Anonymous Referee #2

This manuscript describes a number of long-term simulations with a convective-permitting model over southern France focussing on its ability to reproduce extreme precipitation events in fall. The study, in which several model runs covering a large number of years with forcing data from different global climate models, is clearly interesting and results indicate that this model is more suited to simulate such events compared to standard high-resolution RCMs. As such studies are sparse this one can be an important contribution that merits publication.

Response: Thank you for the thorough review of our manuscript. We appreciate all the constructive comments and suggestions that you provide in your review, and that our work is valued by the reviewer.

However, the manuscript first needs some improvement. This is partly related to the language, I would suggest a thorough language check before re-submission. It can also be clearer what has been done (exactly which model version has been run?) and why (the choice of time periods including the mismatch with the observations used for evaluation). It is also not clearly explained how this high-resolution convection-permitting model performs when driven by perfect boundary conditions. As it is now, it is not clear if biases are related to poor forcing conditions (including wind, stability, SSTs) from the GCMs or if they result from poor model performance. The same is true for the coarser-scale EUR-11. The results in some of the figures are not entirely in line with how the text describes them.

Response: We will revise the text carefully before the re-submission.

We have mentioned in our preprint line 81 that the WRF-ARW version 3.8.1 is used to perform downscaling experiments in our study.

- We selected the two periods of 1951-1980 and 2001-2030 for our study because of two main reasons: 1) the forcing EURO-CORDEX of our simulations started in 1951, so we cannot go further past. 2) We would maximize the difference in Greenhouse gas concentrations between the two periods while the simulations are not going two far to the uncertain future scenarios for which we do not have the observations.
- For the mismatch between simulations and the benchmarks, we will clarify in your specific comment.
- As we stated in line 89 in our preprint that we will use the simulations generated in this study for further anthropogenic climate change impact investigation, we decided to perform our downscaling for EURO-CORDEX/CMIP5 runs rather than forcing our runs by perfect boundary conditions. In fact, we have done a few short runs (3 months) forced

by ERA-Interim for the purpose of testing our CPS domain position. These results were provided in the supplementary section. We also keep in mind that several studies that we mentioned in our introduction had done convection-permitting simulations forced by ERA-Interim using different RCMs and focusing on different areas. And they found the advantages of this approach in replicating extreme precipitation events. Their findings fed our idea that we can step further in this field of modelling by running CPSs in climate scale and forced by CMIP5 boundaries. We will discuss our experiments with reanalysis forcing in the text.

- We will discuss the performance of forcing GCMs (especially SSTs) in another specific comment below.
- We will check and correct where the text does not describe the Figures correctly.

As for the structure of the paper, I find that there is no proper discussion of the results. Currently, there are some references alluded to and compared with both in the result section and in the conclusion chapter. I think that the discussion should go into either the results section or be introduced in a separate chapter of its own. Furthermore, the supplementary material is interesting and I'm thinking that it may be useful to include directly in the paper instead (it could be part of the discussion), the paper is not that extensive in its present form.

Response: We will discuss our results and tie the results to literature in the results and discussion section. For the supplementary section, we prefer to keep those materials in the supplementary. Because all figures were provided in similar styles as those in the main manuscript that may confuse the reader if we mix the two parts together. But as mentioned above, we will discuss the results from experiments forced by reanalysis on the main text of the revised manuscript.

Specific comments:

RC: Line 22 Please explain what is meant by "cloud-resolving". Most convective clouds are smaller than 3x3 km and are definitely not resolved by the convective-permitting models used here.

Response: Our simulations are "convection-permitting". However, in line 22, we mentioned "cloud-resolving" simulations in a general context. This was not implied for our model.

RC: Line 31-37 Now also shown for higher latitudes in Scandinavia (Lind et al., 2020, see https://link.springer.com/article/10.1007/s00382-020-05359-3)

Response: Thank you for mentioning those new interesting results. We will add this to the reference.

RC: Line 42 I would not use "large" and "robust" here to describe the number of simulations done and the status of knowledge. The number is in fact highly limited and only for a few regions mainly covering parts of the mid-latitudes.

Response: We replace "a large number of" by "a few" and remove "robust" from the text.

