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Earth System Dynamics
Discussions

Interactive comment on "Long-term Variances of Heavy Precipitation across Central Europe using a Large Ensemble of Regional Climate Model Simulations" *by* Florian Ehmele et al.

Anonymous Referee #2

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This paper analyzes long-term trends of heavy precipitation in multiple dynamically downscaled simulations for the historical period and the near future over Europe. The different sets of simulations are validated against gridded observations and tested whether they can be combined to a large ensemble for the detection of trends in the historic time period. This paper is relevant in terms of assessing the possibility of combining various simulations from the same RCM with varying driving data. As well as, the detection of trends within the historic time period.

General comments:

1) Please be more clear about what you are showing in the figures. In most cases it

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wasn't clear to me if you are showing the ensemble mean or a metric with pooled data from the entire ensemble.

2) Several sections need more clarification on what was analyzed and for what spatial extend and aggregation.

Major comments:

. . . Model evaluation:

1) I have concerns with the comparison of E-OBS, CCLM and HYRAS over the sub-region AL. It is not clear to me whether the comparison was only performed for the HYRAS grid cells, which cover a substantially smaller area than E-OBS and CCLM, or whether E-OBS and CCLM represent the entire AL domain compared to a much smaller area in HYRAS. On P8-L204f you state this concern yourself '[. . .] which might be a reason for the vanished differences between E-OBS and HYRAS and the resulting specious deviations to the RCM'. Did you compare the three datasets for the HYRAS grid cells only? If not please do so.

2) Further, you state that '[. . .] by taking into account all grid points and all time steps within the investigation are (P6-L151)', does this mean that for both ME and AL you have included ocean grid cells in the spatial average of the RCM data? For both domains gridded observational datasets are only available over land. Please clarify this, and in any case ocean grid cells were included remove them from the comparison.

2) In P8-L212f you state that '[. . .] HYRAS was aggregated to the E-OBS/RCM grid [. . .]'. However, you first mentioned this here for the Q-Q plots, so can I assume that the IPC's in Figure 2 are also based on aggregated HYRAS data? Please clarify this and if the aggregation of HYRAS applies to all related analysis then please move this detail to the methods section.

3) Further, if I understand correctly the evaluation is based on the TP1b time period (1950-2017), however the HYRAS data is only available for the period 1951-2006.

Please comment on why the analysis wasn't based on the shorter HYRAS time period. I would recommend doing the analysis for 1951-2006.

4) The evaluation on such a highly aggregated level poses a risk of error compensation. It might be better to do the evaluation for each grid cell first (e.g. calculating the RMSE) and afterwards averaging the error metric.

5) Based on the concerns above, I don't really agree with your first point in the conclusion 'Extreme precipitation is well represented in the LAERTES-EU [P20-L352]'.

. . . Added-Value:

6) Regarding your conclusions on the IPCs showing '[. . .] a clear added value of RCM data compared to coarser global models'. From that one figure I don't really see the added value, since you haven't compared the driving GCM with RCM simulation. You have compared the IPC's to the 20CR reanalysis dataset. Because of the spatial averaging over such a large area, it might be that the trends in the GCM and RCM might be very close to each other.

. . . CC-scaling

7) Your conclusion on the trends following the CC-scaling in your conclusion [P21-L379ff] are flawed. If you make a statement like this, please perform the temperature scaling with the LAERTES-EU temperature data and not by relating the precipitation change to a temperature approximation from another study. Please see Kröner et al (2017) and Pfahl et al (2017) for other effects than thermodynamics. Kröner et al (2017), Climate Dynamics, https://doi.org/10.1007/s00382-016-3276-3 Pfahl et al (2017), Nature Climate Change, https://doi.org/10.1038/nclimate3287

Minor comments:

P2-L25f: see also Zhang et al (2017) for a discussion on CC scaling Zhang et al (2017), Nature Geosciences, DOI: 10.1038/NGEO2911

