Hydrol. Earth Syst. Sci. Discuss., 9, C4797–C4810, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C4797/2012/

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.

S. C. van Pelt et al.

saskia.vanpelt@wur.nl

Received and published: 15 October 2012

Response Reviewer #1 General Comments The authors address a question of high importance for flood risk management in the Rhine basin. They face two challenging problems: (1) estimating rare heavy multi-day precipitation events (i.e. return periods up to 1000 years) based on a comparative short observational precipitation record and (2) projecting these into a far future (2081-2100). The paper under review focuses on the second problem. An advanced (non-linear) delta change approach is developed, defining transformations between observed and projected precipitation data from an extended ensemble of climate change scenarios (compared to earlier studies). These transformations are subsequently applied to a time series of 3000 years of precipitation data representative for today's precipitation regime. The latter has been derived by resampling from the (short) observational record using a method described in the literature (and which is only briefly summarised in the present paper). The statistical analysis of the transformed time series yields bandwidth' of 10 day precipitation sums over the Rhine basin for different return periods up to 1000 years.

The current study is a valuable contribution towards an advanced understanding of possibilities and limitations of predicting future precipitation regimes. As it is rightly pointed out in the discussions and conclusions the delta method as applied here has its limitations and relies on a number of assumptions (in fact: as all methods trying to look into the future)The single most important information lacking to me is an estimate of the impact of all these assumptions on the bandwidth determined and thus on its reliability. How sensitive is the bandwidth to various assumptions? The authors rightly cite Klemes (2000a, b) in the context of difficulties in the extrapolation of distributions fitted to observed flood peaks (p. 6536, l. 9). However, neither uncertainties in determining the parameters of the weather generator used nor in scaling the excess above P_90 are examined, though both in a sense correspond to extrapolating distributions towards extreme events. When exploring these (together with in addition the impact of "some subjective choices" regarding temporal and spatial smoothing) my basic (nil) hypotheses were, that the uncertainty in determining a climate signal on extreme precipitation events is much higher than the conclusions suggest (see specific comments below).

Overall, I had some difficulties to always keep oriented about temporal and spatial scales and intervals (1, 5, 10 days, overlapping, non-overlapping means, 20, 35 years) used and compared including rational behind choices, though Fig. 3 helped a bit. I suggest to add an overview (e.g. by extension of Fig. 3 or a separate table) and rational at one position (e.g. in an introductory paragraph).

We would like to thank the reviewer for the valuable comments on our study. As the reviewer rightly points out, projections of future climate are subject to large uncertainties. The main climate change uncertainties can be attributed to model uncertainties, internal climate variability and scenarios uncertainty. The methods used to obtain suitable time series for impact modelling, however, also contribute to the uncertainty, as they rely on a number of assumptions and often require the estimation of unknown parameters. In this paper the delta method has been extensively described and applied to study extreme precipitation. The reviewer feels that there is a lack of an estimate of the sensitivity to the choices made in this method and its impact on the bandwidth. Although some sensitivities have been described in the paper, the authors added some extra results explaining the sensitivity of the delta method and the impact of the assumptions on the bandwidth. For details, see authors' response to specific comments. The reviewer also misses an estimate of the uncertainty of the return levels derived from a long simulated time series. However, the aim of the paper is to explore the range of future change in multi-day precipitation from a small ensemble of RCM simulations and a larger ensemble of GCM simulations (see the end of the introduction on p. 6536). It is not the intention to estimate the uncertainty of the 1000-year precipitation from resampled data or the total uncertainty of the 1000-year precipitation (climate change uncertainty plus resampling uncertainty). This would require additional simulations and preferably also longer simulations (see response to specific comment 1). The simulations in this study were made available from other projects. Notes on this are added in section 3.2. Another comment of the reviewer was about the rationale behind some choices. The authors have clarified this in answer to the reviewers' additional comments.

Specific Comments (1) Generation of 3000 years of "observation-like" data is based on 1961-1995? How sensitive is the generation method to choosing e.g. 1966-1995 or 1961-1990 or leaving out the most extreme event from the "seed"-data set? How does the bandwidth from this sensitivity compare to the bandwidth of the ensemble of climate change projections?