RC: Line 56-57 Instead of referring to a project (HyMex) I think it is more interesting for a reader to learn stg on what scientific questions are being addressed and/or why this is interesting from a societal perspective (the references given may be good here but I don't see the need for introducing the project).

Response: We will replace a sentence starting in line 55 by 3 sentences providing the motivation why the Mediterraen region has been receiving more interest and specific scientific questions are being addressed by research communities.

"*The coastal regions along the Mediterranean frequently undergo heavy precipitation events in the autumn, which lead to flash floods and landslides causing massive losses and damages (Delrieu et al., 2005; Fresnay et al., 2012; Llasat et al., 2013; Nuissier et al., 2008; Ricard et al., 2012). In addition, this area is considered as a hotspot of climate change that strongly responds to warming at global scale (Giorgi, 2006; Tuel and Eltahir, 2020). As a result, the Mediterranean has received an increasing scientific interest in understanding mechanisms leading to flood-inducing heavy precipitation as well as in improving the model ability to predict and project those events in a complex changing climate that provides substantial support to adaptation and mitigation for society (Drobinski et al., 2014; Ducrocq et al., 2014).*"

RC: Line 87-88 This is unclear. Why is the model run for 1951-1980 and not 1961-1990? Later results are compared to observations from 1961-1990 and even if the results are from GCM-driven simulations there are forcing differences between these periods potentially compromising the comparison. This should be addressed in the paper. Furthermore, the use of 2001-2030, that is mostly based on a future scenario (RCP8.5) is also not clearly explained and in a way difficult to understand. In a similar way, comparison is done between observations covering 1998-2017 and model simulations covering 2001-2030. Again, there is a mismatch of more than 10 years in a period with a strong global mean change. Is there any implication for the results from this

(mismatch in extremes as simulated in the 2020ies with stronger forcing compared to the previous 20 years)?

Response: All simulations in this study are designed for evaluation of convection-permitting setup and further investigation of impact of human-induced climate change on current climate (i.e. 2001-2030) and historical climate (i.e. 1951-1980). For the latter purpose, we aim at maximizing the climate signal by selecting the two periods with the distance as large as possible. This explains why we selected 1951-1980 as a historical climate.

We then evaluate simulated daily precipitation indices of 1951-1980 against in situ observations and SAFRAN for 1961-1990. Our observations contain a few stations starting in 1951, while other stations start a few years later. In order to make a homogeneous length among stations and SAFRAN, given that SAFRAN starts in 1961, we select the 30 years period of 1961-1990 as a benchmark.

We also compare 3-hourly precipitation indices from 30 years (2001-2030) of simulations against 21 years (1998-2018) from in situ observations. For this set of observations, we only have those 21 years, therefore we cannot use them to evaluate our historical simulations. Note that our daily observations and 3-hour observations are two separate datasets.

RC: Line 91 Here, Cévennes is mentioned for the first time. For the not-so-very-French reader it is not clear where these mountains are. It becomes clearer when looking at subsequent figures. But, it would be good already in Figure 1 to illustrate where these mountains are (as part of the Central Massif – I guess?). Also the "Cevennes-box" could be given there. (Reference could also be given to this figure on line 141 where the box is detailed in the text).

Response: In the preprint, the Cévennes is mentioned for the first time in line 76. Indeed this mountain range is the southern part of the Massif Central in the south of France. We will state more clearly in the text and highlight it, and the Cévennes box Figure 1.

RC: Line 100 What is "the French Mediterranean Sea"? Is this stg outside of the territorial 12 nm zone?

Response: We mean our domain covers a large part of the Mediterranean that adjoins the French coast. We will change those to "*the French Mediterranean region*".

RC: Line 105-109 It is not clearly described in the paper how the current configuration of WRF 3.8.1 performs w.r.t. the observed precipitation extremes in any ERA-Interim driven simulation.

The supplementary material holds such ERA-Interim driven simulations, however, it should be better addressed at some point in the paper how this model (and the currently used setup) works. Also, why are these particular schemes mentioned here and not others? Is it clear from reading these few lines exactly which version of the model that has been used? Could someone else reproduce your experiment based on what is written here? On line 106 it says SSTs are updated every 6 hours. Is this also true for the lateral boundary conditions?

