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Discussions

Interactive comment on "Local impact analysis of climate change on precipitation extremes: are high-resolution climate models needed for realistic simulations?" *by* H. Tabari et al.

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I accepted to review this paper because it is of interest to me. I have however had collaboration with one of the authors leading to numerous publications in the period 2012-2014. I have checked the HESS guidelines of conflict of interest and do not think there is a conflict. Nevertheless I have been in contact with the editor and will make my review public to avoid any misunderstandings.

Overall the paper is well written and shows that the authors have a comprehensive overview of the relevant literature. Further, the work is a logical extension of the work we have done together. The study fits well within the special issue to which it is submitted. It specifically addresses the gab in knowledge on to which spatio-temporal scale

dynamic downscaling should be performed. So I I think the work is novel and should be published. I have however some comments that I would like the authors to address that I think will lead to an improved paper.

It is probably too late to change now, but I am surprised that you use the A1B scenario for the climate projections. The simulations seem to have been made specifically for this study and hence I had expected an RCP-formulation of the projections.

The CCLM model formulation with three nestings seems very complicated. I have no experience with triple nesting and would like to see some references to work indicating that this is a feasible approach. Also a few words on the approach would be nice. Do you apply some sort of nudging or are you only providing boundary conditions? I would have preferred to go directly from the 25*25 km to the highest resolution and then let computational cost define the area. In any case it is unclear whether the 7*7 km is non-hydrostatic or not. So I would appreciate more information on this crutial step.

On page 5 and 6 you have a quite detailed interpretation of how the various statistics perform. It would be nice to have a metric of uncertainty to distinguish between sampling errors and actual signals from the simulations. The most simple would be to include the at-site confidence intervals for the point observations. Another solution could be to consider the variation in the extreme statistics caused by observed decadal oscillations.

It is not clear to me exactly how the datapoints in the figures are derived. In Figures 1 and 3 it seems that the points are the raw model output statistics (MOS) plotted using a plotting position formula (which one?). This ignores the dependency of extremes on spatial scale often described by an Areal Reduction Factor (ARF). Plotting these values on the same graph implies that the numbers can be compared directly, which is not the case. Please clarify what you do and consider to modify the graphs by incorporating ARFs. In Figure 1 I miss an explanation on how you can show 30 year return periods when using 9 years of observations for the CCLM model (it looks like raw MOS?)

On Figure 2 it would be very interesting to see how the aggregation of the results from the Alero/CCLM models perform. I would suggest to aggregate the model results in the same way as the observations have been manipulated. It would give an indication of the accuracy of the spatial structure in the model.

When doing comparisons as shown in Figures 5 and 6 the impact of using raw MOS is very clear, leading to very abrubt changes and high noise. Smoothening by fitting a POT distribution to the MOS would lead to results that are easier to interpret. Perhaps showing both types of results in the same figure would lead to an interesting discussion about signal to noise and variation. In any case I find the variation shown in Figure 5 substantial and is less certain about the added benefit of applying non-hydrostatic than your text indicates.

Below are a few detailed comments:

P2, L5. I think you mean 'spatial resolutions up to' rather than 'spatial resolutions down to'

P2, L12: Mayer et al (2015) missing from reference list.

P4, L2: It is difficult to do decadal statistics with only 9 years of simulations.

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