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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Systematic errors analysis of heavy precipitating events prediction using a 30-year hindcast dataset" *by* Matteo Ponzano et al.

Anonymous Referee #2

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For this study the authors put a lot of effort in analyzing an operational used weather forecast model with respect to its performance and applicability on heavy precipitation events (HPE) in the Mediterranean region. The authors create a 30-year long 10 member hindcast ensemble using different parameterization schemes for convection and compared simulated HPEs with observations. HPEs are of great importance for that region as they are relatively frequent in the autumn and early winter season. Severe flooding and damaging are related hazards. This study falls within in the scope of NHESS.

The title of this study sounds very promising in giving some real benefits to improve

the performance of numerical models in predicting extreme events. Unfortunately, this is not the case in my opinion and I miss the added value of this study. My fundamental concern with this study is the chosen model. The authors used PEARP, an ensemble using the global model of the French Weather Service ARPEGE. Even though it has an in-model nesting of different grids down to a highest horizontal resolution of 10 km over France, convection is parameterized using known convection schemes as described in the data section 2. But, deep moist convection generates most of the precipitation amounts during HPEs in the western Mediterranean. The global model with parameterized convection is not meant to simulate such events properly. I would have expected an analysis of prediction errors using a higher resolved regional and convection permitting model like AROME or ALADIN, both also run operational by the French Weather Service. Therefore, the authors' conclusions, e.g. that the size of simulated object is larger than in observations but the amplitude is reduced, seem to me quiet obvious and more a consequence of the parameterization, which is already known and nothing new.

Beside my maybe wrong expectation, I do see the point, that the coarser model is cheaper in computation time and therefore it is worth looking at systematic errors, but as there is a trend to more and more higher resolutions for weather forecasts it should be stated clearly what the benefits of the coarse model would be. Nevertheless, the presented methodology is interesting and suitable for such kind of study. Furthermore, analyzing possible systematic errors especially in predicting extremes is also very important and improvements would give benefit to different applications. Beside my main concern above, I have a few major comments and some specific points listed below.

Major comments:

1) The paper is hard to read due to some language deficits especially when it comes to the technical parts. I would strongly recommend a revision on sentence structure, grammar, comma, or word usage.

2) The authors only analyzed precipitation fields and differences between the parameterization schemes for deep convection using the SAL method. A broader look on other quantities like ambient and/or convection favoring conditions is missing. Initial and boundary conditions as well as model physics related to the model resolution have a significant influence on the simulation of convection as presented, for example, in Kunz et al, 2018 (doi: 10.1002/qj.3197), Khodayar et al., 2018 (doi: 10.1002/qj.3402) or Caldas-Álvarez et al., 2019 (doi: 10.5194/asr-14-157-2017). Furthermore, local dynamic pattern also influence the initialization of convection especially in mountainous terrain or on islands (e.g. Ehmele et al, 2015; doi: 10.1016/j.atmosres.2014.10.004), so a misrepresentation of these also lead to distinct differences between model and observations. A third thing are specific weather patterns which have an influence on ambient conditions and convection. Errors or deviations in the model regarding such patterns will also cause a bad representation of HPEs as well. A connection of weather patterns to convection across Central Europe (including France) can be found, for instance, in Piper et al., 2019 (doi: 10.1002/qj.3647).

3) What is the added value of this study? This is the crucial thing of this study and should be strongly pointed out not only but especially in the conclusions section. Additionally, some concrete statements on how to apply the results in terms of future model improvements should be given so that the reader can really benefit from this study.

Specific comments [page line]:

[1 18] '[...] daily rainfall amounts associated to a one single event', 'a single event' or 'one single event'

[2 27] '4) a synoptic system to slow the convective system [...]', I think you mean 'to hold' or better 'to retain'

[2 30ff] Another study analyzing extreme precipitation in the Mediterranean, also both pure convective and convection-stratiform mix, and related mechanisms and processes is presented in Ehmele et al., 2015 (doi: 10.1016/j.atmosres.2014.10.004).

[3 88] 'affected by the precipitation', not 'precipitations'

Figure 1: speaking of domain D, it should be given in the plot where D exactly is. Is it the red box in (a) meaning the whole plot area of (b) and (c)?

Table 1: Why only this combinations of parameterization schemes? CAPE is only used for one simulation while B85 is used 5 times or PCMT 3 times, for example TKE + CAPE is missing and so on. Why don't you use equal numbers of every possible combination?

[7 160ff] First you say threshold T = 85mm, but then it is 100mm. So what is the correct threshold you have used? Is it the same threshold or something different? This needs to be clarified. Furthermore, you define a HPE with a single grid point reaches 100mm. You have interpolated to point observations to a regular grid. Is it possible that you miss events due to this interpolation meaning that an exceedance of 100mm at a single grid is too high? What about HPEs with rainfall amounts below the threshold for 24h but excessive rainfall over 48h or 72h?

[7 165] so 192 HPE days in 30% is 5%, I agree. The 99.5% percentile would be 18 days in 30 years. Can you please explain the difference?

Table 2: I do not understand the difference between HPEs (%) and Fraction of HPEs (%). Can you please specify?

[9 196] Cluster 5 contains 86% of the HPEs. In Table 2, it says Fraction of HPEs is 65.2%. Should this be the same?

[11 248] Equation (6): I think the 'x element of Obj_k' should not be below the fraction but behind?

[13 280ff] Are there some simulated HPE days among the false alarms?

[15 306ff] As already mentioned, differences could results from the parameterization schemes as convection could not be resolved by the model. Also initial conditions like soil moisture have a significant influence (for references see main comment above)

Figure 7: Differences in A-component may result from the parameterization which lead to an underestimation of rainfall mounts. Deviations in the S-component can origin in misrepresentation of the orography and other local dynamic effects

Table 4: The correlations are very weak and care has to be taken for the interpretation.

[16 325] 'table 4': Table always with capital 'T'

Table 5: In general, this table is hard to read and understand. Which bracket belongs to which cluster? For scheme combinations that where used several times (e.g. B85) is it a mean value of all simulations? There are a very few cases with statistically significant differing distributions. It is also a bit confusing that one part of the table belongs to the A-component and the other part to the S-component. Same for Table 6. Maybe it is better to split this.

[19 381ff] Where can I find this? You say in Table 5 + 6, but it is not given which bracket belongs to which cluster. And how do I have to interpret the numbers to get this statement.

[19 385ff] Where can I find the numbers to prove this?

[20 400] 'The departure from [...]', I think you mean 'The deviation from'

[20 402ff] Eq.(11)+(12) Are there other possibilities for the lower/upper boundary of the integral instead of -2 or +2? Where does this come from? Please specify.

[22 420ff] '[...] the S-component exhibits the highest error on the right side of the distribution for B85 [...]', according to the given tables, this is not true for cluster 2 and LT34

Figure 11: Differences for dashed lines not visible. I would recommend a logarithmic y-axis or a separation into two y-axis (left and right)

[25 446] too many brackets in a row

Figure 12: I wonder what is about objects that are larger than the investigation area?

[28 480ff] Following Fig. 13, there is an underestimation of the model compared to the observations for cluster 5 and a huge overestimation for cluster 2. Only for cluster 3 the distributions look similar over the total range. So the statement given here is imprecise.

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