Response: 1) In the simulations at climate scale, we used the configuration similar to what was used in experiments driven by ERA-Interim. We will mention this in the main text of the manuscript. **2)** We mentioned in the text those schemes that have direct impacts on the development processes of precipitation and temperature. Those schemes are consistent with what were used in EURO-CORDEX. **3)** We mentioned clearly in line 81 that we used the WRF-ARW version 3.8.1. The simulations generated in this study can certainly be reproduced based on the information given in the text and additional configuration information of WRF that was used in the EURO-CORDEX experiments (Coppola et al., 2020; Vautard et al., 2020). **4)** We update the SSTs every day, which is consistent with the EURO-CORDEX experiments using WRF model. We will correct this information in the main text.

RC: Figure 1 Why is there no altitude associated with Menorca and Ibiza on the map. Are these islands not resolved by the model?

Response: Those islands are not represented in topo data of WRF.

RC: Line 146 Unclear what "few" means here. Is it only a few time steps from the 6480 time steps (27 hours times 60 minutes times 4 time steps per minute)? Or do you mean that the 6480 time steps are few relative to the full length of the simulations?

Response: We meant a few time steps with an interval of 3 hours for model simulations and 6 hours for ERA5. We will clarify in the article.

RC: Line 157 A reference is missing for ERA5.

Response: Added

RC: Line 227-240 I don't fully agree on the interpretation of the figures including the temperature intervals given in the text here. For instance, in Fig 4a I think it is quite clear that the approximate

CC-scaling holds between 3-13C. Between 13 and 18 there seems to be no such relation, but rather a constant precipitation rate regardless of temperature. Similarly, for 4b I think the super-CC scaling applies up to approximately 13C whereafter CC-scaling applies. Furthermore, it is not clear that the models reproduce the behaviour, in some aspects yes but not in the details. The slopes do differ. Also, the slopes differ between EUR-11 and CPS model versions (e.g. HadGEM). This text needs revision. I also think it would be easier to follow if the figure was remade so that the corresponding EUR-11 and CPS simulations (driven by the same GCM) where colored in the same way (suggested to be denoted with full and dashed lines).

Response: We have updated our routine in this scaling analysis by adding a threshold of at least 300 data points to take a bin into consideration. By doing so, a few bins in the lowest and highest temperature ranges were eliminated, therefore we can avoid the artificial effect of under-sampling. We also adjusted colors and lifestyle following the suggestion of the reviewer. The updated analysis shows that observed scaling follows the C-C relation in a range of 2ºC to 13ºC for daily precipitation (see Fig.1 at the end of this document by which the Figure 4 in the preprint will be replaced). The behavior of each convection-permitting simulation replicates its driving EURO-CORDEX model for the daily precipitation scaling analysis. Specifically, the 2 downscaling simulations of the IPSL-CM5A-MR reproduce roughly the C-C relation in a range of 9ºC to 17ºC, while the 2 downscaling simulations of the HadGEM2-ES follow the C-C in range of 5ºC to 13ºC. The 2 simulations of NorESM1-M show similar behaviour that follows the C-C in the range of 4ºC to 14ºC. The overall scaling rate from EUR-11 simulations are close to observations, while CPSs slightly overestimate this rate. For sub-daily precipitation scaling with temperature analyses, observations show a super C-C relation in the temperature range of 6ºC to 13ºC. The 3 CPSs can reproduce this feature, while the 3 EUR-11s completely fail to provide both super C-C and C-C relations. Specifically, CPS_IPSL-CM5A-MR shows a super C-C scaling in the range of 9ºC to 17ºC. The CPS_HadGEM2-ES and CPS_NorESM1-M follow super C-C in the range of 5ºC to 17ºC and 7ºC to 14ºC, respectively.

RC: Line 243-244 Here it says that "We could explain ... underestimation by the fact … simplified cloud process". I don't see how this is explained here! Please be more explicit.

