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Hydrology and Earth System Sciences Discussions

Interactive comment on "Climate change and hydrological extremes in Belgian catchments" *by* P. Baguis et al.

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We would like first to thank the Referee #1 for his/her detailed comments and constructive suggestions.

The first part of this reply addresses the fundamental critique on our methodology.

(1) Response concerning the referees' comment on methodology

This part of our replies will address the fundamental remark of the referees concerning the method used in this paper to assess climate change and its impacts on the hydrological extremes. Consequently, all related comments and questions are answered as well.

It is of course true that applying only the mean climate change signal to the time series used as input in the hydrological model is not sufficient to properly assess the change of the hydrological extremes. We decided to investigate another method, developed and presented in Ntegeka et al., 2010. This is still a perturbation method with the difference that now changes in extreme precipitation are properly taken into account. This is accomplished by perturbing both the wet-day frequency and the precipitation intensity following the principles of the quantile perturbation method (Harrold and Jones, 2003; Ntegeka and Willems, 2008). The analysis in Ntegeka et al 2010 goes further in the construction of climate change scenarios based on correlation analysis between the perturbations of precipitation and potential evapotranspiration (PET). The goal is to determine which combinations of rainfall and PET perturbations are physically possible in order to define the range of the impacts, expressed as scenario levels (low, mean and high). In the present study however we use the ensemble of the available RCM simulations and we analyze the results of the hydrological simulations.

Since the main interest here is the streamflow extremes, we set up and perform hydrological simulations by perturbing only precipitation, which is the dominant variable in this context, with the new perturbation approach. The other variables (temperature and potential evapotranspiration) are adjusted with mean perturbations. In this way any differences in the results for the extremes will be more consistently assessed. We will refer to the method used in the paper as “mean perturbation” and to the new one as “quantile perturbation” technique.

The quantile perturbation method is applied only on the Gete basin since the perturbation values have been obtained from the Brussels time series as the result of a project for the development of a perturbation tool for central Belgium. The Gete basin is a small one with little altitude change and not far from Brussels, so application of such perturbations is justified. On the other hand, the Ourthe river (eastern Belgium in the Ardennes region) exhibits more variable topography and quite different climate; consequently this river has been excluded from the new analysis.

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The results of the simulations are presented in Table 1 (see supplement). These should be compared with the results included in Table 5 of the article. The estimations based on the quantile perturbation method show small decreases on the p99 streamflow value compared to the corresponding p99 values obtained by applying mean perturbations on the precipitation time series. Nevertheless, no significance could be attributed to this decrease since the corresponding confidence intervals still intersect.

The explanation of this phenomenon must be looked for in the perturbed precipitation series. In general, the quantile perturbations produce more pronounced extremes than the mean perturbations. However, this alone does not always produce more extreme streamflow in the river system of Gete. The reason is that in some cases the precipitation extremes from the mean perturbations are accompanied by more precipitation before and/or after the extreme event, resulting in more abundant streamflow. Also, there are some precipitation extremes from the mean perturbations that are higher than the ones from the quantile perturbations. The combined effect of these factors produces slightly but not significantly lower p99 values for the case of quantile perturbations compared to the mean perturbations.

The previous comments on precipitation undergoing transformation by perturbations highlight the differences between the two methods adopted here. One has also to add that some precipitation events that survive after application of mean perturbations, are diminished in intensity or even completely disappear after application of quantile perturbations. The reason for this is that, in the quantile perturbation technique, the precipitation series is also adjusted for the wet day frequency. This is accomplished by randomly removing or adding wet days according to the corresponding wet day delta.

The obvious drawback of every perturbation method is that the data for the future climate are obtained by perturbing the historical series. However, with the quantile perturbations we take into account not only the change in the intensity at equal quantiles, but also the change in the frequency of the events. In this way the RCM dynamics under climate change conditions is properly taken into account for the most part. It would be however interesting to compare this perturbation method with some bias correction method, but this is beyond the scope of the present study.

(2) Response concerning the referees' recommendations

We will take into consideration all the recommendations kindly suggested by the referees in a revised version of the paper. In the replies below we mostly focus on the remarks and questions of the referees.

