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Hydrology and Earth System Sciences Discussions

Interactive comment on "Future changes in extreme precipitation in the Rhine basin based on global and regional climate model simulations" *by* S. C. van Pelt et al.

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Response Reviewer #2 General Comments Review of van Pelt et al "Future changes in extreme precipitation in the Rhine basin based on global and regional climate model simulations" This paper discusses future changes to precipitation as modelled by GCMs and RCMs over the Rhein basin. It applies a novel approach of bias correction of precipitation to assess future changes in return periods. The approach is tailored for hydrological applications, albeit this is not done within this study. The paper addresses an important area of climate impact studies and as I understand it develops an existing

method of bias correction. However, I struggled with the method description, which was unnecessary long and complicated. Same goes for the experimental setup which made the evaluation of the results difficult.

We would like to thank the reviewer for the valuable comments about our study. The reviewer suggests that the method description was unnecessarily long and complicated and the reviewer struggled with it. The authors, however, feel that part of the importance of this paper is the extensive description of the advanced delta method. When parts of the description are left out, it will be difficult for others to replicate and use this method. Therefore, the authors prefer to leave the description as it is. We did, however, try to improve the readability by adding blank lines, thereby separating more clearly the paragraphs. Also a small caption/title (Transformation for large P) is added to section 3.1.1. The authors also feel that the reviewer confuses our transformation of observed data with bias correction methods that have been applied to climate model simulations. We think that the confusion is due to the sentence "This type of transformation was first applied for precipitation bias correction in the Meuse basin by Leander and Buishand(2007)" on P6539, L11-13. However, Leander and Buishand applied the transformation to climate model data whereas in the present manuscript it has been applied to an observational dataset. The first paragraph of this section has been changed to: "In this study, a more advanced delta method was introduced, that not only takes changes in the mean into account but also the changes in the extremes. Again these changes can vary seasonally and spatially. Rather than a proportional adjustment of observed precipitation, the following non-linear transformation was applied to the bulk of the data (see also Fig. 1 for a graphical summary of the complete procedure): (1) where P and P* represent the observed and (transformed) future precipitation, respectively, and a and b are the transformation coefficients (a, b >0). Shabalova et al. (2003) showed that this relation between P* and P arises if the parameters of a fitted Weibull distribution are perturbed. Leander and Buishand (2007) used this type of transformation to correct for bias in RCM simulations for the Meuse basin, i.e., Eq. (1) was applied to RCM output rather than observed precipitation as in the present study. In addition,

Eq. (1) was modified for large P and the transformation coefficients were smoothed (see below)."

Scientific content

The description of the method needs to be written in a more clear and concise manner. As it is now it is much too long, and quite trivial equations are excessively explained. I would suggest to try and cut it to half its current size. Even though the section is long it is not clear to me how the methodology was implemented to the climate simulations. Also, figure 1 does not add any clarity to the steps taken and should be revised to show the steps rather than resulting precipitation fields (which can be interesting to show in a figure by itself. I would also urge the authors to clarify the following points: 1.This method builds on previous work, and what is novel in this particular application? It was not clear to me which part of the methodology that was new developments. 2. Why was only 5 RCMs from the ENSEMBLES project used? There exists a much larger sample of RCMs, and I would suggest adding these to the paper, especially since the authors state that this would be useful. 3. The section on temperature correction should be deleted since it is not discussed further.

The reviewer suggests that Figure 1 should be revised, however, the authors feel that part of the problems with readability of the figure are due to the small size in which the figure was printed. The steps are indicated in the figure by the numbers 1 to 4. In response to the first question of the reviewer: this method indeed builds on previous work. The advanced delta method, however, has been developed further and the sensitivity has been tested further. This work results in increased usability of the method and deeper understanding of the assumptions and limitations. Furthermore, this study shows results for a large GCM ensemble and an RCM ensemble. For the RCM ensemble the changes resulting from our delta method are compared with those from the bias-corrected model output. The changes from the RCM and GCM ensemble are also compared. Such comparisons have not been made before for extreme precipitation in the Rhine basin. This study gives new insights in the changes in extreme precipitation

and will together with the follow-up papers on hydrological impacts and natural climate variability add to the knowledge of the uncertainty of extreme events. In response to the second question about the RCMs from the ENSEMBLES project, the reviewer rightly points out that the ENSEMBLES project holds a larger sample of RCMs. The five RCM simulations in this study were selected from 12 ENSEMBLES RCM simulations that were available at the beginning of the Rheinblick2050 project. An important point in the selection was that the simulations covered the range of changes in extreme precipitation characteristics well (for more details see the Rheinblick2050 report, reference Görgen 2012). In a companion study (see reference Ward et al., 2012) the hydrological results were analysed and compared with those already available through the Rheinblick2050 project. It would be interesting to use a larger sample of RCMs, including simulations from outside the ENSEMBLES project, to study other issues, like the influence of the GCM forcing on the RCM output. However this could easily fill up another paper, the main value of this paper was showing the use of the delta method on a large GCM sample. The third point raised by the reviewer is that the section on temperature could be deleted since it is not discussed further. The authors disagree with this, because results for temperature are discussed in Table 3 and section 4.1, second paragraph

Structure and presentation

As mentioned before, the method description needs to shortened and clarified. This goes for the paper in general as well, where things are often repeated. The language is a bit too casual, and I would recommend to remove all “we” from the paper and rewriting it accordingly. There is also not necessary to describe in words what a table or figure is showing, that should be contained within the figure caption. It is also a mix of tenses, and I would suggest to stick to past tense when describing what has been done. The figures needs some improving, for example by removing the headers on figures and adding letters to describe them. That makes references in the text easier to follow as well.

