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Comment on nhess-2022-162

Anonymous Referee #2

Referee comment on "Droughts in Germany: performance of regional climate models in reproducing observed characteristics" by Dragan Petrovic et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-162-RC2, 2022

The manuscript describes an evaluation study of EURO-CORDEX hindcast simulations and additional WRF simulations with focus on the SPEI drought index. The methods are clearly described and mostly sound. In particular, the authors discuss the potential benefits of higher spatial resolution and regionally-tuned model setup, which may help to design efficient yet suitable model setups in future studies. The manuscript is suggested for publication after minor revision as described below.

Minor Revisions suggested:

Please provide your motivation for the model selection you applied (only 6 simulations). E.g, I'm pretty sure that EURO-11 simulations with several WRF configurations are available since many years. Why are they not used in this study?

In most practical applications, RCM simulations are only used after bias correction. Did you analyze the effect of bias correction (of temperature and precipitation) on your results? Please comment.

I would like to see a critical discussion on the practical relevance of the SPEI in climate change (i.e. trend) studies. Since NHESS has a focus on hazards and the hazard is rather the agricultural drought than the meteorological drought, the question arises to which degree the SPEI is able to describe drought hazard and what the of the SPEI to serve as proxy for agricultural droughts are. E.g., SPEI does not cover the effect of increasing surface runoff during heavy precipitation events, which, however, is a loss in the soil moisture budget. Similarly, SPEI does not regard increased transpiration due to longer vegetation periods in a warmer climate. I respect that this is not the main topic of the manuscript and therefore cannot be treated in a quantitative manner, but it is a very important boundary information and should be discussed in the conclusions and/or

introduction sections.

L295: “Therefore, it can be assumed that if existent, the benefits of a resolution increase from 5 to 12.5 km are less distinct,” A single simulation does not allow drawing such general conclusions. This statement refers also to other conclusions (e.g. line 321, 322).

L329: “Because of the results in the previous section, here we focus on the values from the WRF@5 km run in direct comparison to the reference values from OBS (Figure 3).“ In the previous section, you argue that the 15km WRF simulation outperforms the 5km simulation, at least for temporal correlation. How does this lead to the decision to use only the 5km simulation in the subsequent section?

More generally speaking: By (partly) removing the 15km WRF simulation from the analysis, you lose the option to directly compare it to the WRF 5km simulation and therefore you lose the most direct indicator for the effect of the spatial model resolution. On the other hand, major results of the study are statements like “computation resources could therefore be saved, since a coarser resolution can provide similar results” (Abstract) or “WRF’s increased resolution and setup is turned out to be beneficial in the analysis of the monthly values of the meteorological variables and the correlations of the SPEI time series”. Therefore, I suggest keeping both the 5km and 15km WRF simulations in each part of the analysis in order to better support your conclusions. (Side benefit: The additional Fig.5 could be avoided, if you had the 15km simulation in the general analysis (Figure 4).)