(3) Answers to the questions and points raised by the referee

General comment: The authors claim to evaluate the contribution of GHG-emission scenario and RCM to the uncertainty in their hydrological simulations. However, the manuscript is very unclear about the estimation of confidence intervals for the different experiments and lacks rigorous statistical significance tests. Moreover, the use of largely different ensembles (both in terms of number and origin of the samples) makes the comparison of uncertainty due to GHG scenario rather senseless.
A: Concerning the datasets used in the article, the biggest constraint is the structure of the PRUDENCE database. The SRES B2 scenarios constitute only a small part of the database, which is dominated by simulations based on the SRES A2 scenario. This is inevitably reflected in the analysis. The confidence intervals are calculated depending on the situation: for a unique simulation (control and mean of all scenarios) we use the variance of the p99 value, which depends on the extreme value distribution chosen. When an ensemble of p99 values is available, like in the case of the A2 and B2 ensembles, we calculate the mean of these p99 values and then we estimate the confidence interval by a non-parametric bootstrap method on the available sample.

Q: Page 3, lines 5-15: Have there been any changes observed in hydrological regimes in the regions mentioned that can be linked to the observed changes in meteorological variables?
A: This is not easy to answer. Many management actions took place in both areas of study during the last decades and isolating the effect of the meteorological changes in

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the hydrological regimes is beyond the scope of the present work.

Q: Page 4, line 10: Authors should provide more information about the projected shifts.
A: Will be addressed in a revised version of the article.

Q: Page 4, lines 11-14: It is not clear what the three climate change scenarios of Ntegeka (2010) represent (what does high, mean and low stand for in terms of (changes in climate conditions) and how they are derived.
A: See "Response concerning the referees' comment on methodology" above. The three level scenarios approach of of Ntegeka (2010) is not adopted here.

Q: Page 4, lines 14-16: Why are the climate change scenarios of Ntegeka (2010) not used in this assessment? Why is a different method adopted here?
A: Our goal in this article is to produce an ensemble of hydrological simulations. On the other hand, the applicability of the scenarios of Ntegeka (2010) is rather limited (see the comments for the Ourthe river in "Response concerning the referees' comment on methodology" above).

Q: Page 5, line 12: Scale in Figure 2 is missing.
A: Will be addressed in a revised version of the article.

Q: Page 6, lines 9-10: I find the grid resolution (7km x 7km) used somewhat large for the size of the catchments (especially for the Gete catchment, which comprises only 17 grid cells). Continental model applications these days even use finer grid resolutions (e.g., WaterGAP 5x5 arcminutes; LISFLOOD 5x5km). Is there any sub-grid variability taken into account in, for example, land use or soil properties? If not, variability that could easily be included, and more importantly, that affects the processes at the scale of application, is not accounted for.
A: In the model there are 9 types of land cover that are always represented by the appropriate variable fractions on each grid cell. Sub-grid soil properties are also indirectly taken into account through the regionalization process.

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Q: Page 6, line 21: How is streamflow routed?
A: The routing is handled through a 1D-model based on the width function (Naden, 1992). This information will be added in a revised version of the article.

Q: Page 7, line 11: The authors should provide more information on the meteorological dataset used (e.g., how many stations are used to derive the forcing fields).
A: See “Response concerning the referees’ recommendations” above.

Q: Page 7, lines 25-27 (continued on page 8): What is the expected (relative) magnitude of this source of error?
A: It is very difficult to tell since the corresponding competent authorities apply corrections to the series. The content of this paragraph will be examined again.

Q: Page 8, lines 9-12: The authors suggest here that the uncertainty in climate simulations is due only to the difference in spatial and temporal resolutions of climate models, which is not correct. There are several other factors, e.g., differences in conceptualization, parameterization, and initialization, that contribute to uncertainty in climate model output.
A: We are well aware that there are many sources of uncertainty in climate modeling. The previous excerpt is just a misleading statement from our part that will be corrected in a revised version.