Especially the upper end of the solid line in the right (lower) panel of Figure 4 (Figure 5 in the revised version) is sensitive to the choice of the historical data used for resampling. The 1000-year event is also sensitive to the random number seed used for the 3000-year simulation. The latter sensitivity can be reduced by simulating longer time series than 3000 years. The sensitivity to the historical time series used for resampling (sub series with relatively high or low winter rainfall, sub series with and without the most extreme winter precipitation event) has been studied for the Meuse basin, which is adjacent to the Rhine basin, by generating sequences of 20,000 years (http://www.knmi.nl/bibliotheek/-knmipubmetnummer/knmipub196_IV.pdf). Similar work is planned for the Rhine basin, but this is too late for the present paper.

(2) Parameters of the advanced delta change approach are based on statistical analysis of periods 1961-1995 and 2081-2100 (i.e. 35 and 20 years periods)? Statistics on e.g. P_60 and P_90 or the excess thus are based on time series of different length for past and future. This is not nice (imagine to base the analysis on an even shorter future period, in extreme on just one single year, e.g. a very extreme or very moderate one. Periods should be as long as possible and of equal length. At least it should be explored how sensitive the parameter estimation method is to the choice of periods and again, how does the bandwidth from this sensitivity compare to the bandwidth of the ensemble of climate change projections?

The authors agree that it is not nice using time periods of different length. The main problem with unequal sample sizes is that this may lead to an additional bias in the estimated change of the mean excesses. This is because the mean excess is biased owing to the bias in the 90% sample quantile, which depends on sample size. The benefit of equal sample sizes is then that the biases in the mean excess are the same for the control and future climate. Because of data availability it was, however, not possible to get long time periods for both future and control periods. For the future runs the period 2081-2100 was the only common period for which daily data was available for all GCMs. Although, we were aware of the problems with unequal sample sizes, we have chosen to take a longer control period, to reduce the influence of sampling variability on the estimated parameters in the delta method. The effect of the different lengths was analysed by considering the changes with respect to the 20-year control periods 1961-1980 and 1976-1995. The average of these changes did not differ much from the changes with respect to the 35-year control period 1961-1995. A comment about this has been inserted in section 2.2.

A number of additional remarks in consecutive order: p.6535 l.20: “is believed”: please choose another expression, science is not about believes but about hypotheses and their confirmation or rejection. E.g. use “it has been shown” or “several studies prove that under conditions” or something else.

Thank you for this suggestion, we chose another expression

p.6536 l.25: according to http://www.chr-khr.org/de/node/432 the length of the Rhine is only 1.238,8 km (older literature states 1230 km, only recently 1320 km has been used instead)

We think different lengths of the Rhine river are a result of a mistake that has been made in the past, the length was written as 1320 km instead of 1230 km and this has been copied by many authors. We changed the length to 1.238,8 km as stated by the CHR reference.

p.6537 l.23: “with a high resolution precipitation and temperature data set”: Which set? Cite. Which period? Which bias correction method?

We added a reference to section 2.2 about the precipitation and temperature data set and the bias correction method used.

p.6538 l.10: “but this was not used in this study..” “..could not be used”?

This is changed in section 2.3 and the reason why this data set could not be used is given.

p.6539 l.19: “non-overlapping”: may miss more extreme 5-day sums. Sensitivity, if start of non-overlapping 5-day periods is shifted 1,2,3,4 days?

We did a sensitivity analysis of the selection of 5-day sums. We compared the original non-overlapping 5-day periods with those after a shift of 1,2,3, and 4 days. The results showed differences for some models, which can be expected as the shifting of 5-day periods can be seen as sampling of natural variability, but the ensemble range of the extreme events only showed a minor change. A figure is added to the manuscript (section 3.2.1) and added as supplement to this reply (Figure 1- the caption is also added here, as the complete caption could not be added to the supplement).