Response: We will modify that sentence by " *... underestimation by the fact that the resolution of EUR-11 is insufficient to reproduce the more localized extreme events and* that the convection scheme … cloud process *by statistical distributions and imposing assumptions of quasi-equilibrium with large-scale forcing (from grid points), approximation of moist air entraining in the updraft, and representation of all single cloud elements by sole steady state updraft of the*

whole cloud ensemble (Houze, 2004; Lenderink and Attema, 2015; Prein et al., 2013; de Rooy et al., 2013)".

RC: Line 262 This section about moisture sources would be a good place to say stg more explicit about the underlying GCMs. From the figure it appears that the Hadley model have a better representation of the moisture flux over the southern parts of the Mediterranean in association to the events examined. Another feature that could be addressed would be SSTs of the GCMs in association with the events. If some of them have strong biases it would likely influence the moisture source and transport. The moisture supply from the sea is of course very important in this aspect (as also shown in a convective-permitting model for this area by Lenderink et al., 2019, https://iopscience.iop.org/article/10.1088/1748-9326/ab214a/pdf)

Response: There are many processes potentially contributing to the better moisture fluxes of the downscaling experiments, including dynamics and sea surface temperature. Here we will simply check the GCM SST biases with respect to the ERA5 for the 12 heaviest precipitation events. Fig.2 of this document shows that the IPSL-CM5A-MR and HadGEM2-es provide warm bias with their mean biases of 0.3 and 0.9ºC, respectively, while the NorESM1-M gives cold bias of 2.2ºC. This can partly explain why the downscaling experiments from NorESM1-M reproduce extreme rainfall over the Cévennes lower than others.

Detailed minor comments:

RC: In general, the manuscript needs a careful revision of the language. There are many examples of errors and/or things that could be clarified, some of which are given below.

RC: Line 8 Change "downscaled" to "run"

Response: OK.

RC: Line 10 Remove the first "simulations"

Response: OK.

RC: Line 24 Change into "is also hope"

Response: OK.

RC: Line 25 Consider changing to "conducting several runs to generate large ensembles of simulations with sufficient resolution"

Response: OK.

RC: Line 45 Instead of "surface field" I would suggest "surface properties"

Response: We agree.

RC: Line 49 Remove "in the simulation results"

Response: OK.

RC: Line 59 Here is an example of a language problem where it says "Z et al found that convective-permitting model outperformed ...". Either it should be "a convection-permitting model" or "convection-permitting models".

Response: We do not see this error 'convective-permitting' anywhere in the preprint version.

RC: Line 72-73 This is difficult to understand. The "analysis" is not "downscaling results of EURO-CORDEX" as it says. Rather the analysis is undertaken on results from downscaling EURO-CORDEX simulations.

Response: We meant that the analysis in this article is made by downscaling "the existing" results of EURO-CORDEX experiments. To avoid misunderstanding, we will change that sentence to "The analysis made here is at a climate scale and is done by *dynamically downscaling the climate information provided by the existing EURO-CORDEX experiments*."

RC: Line 77-78 This is not really needed in this short paper that is quite standard in its structure. In case it is retained it could be explicitly mentioned that there is also supporting material and what can be found there (and why not in the paper itself?)

Response: We prefer keeping those sentences and will mention in the revised manuscript that the experiments done with WRF driven by ERA-Interim are provided in the supplementary material. We will also discuss the result from those experiments in the main text.

RC: Line 84-85 Shorter with ": : :EURO-11 simulations were also done with WRF-ARW version 3.8.1 driven by three general circulation models (GCMs): : :"

Response: OK. The sentence is rewritten as "*These EUR-11 simulations were also done with WRF-ARW version 3.8.1 and driven by three General Circulation Models (GCMs) including the IPSL....*"

RC: Line 128 It is not the "moisture sources" but the "moisture" that is transported from the Med Sea. And on the same line not the "massive moisture" but the "massive amount of moisture".

Response: We will replace the former by "*moisture*" and the latter by "*massive amount of moisture*" following the suggestions of the reviewer.

RC: Line 136 Suggest to replace "zonal and meridional" with "horizontal"

Response: We agree.

RC: Line 137 "hPa" instead of "mb"

Response: We agree.

RC: Line 251-252 I think the () can be removed here. References to the figures are given appropriately in the subsequent text.