Q: Page 8, lines 13-23: Redundant information.
A: Since the numbering of the referee seems to not exactly match the numbering of the online article, a problem present throughout the referee report, we are not sure which part of the article this refers to. We suppose that this concerns the second paragraph of section 2.3.2 (page 5040, line 21 – page 5041, line 12); it will be addressed in a revised version of the article.

Q: Page 10, first paragraph: The authors should be more specific on the data used from PRUDENCE. What models, what runs? What do the ‘small ensembles’ consist of?

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A: All simulations of the PRUDENCE database at 50 km horizontal resolution have been used. There are only two simulations at 25 km in the database; those two have been left out in order to have a more uniform dataset. Small ensembles means sets of control and scenario simulations obtained with the same RCM using ensemble techniques. For example, such sets from the DMI are the control simulations with codes HC1, HC2, HC3 and the scenario simulations with codes HS1, HS2, HS3 in the database. We will clear this up in a revised version.

Q: Page 10, lines 10-14: If the aim of this paper is to assess the impact of climate change on hydrological extremes, I do not understand why the authors rely on the delta approach using mean monthly perturbations. This implies that they neglect any change in climate variability, which prohibits a proper assessment of changes in hydrological extremes.
A: See “Response concerning the referees’ comment on methodology” above.

Q: Page 10, lines 19-24: What is the reason for excluding the outliers? Why artificially deflate the uncertainty? Also, it is not clear from the text, but does this mean that each model simulation (represented here by a perturbation of T, P and PET between scenario and control) for which (at least) one monthly perturbation of (at least) one of the three variables is an outlier (based on the definition provided), is excluded from the analysis? Or is only the month for the particular variable for which that model simulation is considered an outlier excluded from the analysis? In the former case, how many climate simulations are excluded (how many based on T, how many based on P and how many based on PET)? In the latter case, how are the data gaps filled and is consistency in the data ensured?
A: The outlier filtering is a reminiscent of earlier work on perturbations. In the current study all the perturbations have passed the filter and there is no exclusion or consistency issue for any simulation. We will state this more properly in a revised version.

Q: Section 2.3.3: The section on potential evapotranspiration is too extended with unnecessary detail that does not add value to the paper. **Q:** Page 12: The description

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and interpretation of the results in general is very poor throughout the paper. A clear example is lines 15-20. Results should not merely be reported, but the authors should (at least) attempt to explain the outcomes of their analysis.
A: Here is again the problem with the page numbers. We suppose that the referee refers to the first mention of validation results either in page 5045 (NS, RMSE) or 5046 (p99 validation). Both will be addressed in a revised version of the article.

Q: In Figure 3 (left plate) the yearly mean streamflow for the complete control series (in blue, 206 mm) is larger than that for the shortened control time series (in green, 205 mm), even though that the green curve is always above the blue one?
A: This is an error. The correct value corresponding to the green curve is 229 mm.

Q: Page 13, line 4: The paper is not structured well. First the authors should detail the methods used, and then they should describe the results. **Q:** Page 13, line 5: Table 3 can be omitted, as its content is fully contained in Figures 4 and 5.
A: Both will be addressed in a revised version of the article. Regarding Table 3, it will be merged with Table 4 as Referee 2 suggested.

Q: Page 13, lines 7-10: Why do you see more marked differences in p99 values? Why the differences between the EV distributions?
A: The p99 values are calculated by fitting a distribution on the sample of actual maximum values. This may amplify the existing difference in the magnitude of the extremes, depending on the sample used, since the calculations are performed for a return period of 100 years. In our case the sample extends to a period of 30 years at the most. Differences between the EV distributions can be attributed to differences in the sensitivity of their parameters.

Q: Page 14, line 11 and line 17 + Figures 4 and 5: How are these 90
A: In this case the confidence intervals are calculated from the distribution of the percentile p99. The jumps are unique to the GEV distribution and are due to the method of calculation of the variance of the percentile. In this calculation a piece-wise defined

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function is used; consequently, the jumps are just artifacts of the calculation procedure.