Figure 1 CAPTION: Relative changes of the 10-year return level of the 10-day basin-average precipitation in the winter half-year (Oct – Mar) for each GCM. This figure shows the effect of shifting the 5-day period. Mean indicates the mean of the relative changes of the 5 different shifts for each GCM simulation. The asterisk indicates the 5-day period considered in this study.

l.22ff: A definition sketch of both (present and future) probability functions indicating shifts of P_60, P_90 and Excess would be helpful

We think that panel 3 in Figure 1 (of the manuscript) explains the shift in P60, P90 and the Excess. The figure was printed a bit too small, we think an increase of the size of Figure 1 will improve the readability a lot and explain the shift.

p.6541 l.13: and p.6542 l.7 “unrealistic high precipitation”: “unrealistic” in regard to what measure? Probable maximum precipitation (PMP)? The problem with events featuring return periods of 1000 and more years is that they always are very close to “unrealistic” as they hardly ever occur! It is not even clear, whether they belong to the same statistical basis, as their generation mechanism may be completely different. How sensible is the result of this paper to the exact scaling of the excess? This relates to my earlier (general) comments. p.6541 l.14/15 “adequately”: again, in view of my previous comment, what is adequate?

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

In the most extreme scenario, the 50-year event in the left panel of Figure 4 (of the manuscript, Figure 5 in the revised version) is about 1.5 times the observed largest 10-day precipitation amount. The 1000-year event in the right panel of Figure 4 is almost twice the observed maximum in the most extreme scenario. However, in the absence of smoothing, we already get relative changes as large as 3 at the 50-year event (Figure 2). This means that an observed precipitation maximum of 122 mm would become a maximum of 366 mm. We cannot exclude that this is possible, but we found it unrealistic as we cannot give a physically plausible explanation for such a large number. The risk of getting unrealistically large changes increases if we do not use the modification given by Eq. (11). This modification also ensures that the changes in the mean excesses of the exceedances of the 90% quantile P90 are reproduced, which may not be the case if Eq. (1) is applied to the daily precipitation amounts exceeding P90 too. The modified equation therefore generally reproduces the changes in extremes better (or "more adequately" in our words). A comment on this is added to the manuscript (section 3.1.2). Note that section 3.1.2 has been given a new title: "Exploring the sensitivity of choices" and is restructured accordingly.

The changes in the return levels in Figure 4 (Figure 5 in the revised version) strongly depend on the scaling of the mean excesses. This scaling is quite uncertain. There is for instance a pretty large difference between the changes in the mean excesses from the two ECHAM5 simulations in Table 2 (ECHAMr1 and ECHAMr3, respectively). Apart from the scaling of the mean excess, a change in the shape of the right tail of the distribution will strongly influence the extremes in the future climate, which is not taken into account in the delta method used (p.6550, l.9-11). The inability of finding significant changes in the shape of the upper tail in relatively short climate model simulations (p.6553, l.10-11) leads in fact to a large uncertainty. This argument has been added to the Discussion and conclusion section 5.

p.6543 l.25: "unrealistically": In view of my previous and general comments: Why? Why subjective smoothing? Why not even more smoothing? It's a rather arbitrary choice and its bandwidth needs to be explored.

Apart from the fact that a change factor as large as 3 leads to very large precipitation amounts in the future climate, it is not realistic in our view to multiply the precipitation amount at one grid box by a factor of 3 and the value at the surrounding grid boxes by a factor ranging from 1 to 1.5. Spatial smoothing is therefore necessary. We have added a figure to the manuscript (section 3.2.1), explaining the effect of temporal smoothing on the range of the changes in the 10-year event. This figure is also added as supplement to this, together with an additional figure on spatial smoothing. The latter was not added to the manuscript as the information of this figure could easily be explained in the text.

Figure 2 CAPTION: Relative changes of the 10-year return level of the 10-day basin-average precipitation in the winter half-year (Oct – Mar) for each GCM. The figure shows the effect of different choices for temporal smoothing: two 5-month moving averages with weights 1/16, 1/8, 3/8, 1/8, 1/16 (smooth 1) and 1/8,1/4,1/4,1/4,1/8 (smooth 2), two 3-month moving averages with weights 1/4,1/2,1/4 (smooth 3) and 1/8, 3/4, 1/8 (smooth 4) and no temporal smoothing (smooth 5). The asterisk indicates the type of smoothing used in this study.

