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Comment on esd-2022-24

Anonymous Referee #1

Referee comment on "Vb-cyclones and associated North-Western Mediterranean Sea state in regional coupled climate simulations: evaluation and projection" by Praveen Kumar Pothapakula et al., Earth Syst. Dynam. Discuss.,
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Review of «Vb-cyclones and associated North-Western Mediterranean Sea state in regional coupled climate simulations: evaluation and projection»

by Praveen Kumar Pothapakula, Amelie Hoff, Anika Obermann-Hellhund, Timo Keber and Bodo Ahrens

submitted to: Earth System Dynamics (esd-2022-24)

The submission tries to assess Vb-cyclones in a centennial simulation downscaled using a regional climate model coupled to an ocean module to investigate precipitation related to Vb-cyclones over three different catchments. The vast amount of data provided by such a simulation provides certainly an interesting and important basis to obtain a better understanding of Vb-cyclones under global warming, and to deepen the understanding during such events. This is important because these events are often related to extreme precipitation and flooding events in central Europe. Nevertheless, I don't see how this study helps to foster or even increase the process understanding of Vb-cyclones with respect to the Mediterranean Sea:

First, the method is not well enough described, so that I find it hard to understand how variables (e.g., evaporation, wind speed and sea surface temperature) over the Mediterranean Sea are linked to the precipitation over the different catchments. How long are the Vb-cyclone events typically? What period is taken to accumulate precipitation? Is it daily and a four day lasting Vb-cyclone is counted as four events? How is it assured that the precipitation over the catchment is actually linked to the Vb-cyclone and not to another large-scale feature over northern Europe? The fact that half of the cyclones result in a negative anomaly with respect to the climatology (Figure 1) indicates that no real cyclone situation is present over the different catchments for these negative anomalies.

What is the argument for negative precipitation anomalies while a Vb-cyclone is present in the vicinity of the catchment?

Second, I find the conclusions drawn for the future too strong (particularly in the abstract), particularly in the light of obvious missing processes in the GCM, for which the RCM cannot compensate for (e.g., evaporation and SST values and patterns) in the Mediterranean Sea. The chosen climate simulation does not very well represent the SSTs over the Mediterranean Sea during the historical period and some other CORDEX simulations do a much better job. Why did the authors not consider including these simulations in the analysis as well? This would certainly add value as it allows to estimate the robustness of the results and would give an indication of the sensitivity of the results with respect to the Mediterranean SSTs. Another option could be to correct the SSTs in the boundary conditions of the RCM by a delta change approach. These missing processes at least need to be better discussed in the conclusions and in the abstract.

Third, the results for the future are shown in figures, but are often mentioned in only one sentence in the text. In a climate study, it is valid to evaluate the historical period against a reference and point out misrepresentations or biases, but then the focus should be on the differences between the historical and the future period. These differences allow to reveal changes in processes, which should then be discussed with respect to the bias of the model. This is widely missing in this study. In addition, the results obtained in the evaluation period are often compared to the results in Krug et al. (2021 and 2022) and it is pointed out that the results of the two studies matches (e.g., L340ff). This is not surprising as the underlying data, the method and the analysis are identical, therefore also the figures are very similar to the Krug et al. (2022) study.

Fourth, a lot of the method is based on other studies. I agree that these methods do not need to be explained in detail, but it would be very helpful for the reader if the method summarizes the main aspects, which is still lacking, particularly if the methods have been further adapted. I do not think that a reader wants to read several other studies to understand the underlying method of the study (e.g., cyclone tracking, track density, regressions, etc.).

Fifth, for me it is unclear how the trendlines in the Figure 1 and S1 are calculated. I would suggest using a non-parametric approach, such as the Mann-Kendall test for a monotonic trend. The trend line could even be removed and just the statistical number about the trend (slope, p-value) could be given. Since the authors have a seamless simulation that connects the historical and the future period it could also help to use the full period for the trend analysis, rather than just the two 55-year periods. One could also think to show this analysis in terms of histograms, where the evaluation, historical and future periods are overlayed into one panel. In my opinion it seems as if particularly the variability of the Vb-cyclones is changing in the future, at least in some seasons.

Sixth, I think it would be more meaningful to restrict the whole analysis to the 50-100 most intense Vb-cyclones only. This might help to obtain more meaningful results. This would further allow to cover the systems that impact the different catchment areas a bit

at least.

Seventh, the language of the paper is still not very fluent, and the grammar is not very good, rendering the manuscript difficult to understand.

Finally, I would like to point out that I have reviewed this manuscript before and most of the major comments have not been addressed or implemented into the new version. Reviewing publications is quite time consuming and I think it would be respectful to the reviewers to address or include major comments from previous submissions. To submit a less sloppy manuscript, a final critical check of the spelling, citation style (usage of brackets, L35ff meaningless citation of "vol") and the quality of the figures (aspect ratio of maps is not correct, axis labels overlay with labels of colour bar) before submission, is highly recommended.

Some further comments are (non exhaustive):

- Why did you downscale ERA-20C reanalysis rather than ERA5, as it covers the same period, but with a much better resolution and quality?
- Did you use the ensemble mean of ERA-20C? This is not clearly stated
- To measure the intensity of a cyclone the mean sea level pressure is not the best possible variable, it would make more sense to check the Laplacian of the pressure field within the closed isobar for example.
- In Krug et al. (2022) it is pointed out that the Mediterranean is not important for Vb-cyclones unless for the most precipitation intense ones and also several other studies indicate that the Mediterranean Sea is not the most important factor (as described in the introduction). I am not sure if I understand the motivation starting at L53 and hence of the whole study.
- L86ff: is the representation of Vb-cyclones in uncoupled simulations not realistic?
- Compared to other studies, this one is not convection resolving. How does this affect the results and what is the added value?
- Method section: Information about the EC-EARTH model is completely missing. Add an extra section.
- How is precipitation associated to Vb-cyclones? (e.g., L144, L173ff, L278, L399) Vb-cyclones are rather mesoscale features (much smaller than other cyclones) and hence their impact radius is not very large.
- Cyclone tracking at mean sea level pressure is often a problem for Vb-cyclones as they are in the vicinity of Alps and can often be detected earlier in a higher level using the 900 or 850 hPa level. How sensitive are the tracks with respect to the chosen pressure level?
- I am a bit confused with the selection of the probability distribution of the transfer entropy. You have tested a lot of different distributions and then decided to use a Gaussian distribution, which does not make sense in my opinion as neither daily precipitation sums, nor anomalies are Gaussian distributed.
- L255: it is still unclear to me how the generation of a Vb-cyclone is related to the Mediterranean SSTs.
- L272ff: This might also be due to an underestimation of the climatological mean sea level pressure field in the historical simulation. As mentioned before, the minimal

pressure of a cyclone is not the best estimate for its intensity as it strongly depends on its surrounding pressure field.

- Figure 3: I find the concept of the offset a bit weird. One could also use a Mann-Kendall test for the estimation of the trend.
- For Figure 4 to 11 there is always very little text and almost no information for the future. Are there significant changes between the historical and future periods?
- What is the causal relation between a high wind speed over the Mediterranean and the precipitation at the same time over the different catchments? Or is there a time lag in the analysis that I am missing? (L360ff)
- Figure 8: are there any significant changes between the historical and future period?
- Figures with patterns: The figures would look much nicer and cleaner if you would use only one colour bar for the nine panels as it is the same for all. In addition, the coordinates do not have units.
- Fig. S2: Units are missing
- Figs. S13 and S14: Aspect ratio is completely wrong.