Natural Hazards and Earth System Sciences Discussions

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC1
https://doi.org/10.5194/nhess-2022-283-RC1, 2023

Comment on nhess-2022-283

Anonymous Referee #1

Referee comment on "Future heat extremes and impacts in a convection-permitting climate ensemble over Germany" by Marie Hundhausen et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-283-RC1, 2023

The present manuscript analyses temperature extremes and associated impacts through a high-resolution convection-permitting (2.8 km), multi-GCM ensemble with COSMO-CLM regional simulations from 1971 to 2100 over Germany. The study points out a projected increase in temperature and its variance combined with hotter and more lasting heat waves. The analysis also considers a comprehensive set of heat stress and user-tailored climate indices.

The research is surely relevant in terms of temperature extremes analysis and also for considering for the first time a multi-GCM ensemble of convection-permitting (CP) climate simulations over a multi-decadal time period.

The topic is surely fitting with the scope of the journal but different relevant aspects mainly pertaining to the methodological choices and technical aspects related to the simulations performed and analyzed in the study deserve clarification before being reconsidered for publication in NHESS journal.

General comments

- My first concern regards the very basis of the numerical simulation strategy adopted. I refer to the three-step nesting dynamical downscaling. It is well-known (e.g., Rummukainen 2010), the importance of a buffer or sponge zone of several grid nodes width between two nesting boundaries. This relaxation zone has the fundamental role of bringing the model solution towards the lateral boundary condition (LBC) fields diffusing (smoothing) the differences between the model solution and LBC. The sponge

zone is characterized by a varying level of numerical instabilities. Coming to the simulations analyzed in the present study this buffer zone seems to be almost absent between the second (d02) and third (d03) nested domains. Subsequently, I expect a very noisy field driving d03 and I wonder if and to what extent this could negatively impact the proper development of d03 dynamics. Could the Authors provide any justifications for this quite atypical nesting strategy? And if any numerical detrimental effect has been detected or can be excluded.

- My second concern regards the bias correction. (i) from a technical point of view the quantile mapping (QM) configuration is not sufficiently described. I am especially referring to the correction of future time segments. As many studies point out empirical or parametric QM can affect the original climate change signal e.g., (Maraun 2016). So, it is a relevant choice to let QM free to alter the original simulated change signal, or conversely apply a trend-preserving QM configuration. Another relevant point is the extrapolation of the correction function over extreme values not present in the reference period but appearing in the future period. (ii) I do not get the meaning of bias adjusting convection-permitting (CP) scale simulations. This is not in general as we know they are still to some extent affected by processes misrepresentation but in the context of this study. Firstly, it is not clear when and how bias-adjusted simulations are considered in the analyses. I suggest making much clearer this point throughout the manuscript. Further, how we can disentangle the so-called added value of CP-scale simulations generated by an improved representation of physical processes and what is generated by the application of bias adjustment if raw and adjusted simulations analyses are not compared? This is especially when the same time segment is considered for deriving QM correction function and to evaluate it since the adjusted simulations and the observations will have by construction very similar statistical moments. Since CP-scale climate simulations are only very recently affordable and many aspects are still to be explored (like mechanisms and dynamics underlying hot extremes) I would rather focus on exploring the eventual added value and weaknesses of original CP-scale simulations compared to the (original) non-CP-scale simulations. This is just a suggestion I am not asking to rewrite a new manuscript. Also, for what concerns future changes, I would rather be interested in the influence of the CP-scale on eventual trend modification instead of using bias-adjusted CP-scale simulations since this latter could have modified original trends as well, especially considering extremes. Here, it is complicated to isolate the "real" effect of the high-resolution shuffling bias correction in this context. I believe that the manuscript already rises many relevant points even without including bias correction since represents another layer of uncertainty over statistics and climate indices that represent a rather high level of sophistication, even though not all the analyses involve bias-adjusted simulations (which increases confusion in the storytelling).

Specific comments

Line 4. "We find an improved mean temperature beyond the effect of a better representation of orography on the convection-permitting scale, with reduced bias, particularly during summer". I do not believe that the manuscript analyses allow us to reach such a conclusion.

The caption of Figure 1. To me it results quite complicated to understand, please rephrase. Especially: "Nesting in (a) and model domain"

Section 2.2 should be improved (see general comments.).

Line 152. “user-oriented parameterizations are tested”. Please explain what you mean in this statement.

Lines 172-174. To me is not cleat the meaning of “reference humidity is constant at 20hPa” is. Please clarify.

Line 187. Please correct the quote's typo.

Figure 3. Instead of monthly means, I would rather compare the five daily temperature distributions (e.g., boxplots) or a percentile-based error to see which part of the distribution benefits the most from the higher resolution during the different parts of the year.

Line 208. Please clarify how the Wilcoxon test is applied in this context.

Line 210. Why is talk about trends here? This sentence is not clear to me.

Figure 4 caption. (c) appears twice.

Line 239. “Average variance” Perhaps ensemble variance?

Line 252. Please clarify the meaning of “the full width of half maximum (FWHM)”.

Figure 6. Why change color bar limits and colormap between (a) and (c) panels?

Heat wave characterization results section is quite loosely described, I would suggest better discussing this part.

Figure 7. caption, the description of the panel (d) is not clear to me. Also, PDFs are not described.

Discussion and conclusion section. Here it should be clarified how the CP-scale and/or the bias correction contribute to the reported improvement of temperature extremes representation.