Figure 3 CAPTION: Relative changes of the 10-year return level of the 10-day basin-average precipitation in the winter half-year (Oct – Mar) for each GCM. The figure shows the effect of different choices for spatial smoothing: smoothing with weights $\frac{1}{2}$ for the grid cell of interest and 1/(2n) for the n adjacent grid cells (spatial 1), smoothing with weights only horizontally (spatial 2), smoothing by taking the median over all grid cells (median). The asterisk indicates the type of smoothing used in this study.

p.6546 l.1-26: This paragraph very briefly describes the nearest-neighbour resampling used in the present paper which has been developed, applied and described elsewhere in the literature. Unfortunately there are no comments or estimates about the sensibility of the parameters of this resampling method to the results of the current paper.

Some few suggestions to enhance readability: l.2: add “nearest neighbour” before “re-sampling” already here. l.5: New paragraph after 3.1.3. l.6: After “Rhine basin“ add “applied in this study”

The reviewers suggestions on readability are applied. The sensitivity of the reproduction of the autocorrelation and the extremes to the parameters in the resampling procedure has been explored by Buishand and Brandsma (2001). To reproduce these quantities well, it is important to consider only a small number of summary statistics in the search for nearest neighbours and to resample from a relatively small number (5 or 10) of nearest neighbours. We will add the following two sentences to p.6545, l.20: “In each simulation step, the 10 nearest neighbours of the last generated day in terms of these summary statistics are searched for in the historical data. Details about the sensitivity of the autocorrelation and the simulated extremes to the summary statistics used and parameters in the resampling procedure can be found in Buishand and Brandsma (2001)”. The sensitivity to the baseline time series used for resampling was not considered by Buishand and Brandsma (2001). This requires a separate study (see response to specific comment 1).

p.6550 l.13: “Most..was tested carefully.” does not sound like Science. In fact, as explained earlier, I miss the evaluation of the impact of all choices (at least an estimate, how other reasonable choices could influence the bandwidth of the results). l.14: I missed estimates of “sampling uncertainty”, as mentioned before e.g. by sampling form shifted 30 year and 20 year time series (from the 35 year record).

We reformulated the sentence. Evaluations of the impacts of shifting 5-day periods and smoothing on the bandwidth have been discussed above and are added accordingly. We also added a sentence at the end of Appendix B that a larger number of order statistics in the Weissman approach has no influence on the bandwidth of the estimated 1000-year return levels.

p.6562 l.3 in caption of Table 2: “changes”: add “between climate of 1961-1995 and

2081-2100” or similar

The reviewers suggestion are added.

Technical corrections p.6537 l.15: “a” hydrological model (instead of “the”)

Correction has been applied.

p.6540 l.13ff: the superscripts “C”, “F” and “O” are not explained here (initial use), but two of them at p.6541, l.2, there misleadingly using the word “again”. Also in Appendix A these superscripts are used from p.6553, l.4ff without explanation.

An explanation has been added. The quantities and in Appendix A were already defined in Eq. (10).

p.6541 l.8-11: This note could be moved into a footnote (or separated by a blank line)

The note has been separated by a blank line.

l.12 A small caption “Excess > 90%” would improve readability and overview (or separation by a blank line)

A small caption/title (Transformation for large P) is added to section 3.1.1.

p.6542 l.14: “smoothed”: add “over time” This is changed.

p.6543 l.2:“: : :of the sub-basins: : :”: change to “: : :of all sub-basins”.

This is changed.

p.6565 l.5 in caption of Fig. 2: “Note the difference: : :”: No comma after “Note”

Correction is applied.

p.6567 l.2, 3 and 7 in caption of Fig. 4: change “upper” to “left” and “lower” to “right” panel readability of Fig. 4: - Indicate extension of left panel in right panel - Add tics and labels for 50, 500 and 5000 in right panel - Add tics and labels for lower and upper bounds of plot

The changes to “t” and “right” have been made. The tics for 50 and 5000 have been added, but not for 500 , because adding an extra tick for 500 would decrease readability, we prefer 200, as this is a return period used for the safety levels for parts of the river Rhine in Germany.

p.6568 readability of Fig. 5: - Add tics and labels for lower and upper bound of plot

Tics and labels are added to the figure.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 6533, 2012.

