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Response to the reviewer 1

We would like to thank the reviewers for their constructive feedbacks and insightful comments. We greatly appreciate the time and effort you dedicated to revise our manuscript, which helped us to improve our presentation. We have incorporated your suggestions in the revised manuscript, and you can find our responses (in blue) below.

Comments:

1) L27: could cite Allen and Ingram (2002) for the different drivers of mean and extreme precipitation.

2) L28 (rich gets richer): could cite Chou et al. (2009); Chou and Neelin (2004)

3) L30 "is constrained by the available atmospheric moisture": to be more precise, it is constrained by the maximum available moisture content at a given temperature ($q_v^\star$ is what Clausius-Clapeyron knows about, not q_v), but global extremes occur in regions close to saturation, so it does make much difference for global extremes. It could make a difference for regional extremes, in regions where the values of rain percentiles are rather constrained by the relative occurence of different precipitation regimes.

4) L72: you mention some model limitations later, but at this stage what is coming to mind is that you can add references for that fact that in CMIP5, the rich-gets-richer mechanism breaks in the torpics (Chadwick et al., 2013) and references for the drizzling bias (e.g. Stephens and Hu, 2010)

Responses to the comments 1 to 4: We will include the suggested citations and correct the line 30 as suggested.

5) L110: would there be any interest in having several ensemble members for the robustness of attribution of extremes to different modes of variability?

Thanks for the idea. Using a large ensemble would be always ideal to assess the robustness of modes of internal variability in climate simulations. However, there is a technical constraint on performing several ensemble members of these 3351 year-long simulations with daily time resolution. In case of CMIP5, very few (only two) simulations are available at daily time resolutions covering the past millennium.

Nevertheless, as our analysis considered all the possible conditions of the modes of variability linked with a large number of daily extreme precipitation events during the entire 3351 years, we assume that this amount of data can partially increase the robustness of the association of extreme precipitation with the modes of variability in CESM.

6) L120: Is there any uncertainty reported on the forcing data, that could propagate to uncertainties in extreme rainfall characteristics?

Forcing data have for sure some inherent biases coming from the reconstructions. However, based on the studies on the present-day and future extreme precipitation, extreme precipitation is more dependent on the surface temperature (Pendegrass et al., 2015; Sillmann et al., 2017) and internal variability (Sillmann et al., 2017), rather than the introduced external forcing, for instance, the emission scenarios and solar forcing. Hence, we assume that the uncertainty in extreme rainfall characteristics caused by the forcing data would be minimal.

7) L168 "each cluster is composed of consecutive days of extremes": which threshold is used to separate the clusters, how is it chosen, and is it spatially uniform?

The temporal threshold is set to the distance between the extremes within a cluster. In our study, the value of this threshold is one day. In other words, the maximum temporal distance allowed among the extremes in one cluster is one day, hence, the minimum distance among the clusters is two days. This temporal threshold of one day is a commonly used value in many GEV analysis (Coles et al, 2001; Sugahara et al., 2008), as it does not significantly reduce the number of data to be analyzed and at the same time, it guarantees a statistical independence among the de-clustered extremes. We do not necessarily expect that the clusters are uniformly distributed over the space and time, as except the temporal threshold of one day within a cluster (thus, two days among the clusters), there is no other spatial restriction imposed on the clusters.

We will reformulate the corresponding sentence in the revised manuscript to make it clearer.

8) L181: I am bit confused, it is not clear to me how the data is sampled for the calculation of a given non-stationary GPD. For stationary GPDs, I thought you were using the entire time series at each location and computed the shape, scale parameters for that whole distribution over time. For non-stationary distributions, it seems that you somehow fit σ0 and σ1 for the entire dataset, but how do you get the evolution of the tail distribution over time for each (x,y) point? Do you compute the rain distribution and the GPD for data sampled on moving windows, then obtain a σ(t) and regress it over time to get σ0 and σ1?

The GEV analysis does not consider the entire distributions, but it only works with the tails of the distributions. Thus, only the extremes, which are located at the tail of the distribution, are fit to σ(t) = σ0 + σ1*C(t). The fit to get the scale parameters σ(t) at each (x,y) and at time *t* is done through the maximum likelihood estimation.

