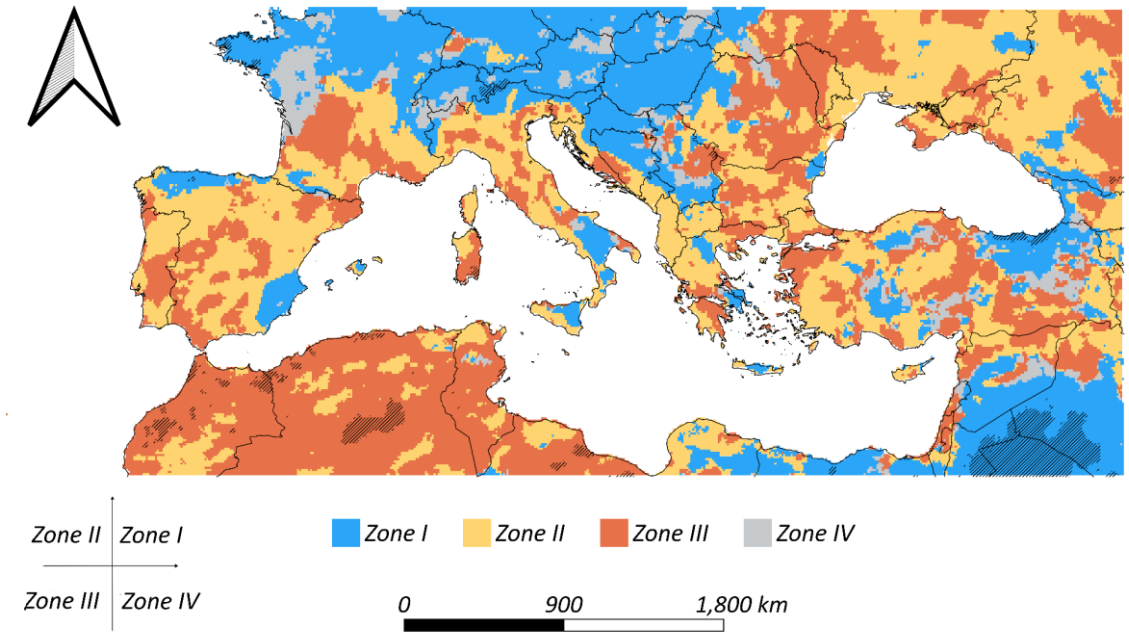
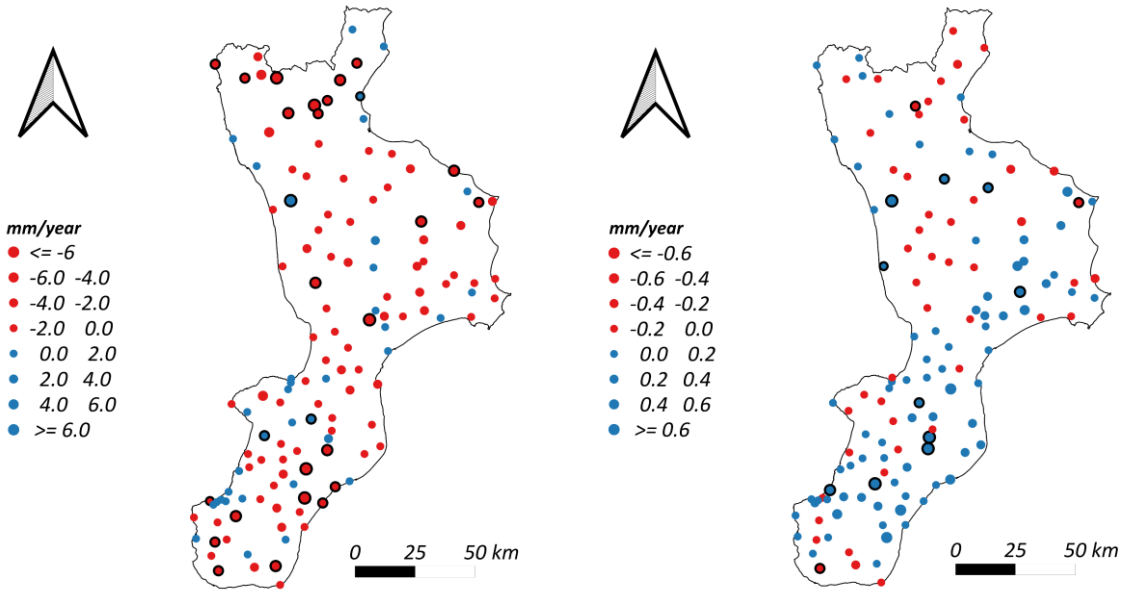


Figure 4: The resolution of the figure is too low. Please provide a better resolution version of it.