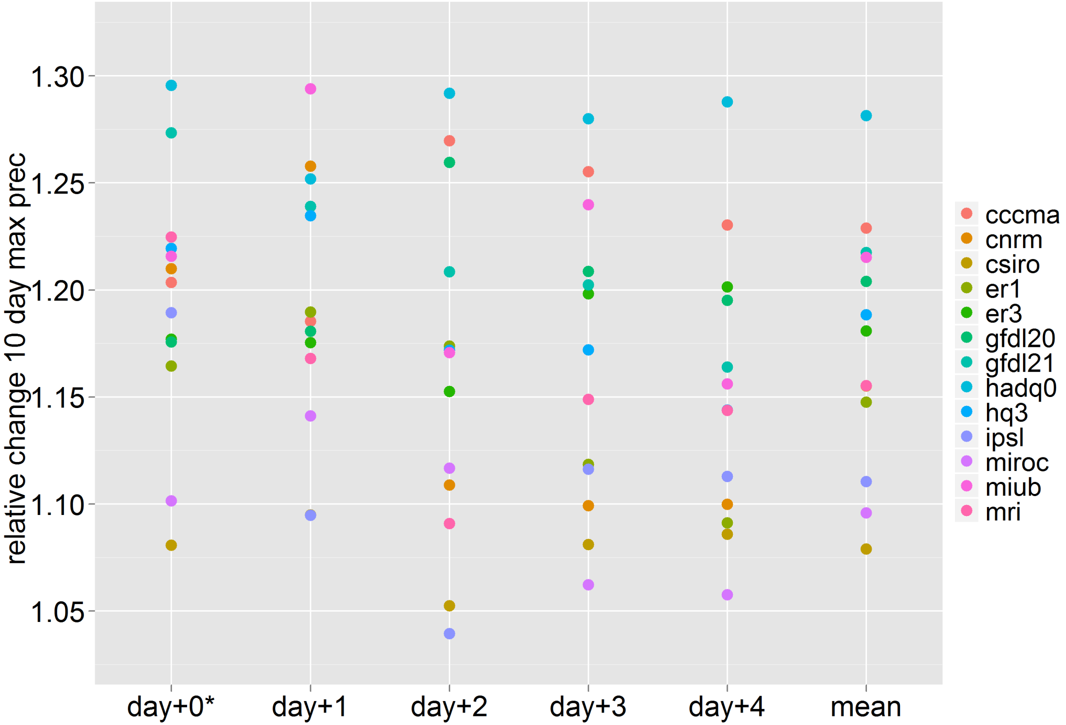

Fig. 1. Relative changes of the 10-year return level of the 10-day basin-average precipitation in the winter half-year (Oct – Mar) for each GCM. This figure shows the effect of shifting the 5-day period.