The basis of maximum likelihood estimation for a non-stationary GPD is as follows (Coles et al., 2001):

The complete vector of GPD parameters β is β=[σ(t), ξ] as only the scale parameter σ varies with time. Under the assumption that the extremes z_1, …, z_m at *t*=1,..,m are independent variables, the log-likelihood function for the parameters σ and ξ when ξ ≠ 0 is:

$$l(\sigma(t),\xi) = -\sum_{t=1}^{k}\left\{\log(\sigma(t)) + \left(1+\frac{1}{\xi}\right) log\left[1+\frac{\xi z_i}{\sigma(t)}\right]\right\}$$

And when ξ = 0, the approximation that ξ → 0 is used and the log-likelihood function becomes:

$$l(\sigma(t),\xi) = -\sum_{t=1}^{k}\left\{\log(\sigma(t)) + z_i\sigma(t)^{-1}\right\}$$

Where σ(t) is σ0 + σ1*C(t) and k is the number of (de-clustered) extremes above threshold.

Then, the maximization of the pair of log-likelihood l(σ(t), ξ) with respect to the parameter vector β=[σ(t), ξ] is performed and this leads to the maximum likelihood estimate of the parameters σ(t) and ξ. For σ(t), β is [σ0, σ1, ξ] (Adlouni et al., 2007). This maximization is done numerically, as no analytical solution is possible.

In the revised manuscript, we will include this explanation on the estimation of the parameters (Note that the parameters for the stationary GPD are also estimated with the same method, but considering the constant σ.)

9) L244 (Mann-Kendall trend test): I don't know this test, would you have a reference? Is it standard practice?

10) L246 (Mann-Whitney U-test): I don't know this test, would you have a reference? Is it standard practice?

Response to the comments 10 and 11: In the revised manuscript, we will add some references to support the tests we used.

11) L284 "the de-clustering method is (typo) only around 40% of the initial numbers of extremes": same comment as above, does that depend on the threshold used to define clusters? Is it uniform?

Also refer to our response #8. The final number of de-clustered extremes depends on the sizes of clusters which are defined by the temporal threshold. If the temporal threshold among the extremes is softened (for example, instead of 1-day distance, taking 5-day distance), the size of each cluster would be large, and the number of de-clustered extremes would be reduced as the de-clustering method only takes a maximum extreme in each cluster.

12) L286: interesting comparison of the extremal index between tropical and extra-tropical regions. It seems there could be a correspondence with the convective-organization view- point, saying that organization is more likely to occur at high SSTs, and which would be consistent with clusters mainly occurring in the tropics.

Thanks very much for your comment. We agree with you that more consistent clustered convective organization would likely occur more at high SST and over the ocean where sufficient sources of moisture are available.

13) L350-351 "distinguishing the regions": do you mean that 'the POT analysis allows to separate distinct and coherent contiguous regions for similar types of distributions', or do you mean that the regions that are exhibited somehow map onto known regimes, ie the regions on Fig. 5b?

It means the former. We will reformulate the corresponding sentence clarifying the point in the revised manuscript.

14) Figure 2: What would the distributions look like with a logarithmic y-axis? Maybe that would allow to better illustrate when there is a finite upper-bound. Is it hard to see as it is shown now.

We will modify the figure 2 in the revised manuscript.

15) Figure 7, middle panel title: "mean above 99th threshold" would me more explicit.

We will modify the part of the caption as: "The thresholds for extreme precipitation defined as the 99th percentiles of daily precipitation relative to the 3351-year distributions."

16) Appendix A, L761-762: several variables are used and I assume they are normalized with mean and variance before analysis so that the RMSE can be compared numerically. But is there a reference RMSE value, a threshold above/below which the error is large/small, or is it just for relative comparison across modes?

Yes, the variables are normalized and standardized before the calculation. As you said, it is the relative comparison across the modes. Although, we did not mention in the manuscript, we excluded a EOF pattern from the model that theoretically should agree with the observed EOF, but present high RMSE relative to the observed pattern.

In addition, we selected the modes that are detected as well in the study by Fasullo et al. (2020) which used the CMIP models including the CESM family models, and by Lim (2014) which used the reanalysis. Both studies estimated the modes of variability based on the EOF analysis.

In the revised manuscript, we will mention these two literatures we have based on.

References

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