

# ***Interactive comment on “Evaluation of convection-permitting extreme precipitation simulations for the south of France” by Linh N. Luu et al.***

## **Anonymous Referee #2**

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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.

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

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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.

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.

Specific comments:

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.

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>)

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.

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

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is interesting from a societal perspective (the references given may be good here but I don't see the need for introducing the project).

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)?

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).

Line 100 What is “the French Mediterranean Sea”? Is this stg outside of the territorial 12 nm zone?

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

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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?

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

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?

Line 157 A reference is missing for ERA5.

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) were colored in the same way (suggested to be denoted with full and dashed lines).

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.

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

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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>)

Detailed minor comments:

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.

Line 8 Change “downscaled” to “run”

Line 10 Remove the first “simulations”

Line 24 Change into “is also hope”

Line 25 Consider changing to “conducting several runs to generate large ensembles of simulations with sufficient resolution”

Line 45 Instead of “surface field” I would suggest “surface properties”

Line 49 Remove “in the simulation results”

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”.

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.

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

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material and what can be found there (and why not in the paper itself?)

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). . .”

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”.

Line 136 Suggest to replace “zonal and meridional” with “horizontal”

Line 137 “hPa” instead of “mb”

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

Line 255 Should it be “-45%” here instead of 40?

Line 263 It is not the ability of the “simulation” but of the “model” that is investigated.

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Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2020-77>, 2020.

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