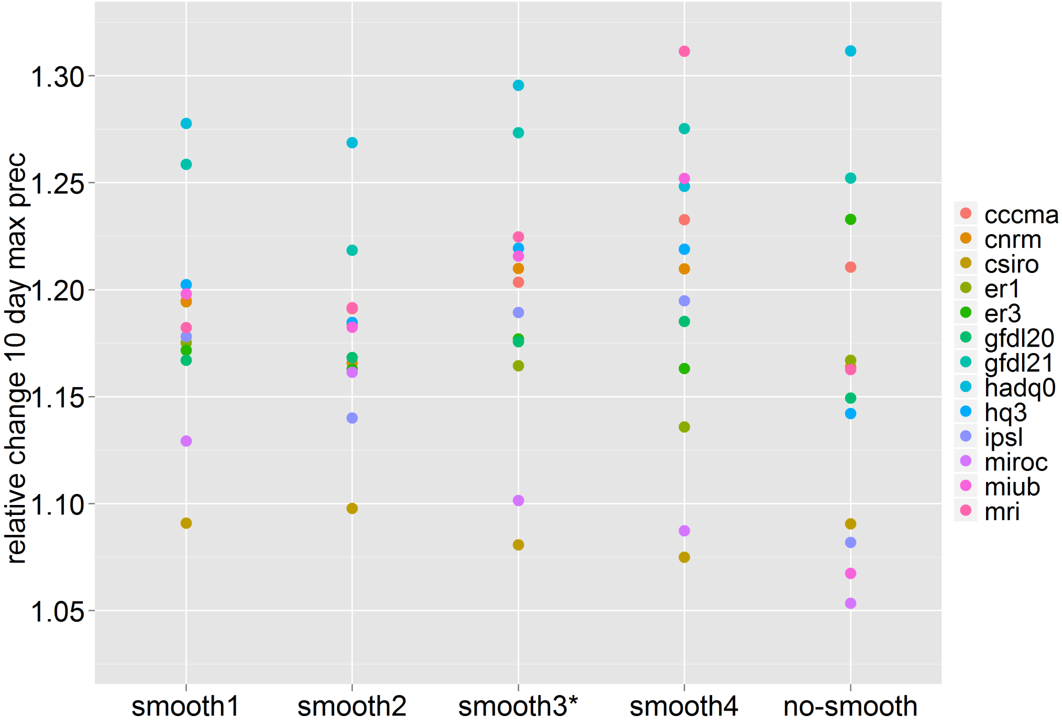

Fig. 2. Relative changes of the 10-year return level of the 10-day basin-average precipitation in the winter half-year (Oct – Mar) for each GCM. The figure shows the effect of temporal smoothing.

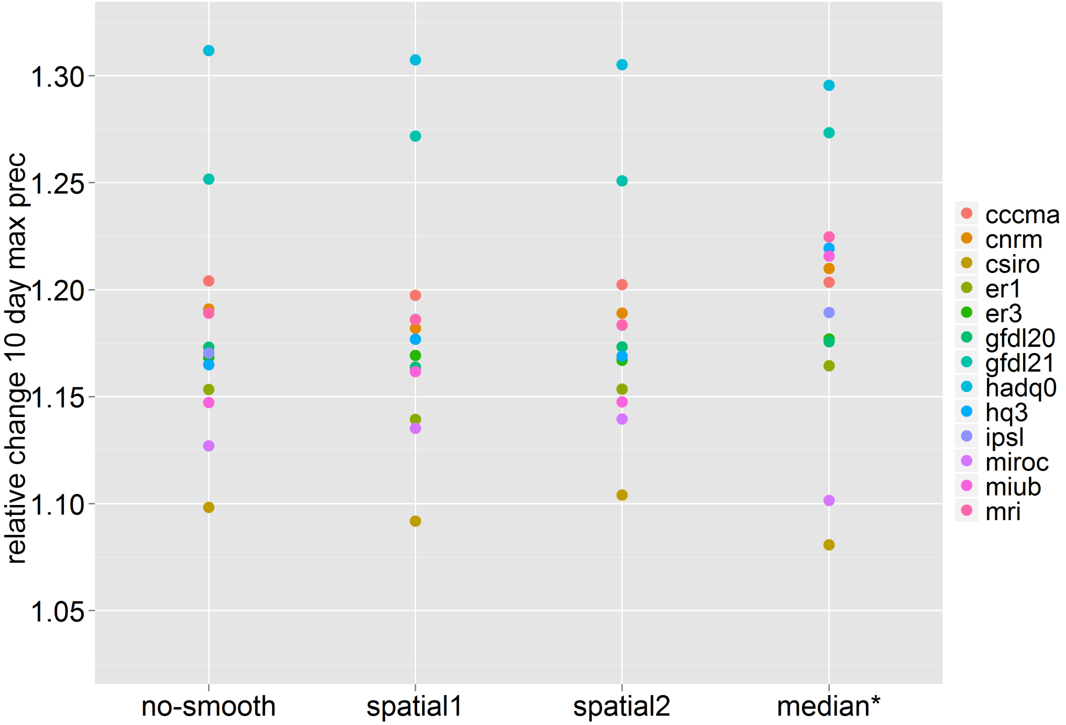

Fig. 3. Relative changes of the 10-year return level of the 10-day basin-average precipitation in the winter half-year (Oct – Mar) for each GCM. The figure shows the effect of spatial smoothing.