Figure 4 will be modified and enhanced with improved resolution, as depicted below (left panel for PRCPTOT and right panel for RX1day trends in terms of Sen's slope).

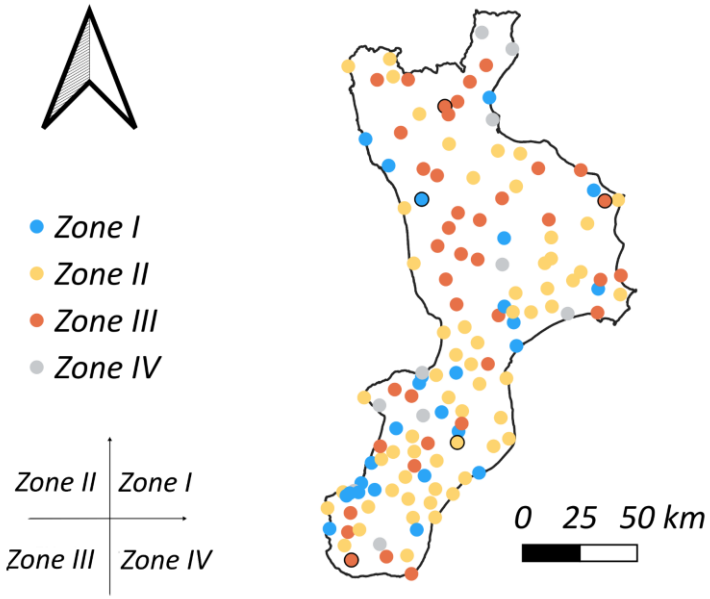


L213-218: The results of significant trends in RX1day should be treated carefully! Considering random processes, we would expect to find more or less 7 gauges (5%) with positive and 7 gauges with negative trends. Please make sure to address this. In contrast, L219-227 discuss mainly the non-significant trends. There, a note should be highlighted saying that these are non-statistically-significant results.

We agree with this comment. The text in LL213-227 will be revised to address the issue of the low number of RX1day significant trends and the fact that mostly non-significant trends are discussed.

Figure 5: I would suggest the same as in Fig. 3 — combine all quadrants and vary the colours.

Similar to the comment related to Figure 3, Figure 5 will be modified as follows.



L240-241: This claim is not clear to me. Please explain.

This comment appears to be related to the general comment (b). The statement emphasizes the representativeness of the study area within the broader Mediterranean context, which, as the Referee states, is supported by reasonable arguments. Furthermore, we highlight the availability of a dense and reliable monitoring network. In other words, this region can provide insights into how local climatic conditions can impact broader regional trends.
This statement will be revised and further clarified in the manuscript, in accordance with our reply to the general comment (b).

L254: Please explain what is the value which the "±" sign refers to (is this the range, standard deviation, 10–90 quantile range?)

The values reported with the symbol ± represent the standard deviations for both the SSP3-7.0 and the SSP5-8.5 scenarios. We will clarify this in the revised manuscript.

Figure 9 and L276–277: Could you provide a reference to another figure, preferably Fig. 1, where you show the area over which you do this spatial interpolation and its boundaries? Is it the same area for the observations and model? What type of interpolation are you using?

We interpolated over the Calabria region boundaries, shown in Figure 1a, using the 150 rain gauge stations highlighted. The description of the methodology used for spatial interpolation is given in LL110-113 and involves IDW (Inverse Distance Weight). We will include a reference in the revised manuscript to the section mentioned above.

Additionally, some events seem to have a less skilful simulation, especially in terms of the spatial distribution of rainfall, for example, event #2. Could you provide some details why that is the case while others are simulated almost perfectly?

If we have understood the question correctly, a detailed answer would likely exceed the scope of this paper. In this context, we can provide some general reasons, including the correctness of the boundary conditions, the difficult predictability of local convective events, and the overestimation of the orographic effect by WRF simulations in certain situations (in the case of event #2 it seems to occur in the central-southern part of the region). Furthermore, in the case of event #2, the Calabria region was at the very border of the transit of a cold front. In any case, we prefer not to distract the reader from the paper's main topic.

Figure 10. If I understood you correctly, boxplots represent the variability across the 20 different events. Is that correct? Please describe this in the text. Furthermore, the labels "Size1... Size19" are not clear. Is this when averaging across 1 pixel, 3 pixels... 19 pixels? Please explain.

The Referee understood correctly that the boxplots represent the variability of the 20 different events. As far as the FSS is concerned, the size represents the number of cells considered in the moving windows. In the revised manuscript, this information will be included in the figure caption. Regarding the SAL, we used a violin plot, which will be added to Figure 12, as it highlights the difference between the SST scenarios and SST0, as suggested in major comment no. 6 by the Referee.

