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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.

Anonymous Referee #1

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General comments

This manuscript presents an assessment of the impacts of climate change on hydrological regimes - in particular extremes - in two Belgian catchments. The topic is well suited for HESS but the manuscript and underlying analysis lack the quality to be accepted. This manuscript does not reveal any new information nor does it provide new insights or developments in the field of climate change and hydrology.

A crucial shortcoming of this exercise is that the method used (delta approach using mean monthly perturbations) prohibits a proper evaluation of changes in hydrological extremes. Changes in extremes can be caused by changes in mean climate, changes in the variability, or a combination of both. Here the authors only consider changes in

mean climate, neglecting changes in the variability or shape (e.g., skewness) of the frequency distribution of climate variables.

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.

The manuscript is not written clearly and structured illogically (e.g., description of the methods on extreme value fitting after presenting results). Results are merely reported, explanations often lacking, or suggestive, without in depth physical reasoning. The specific comments section below provides more details on these issues.

I encourage the authors to take into consideration the suggestions below to improve the manuscript as well as the analysis, before resubmitting a new paper.

Specific comments

- Introduction: The introduction focuses solely on the climate aspect of the problem. It lacks a description of the state-of-play on hydrological impact studies and how their work relates to recent developments in this area. As a consequence, the authors fail to convey the novelty of their work.

- 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?

- Page 4, line 10: Authors should provide more information about the projected shifts.

- 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.

- 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?

- Page 5, line 12: Scale in Figure 2 is missing.

- Page 6, lines 9-10: I find the grid resolution (7x7km) 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.

- Page 6, line 21: How is streamflow routed?

- 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).

- Page 7, lines 25-27 (continued on page 8): What is the expected (relative) magnitude of this source of error?

- 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.

- Page 8, lines 13-23: Redundant information.

- 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?

- 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.

- 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?

- Section 2.3.3: The section on potential evapotranspiration is too extended with unnecessary detail that does not add value to the paper.

- Page 12: The description 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.

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?

- 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.

- Page 13, line 5: Table 3 can be omitted, as its content is fully contained in Figures 4 and 5.

- Page 13, lines 7-10: Why do you see more marked differences in p99 values? Why the differences between the EV distributions?

- Page 14, line 11 and line 17 + Figures 4 and 5: How are these 90% confidence intervals derived? Why the jumps in the uncertainty bounds at certain return levels?

- 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?

- First paragraph section 3.2: Some parts are repetition; other parts should be either in the introduction or in the description of methods.

- 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).

- 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.

- 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.

- 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.

- 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).

- 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.

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