Response: We agree

RC: Line 255 Should it be "-45%" here instead of 40?

Response: We agree

RC: Line 263 It is not the ability of the "simulation" but of the "model" that is investigated.

Response: We agree

Please pay attention that the Fig.1 and 2 are put at the end of this document.

Reference

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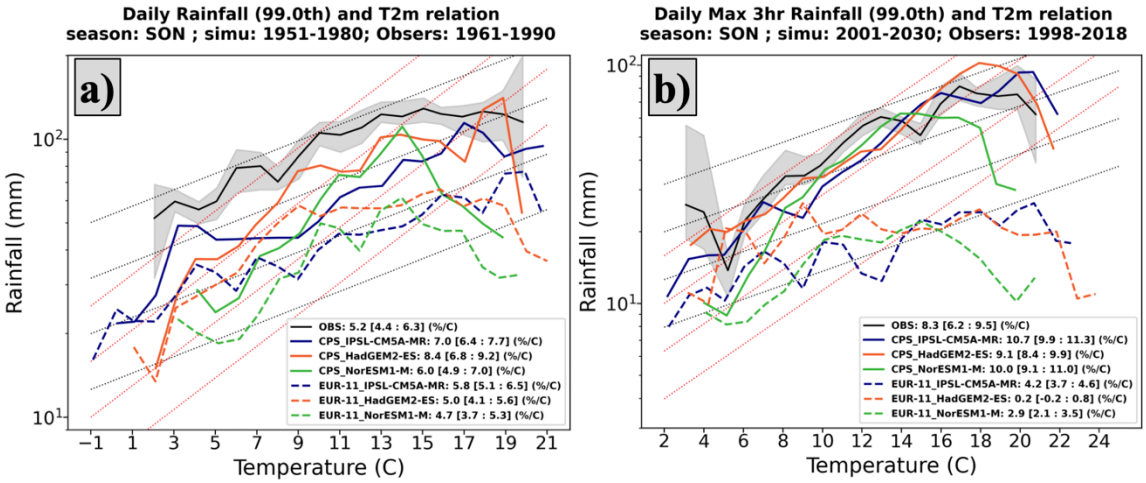

Fig. 1 : Extreme (99th percentile) daily precipitation (a) and daily maximum of 3-hourly rainfall (b) in scaling with daily temperature at 2m from simulations (1951-1980 for daily rainfall and 2001-2030 for 3-hourly rainfall) and in situ observations (1961-1990 for daily rainfall and 1998-2018 for 3-hourly rainfall); the black dot lines show Clausius-Clapeyron relation and the red dot lines show the super Clausius-Clapeyron relation; the grey band denotes 90% confident interval of observational scaling.

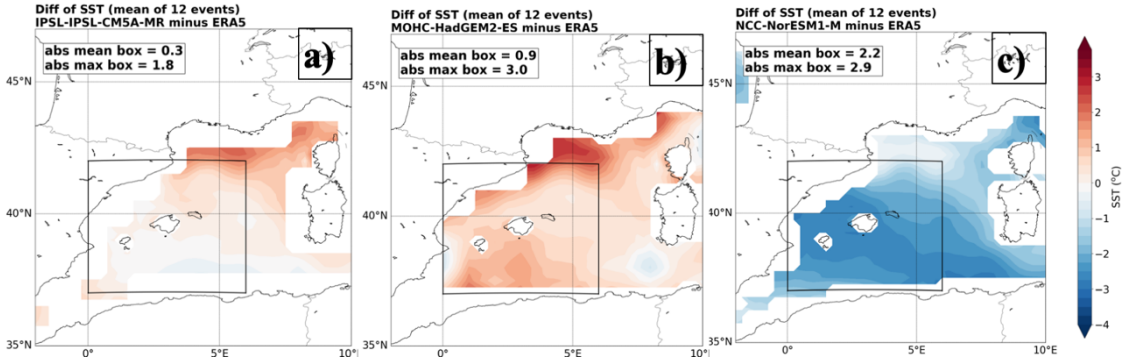

Fig. 2 : Differences in sea surface temperature averaged over the 12 events from the 3 forcing GCMs with respect to the ERA5.