Q: Page 14, lines 11-25: The authors should not just report what the reader can easily see in the Figures or Tables, but they should try to explain, using physical reasoning, the results. For example, how do the changes in the catchments relate to the projected changes in climate, to what extent do the differences in hydromorphology play a role in the behavior observed for the catchments? **Q:** First paragraph section 3.2: Some parts are repetition; other parts should be either in the introduction or in the description of methods.
A: Both will be addressed in a revised version of the article.

Q: Page 15, line 1: It does not make any sense to compare the uncertainty between an ensemble containing 31 (A2) and 10 (B2) members! Not only should the number of members be the same, they should also originate from the same climate model experiments (GCM, RCM, ensemble member of GCM and/or RCM).
A: The RCMs/GCMs used in the B2 case are a subset of the ones used in the A2 case. Regarding the uncertainty comparison, we already have a comment on this in the second paragraph of section 3.2.2. But yes, we agree that any comparison should be based on samples of the same cardinality. This will be properly stated in a revised version.

Q: Page 15, lines 2-14: See earlier comments on discussion/explanation of results.
Page 15, lines 27-28: The analysis does not justify any statements on the comparison in uncertainty
Page 16, lines 19-20: This information should be given much earlier in the manuscript and should have been included in the interpretation of results reported earlier.
A: All will be addressed in a revised version of the article.

Q: Page 16, lines 9-12: This may be due to the application of the delta method using mean monthly perturbations and may not be the case when also higher-order changes in climate variables are taken into account.

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A: It is again not clear what part of the article this comment refers to due to the numbering issue already pointed out previously. If we take it as is (page 5048, lines 9-12) then it is unlikely that the new simulations could provide any useful insight since they use the quantile perturbations for precipitation and the mean ones for the other variables. If it refers to page 5050 (second paragraph of section 3.3 on commutativity), then it is an issue we can investigate in a revised version of the article using the new simulations.

Q: Page 18, lines 5-6: Does this mean the confidence intervals for case 1, for which also an ensemble of runs is available, are computed differently? And what about case 3, for which only one run is available (based on mean climate)? Throughout the manuscript, it is very unclear how the confidence intervals are derived for the different experiments. Since confidence intervals and uncertainty bounds depend on the method used to derive them it is not good practice to use alternative methods for the various experiments.
A: You are right here; we create a lot of confusion. Here is how we proceed. Whenever there is an ensemble of simulations, we calculate the statistic in question (p99 value) for each member of the ensemble and then we take the mean value. The confidence interval of the mean value is then determined by a non-parametric bootstrap method on the ensemble of the p99 values. We consider here 10000 iterations. On the other hand, when we say "control simulation" for the hydrological runs, we mean the unique run with the SCHEME model in which we use as input the interpolated meteorological fields from observations. In such cases where there is only one p99 value resulting from a unique simulation, we calculate the confidence interval using the variance of the p99 estimator. These cases are just the control run explained previously and the run where we use the mean scenario.

Q: Sections 3.3.1 and 3.3.2: Description of the EV-fitting should go in the section on methods.
Sections 3.3.1 and 3.3.2: See earlier comments, proper discussion of the results is lacking (e.g., commutativity issue).

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Page 20, line 12: The authors state that the two catchments are completely different from a hydrological point of few, but fail to incorporate this important aspect in the discussion and interpretation of the results.
Page 21, line 16: These results are of utmost importance to interpret the results of this study and the authors should integrate them better in this work.
Page 21, line 23: Why is this information not provided earlier? See also earlier comment on using different methods of deriving confidence intervals.
A: All will be addressed in a revised version of the article.

References

Harrold, T.I. and Jones, R.N.: Downscaling GCM rainfall: a refinement of the perturbation method. MODSIM 2003 International Congress on Modeling and Simulation. Townsville, Australia, 14-17 July 2003.

Ntegeka, V. and Willems, P.: Trends and multidecadal oscillations in rainfall extremes, based on a more than 100-year time series of 10 min rainfall intensities at Uccle, Belgium. Water Resources Research 44, W07402, 2008.

Naden, P.S.: Spatial variability in flood estimation for large catchments: the exploitation of channel network structure. Hydrol. Sci. J. 37, p:53-71, 1992.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/7/C4893/2011/hessd-7-C4893-2011-supplement.pdf

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 5033, 2010.