P2-L27-34: Please add some more recent literature on this topic, e.g. Fischer and Knutti (2016), Nature, DOI: 10.1038/NCLIMATE3110 Alexander (2016), Weather and Climate Extremes, http://dx.doi.org/10.1016/j.wace.2015.10.007 Barbero et al (2017), GRL, doi:10.1002/2016GL071917

P2-L45f: Please add a view more recent studies on trends in European floods. E.g.: Blöschl et al (2017), Science, DOI: 10.1126/science.aan2506; Blöschl et al (2019), Nature, https://doi.org/10.1038/s41586-019-1495-6

P2-L49f: Connection of Heavy Precipitation over central Europe and cyclones, see also Hoffstätter et al (2017), Int. Journal of Climatology, https://doi.org/10.1002/joc.5386

P2-L55f: Also see van der Wiel et al (2019) and Martel et al (2019) for the added value of large ensembles for flood risk or return periods of heavy precipitation van der Wiel et al (2019), GRL, https://doi.org/10.1029/2019GL081967 Martel et al (2019), Journal of Climate, https://doi.org/10.1175/JCLI-D-18-0764.1

P4-L114f: Please elaborate more on what you mean by '[. . .] more or less independent simulations'

P5-L139: What do you mean by un-initialized? Please clarify this for the reader, that by initialized you mean initialized by observational(-like) salinity and other variables, whereas the un-initialized data originate from a normal CMIP5 historical simulation. I had to go to Marotzke et al (2016) to understand what was meant by this.

P6-L157f: Are the 99th and 99.9th percentile based on all days or wet-days only? If you want to look at heavy precipitation it might be better to look at wet days only. Like this the values would not be affected by the dry-day adjustment as much. Further, it is not clear to me if you have first spatially aggregated and then calculated the percentiles, or the other way around. Please comment on whether you think that this will make a difference to your results. This could maybe also solve your concerns on P15-L282f '[. . .] an overestimation of precipitation [. . .], could be a result of missing data for the

applied dry-day correction.'

P7-L189: Could you briefly comment on why you chose the old 1961-1990 period as your reference climatology.

P9-L218f: Your conclusion to Table 2 stating that there is a higher correlation when driven by MPI-ESM-HR versus lower resolution MPI-ESM-LR is technically correct, however the differences are so marginal that I find it difficult to attribute the differences to resolution of driving data. Especially, when not only the resolution is different in the HR and LR simulations, but also the initialization. Maybe add a short sentence ['However, differences are only marginal.'].

Chapter 4.3: This is a nice analysis that shows the benefit of large ensembles, however since you are not looking at return values afterwards it could be nice to highlight another strength of large ensemble namely isolating the forced response from internal variability. Since you are looking at trends and variability this could be a better fit. But this is just a suggestion to improve the flow of the paper. Like a said it is a nice analysis as is.

Figure 8a: Shouldn't there be also some more positive anomalies in the climTP period? Did I miss something? Because if you base the annual anomalies on this period, shouldn't you be having positive and negative anomalies within this period?

Figure 9: Nice plots!

P22-L396f: '[. . .] can be used as input for hydrological modeling'. In general, yes and especially when looking at higher return levels of floods. However, as mentioned a few lines above this ensemble is restricted by temporal homogeneity, which can play a very important role in hydrology.

Technical Corrections:

Table 1: Projections for the period 2020-28 are missing

Figure 2, 3: Please add the years of the period ([. . .] TP1b (1950-2017)). I had to go back and look for the TP1b definition. But I would anyway suggest changing the period to 1951-2006 (see major comments).

P3-L69: Typo (‘Regionla’, ‘Regional’)

P5-L128: grammar (replace ‘it’ with ‘the’)

Figure 4: Please clarify what the RCM spread is. I assume it to be Min-May, right?

P20-L342: grammar (‘estimate’ instead of ‘estimated’)

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