Figure 11. What is symbolised by the shaded area? What is represented by the line? Please explain. Also, could you add a panel showing the (non-normalised) linear regressions discussed in L299–L310? Additionally, when presenting values in a normalised axis, it is common to present it in log distances, such that e.g., a decrease of 50% has the same distance from 0 change as an increase of 100%.

The shaded area represents the 95th percentile confidence level performed for the 2nd-order polynomial fitting (i.e., the line) over the two different scenarios. In LL299-310, only one couple of linear regressions is discussed (L300). Probably the Referee also refers to the other two couples of regressions (L297 and L318). We feel that this panel could be redundant, given Table A1. However, we will consider the opportunity to add the graphs. Regarding the representation in log distances, we've made various attempts to improve the visual format of the graph and concluded that log distances do not add any benefit (i.e., they do not highlight any specific feature) while making the graph less readable.

L342–343: Could you explicitly write what you mean by those features? The authors' analysis is focused on precipitation accumulation, rather than intensities, and " tracking" is not clear to me in this context. "Cyclone tracks" is further mentioned in L401, which is again, not clear to me.

We agree that the terms used may be misleading and imprecise in this context. We referred to "peak intensity" because the intensity of the precipitation is reasonably very high in the center of mass of the accumulated precipitation (but not necessarily the highest), and to "tracking" because the locations of the centers of mass of the accumulated precipitation are related to the modified cyclone tracks. We will rewrite these sentences in a more precise way.

L355–357: Similar conclusions were made earlier by Zappa et al. (2015). Please consider citing them.

We thank the Referee for this valuable suggestion. We will add the reference in the revised manuscript.

L387: For Storm Daniel, please consider using more relevant literature focusing on the meteorology and hydrology of the event, e.g., Armon et al., (2025) or Flaounas et al. (2024).

Once again, we thank the Referee for their valuable suggestions. We will revise the suggested references and add them to the revised manuscript.

L405–407: This was shown before, over many different studies (e.g., Prein et al., 2015; Ban et al., 2014; Pichelli et al., 2021; Coppola et al., 2020).

We agree that the sentence, in the way it is written, could sound a bit trivial. On the other hand, the topic remains currently highly debated (e.g., Soares et al., 2024; Fosser et al., 2024). Furthermore, we noticed that the main features of precipitation change can be observed already by examining the outer (lower resolution) domain (reply to Referee #4, comment no. 8). We will revise the sentence to take into account new insights, contextualize it better, and consider the suggested references.

L411: Since many studies have already used PGW with more parameters changed rather than only SST, this sentence is not clear to me. Please consider revising this statement. Also, the statement in L413–415 is not in place here; it should either go in the discussion or be deleted, because other factors are competing with it, such as soil drying because of longer dry spells.

We will revise this sentence accordingly. Concerning this point, useful insights are also provided by Referee #4 (comments nos. 2, 3, and 4). However, it is also of interest to us to refer to “complete” global warming scenarios to provide the whole picture, as we are currently working on convection-permitting climate simulations in the study area (https://doi.org/10.5194/egusphere-egu25-15936), which represent an area of future investigation for us. The sentence at LL413-415 will be removed.

Data availability: The netcdf files contain accumulated precipitation, but the 'Times' (at least for the SST+3) vector is corrupted (it contains '0' only). Additionally, there is no spatial information except for the number of grid cells. If you can, it would be very helpful to accommodate the real times in the vector and add information about the coordinates of the data, e.g., lat/lon vectors or arrays.

We thank the Referee for noting that and will add the time strings and the lat/lon arrays in the data uploaded to the Zenodo repository.

Technical corrections

L40: "contributes to lead" — please stick with either contributes or lead.

We will revise the manuscript according to the Referee's comment, and use lead.

L42: "trigger dynamically" — please revise. You could, e.g. use "dynamically interact with orographic lifting" or something similar.

We will modify the manuscript accordingly.

L60: "as the Ianos cyclone occurred in 2020, producing" would be more readable if you expand the sentence like "such as cyclone Ianos that occurred in 2020, which produced..."

We will modify the manuscript accordingly.

L76: "events occurred" please expand to "events that occurred".

We will modify the manuscript accordingly.

L97: "lies" should be "that lies".

We will modify the manuscript accordingly.

L295: "evapotranspiration" should probably be "evaporation".

We thank the Referee for pointing out the typo. We will modify the manuscript accordingly.

Fig. 12: Please correct the "Tyrrenian" label in Fig. 12 to be similar to what's written in the text i.e., "Tyrrhenian".

We thank the Referee for noting that. We will modify the figure accordingly.

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