The authors thank the reviewer for the comments on readability. The text is revised carefully on tense and adjustments have been made especially to the text that refers to tables or captions.

Minor comments

1. P 6534, L24. You mention RCMs here, but it should be GCMs, or GCMs-RCMs

This has been changed.

2. You mention the Rheinblick2050 project, but for a wider audience this is not known. Since you mention the ENSEMBLES project later, I would suggest to use this as reference for the RCMs

The authors would like to keep the reference to the Rheinblick2050 project, as we feel that it will be a valuable addition for the readers who are familiar with this project. Besides this, not all RCMs currently available from the ENSEMBLES project were available at the time of the RheinBlick2050 project.

3. P6535 L19. “better picture” is an example of a too casual language which is too fuzzy. What do you mean with a better picture?

This sentence has been changed.

4. P6537, L11. What is a “hydrologic winter”?

The hydrological winter is part of a hydrological year. These terms have been introduced to get a better correspondence between precipitation and runoff. The hydrological winter in Belz et al. (2007) refers to November-April (added to the text).

5. P6538, L15. Please provide a reference to the HBV model.

The reference has been added.

6. P6538, L16. Delete the sentence beginning with “We have. . .” since you mention in the following sentence which RCM was excluded and why

We prefer to keep the sentence for clarity, but have decreased its length.

7. P6538, L6. What is meant by “commonly available scenario”? The GCMs are projections of that emission scenario.

“Commonly available” has been removed.

8. P6538, L8. HBV is short for “HydrologiskaByråns Vattenbalansavdelning”

The correction has been applied.

9. P6539, L9. Why mention this dataset if you did not use it? And why did you not use it?

We wanted to show that this set is available and we are aware of it, but we could not use it because we used the same datasets as in the Rheinblick2050 project. A comment on this has been added.

10. The whole section 3.1.1 needs substantial rewriting and shortening, so I will not comment on it in detail

We have commented on this in our response to the general comments.

11. The whole section 3.1.2 is misplaced in this section, since it mixes method with results. I would suggest that you explain the smoothing, and then discuss and motivate it in the results and discussion.

Besides that the authors feel that this is a matter of taste this section has been given a new title: ”Exploring the sensitivity of choices” and is restructured accordingly.

12. Figure 3 is not clear to me. Is the comparison done between applying the smoothing filter or not? Or is it comparing the raw GCM compared with bias-corrected?

Figure 3 (Figure 4 in the revised version) shows the effect of the bias correction of the quantiles P60 and P90 in the delta method on the relative changes of the mean 10-day maximum precipitation in the transformed observations. To make it more explicit

that this refers to the relative changes along the horizontal axis, we will change the word “results” in the last sentence of the figure caption into “relative changes from the transformed observations”. The relative changes along the vertical axis refer to the raw GCM output. We will indicate this clearly in the revised figure caption and in the main text (paragraph above Figure 3). The need for smoothing is demonstrated in Figure 2. All other results are based on smoothed transformation coefficients as indicated in section 3.1.1.

13. The first part of section 4.1 is a method description and should be moved to that section.

See response to comment 11.

14. The first sentences of section 4.2 and 4.3 are both examples of sentences that can be deleted, since they are just repeating what is in the figures.

The repeating parts are removed from throughout the text (e.g. also in section 4.1.)

15. The results presented in figure 4 are not clear to me. Why compare with observed return periods and not with those over the control period?

The results in Figure 4 (Figure 5 in the revised version) show the comparison between the observations and the “transformed observations”. The transformed observations are created using the delta method. They are representative of future conditions. In this figure we show the change between the observations and the transformed observations. An alternative could have been to show the Gumbel plots of the 10-day basin-average precipitation for the control run and the future run of the climate models. However, this requires bias correction of the climate model output and separate 3000-year simulations both for the control and future period for each climate model, which is not attractive.

16. Results in section 4.3 are confusing. You mention GCM and RCM ensembles created with the delta method and the bias-corrected RCMs. Perhaps I misunderstood

the method, but the difference between the two datasets are not clear to me.

In the delta-method an observational dataset is modified to obtain a representative time series of future climate conditions. In the case of bias corrected RCM output, the output from the RCM is modified. See our response to the General Comments. In our study, the delta method was used to modify the observed data according to the changes in both GCM and RCM data. Bias corrected RCM output was also available from the Rheinblick 2050 project. As a validation for the advanced delta method, we compared the changes from the bias corrected RCM output with those from the delta method in section 4.3 (and, by the way, also already in section 4.1). To be more specific on the bias corrected RCM output that we have used, a change has been made from “bias corrected RCM output” to “bias corrected RCM output from the Rheinblick2050 project” (P6546 L12, P6548 L5&L11, caption Fig.5).

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