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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 #2 for his/her detailed comments and constructive suggestions.

The first part of the reply to the Referee #1 report addresses the fundamental critique on our methodology and will not be repeated here.

Q: It is not clear what the time step of the model is; throughout the paper only monthly climatologies are shown. Mention this in the model description. In the model validation part, I would like to see some additional validation plots at a sub-monthly time step, as this might give insight in the relatively poor model performance. The authors men-

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tion that results were obtained for the entire Scheldt and Meuse basins, whereas the manuscript focuses only on two relatively small sub-basins, that are small with respect to the spatial resolution of the PRUDENCE data, which does not have a particularly high resolution (25 km). Both basins are covered by only very few RCM pixels, so small spatial shifts in persistent weather patterns (as sometimes happens in RCMs eg when the topography is not well represented) can have large influences. Why not show the results for the entire basins? The model has been calibrated using 'a variety of Belgium catchments', after which the resulting parameters have been regionalized. Obviously, given the very different nature of the two basins and the fact that the model is very conceptual (as opposed to physically-based), each basin would require quite different parameter values. I doubt that this is achieved to a sufficient degree by the regionalization process, as is also shown by the relatively low Nash-Sutcliffe values, especially for the Gete basin. Thus, why didn't the authors choose to calibrate each basin separately? Also, very few details and no references are provided for the regionalization process. Please elaborate more about that.

A: The model time step is equal to one day. This will be reported in the model description. Concerning the regionalization process: the model was initially calibrated using data from smaller basins than the ones considered here. Our results come from a broader study over Belgium in which all the important basins of the country have been included. For this we needed the regionalized version of the model and we are well aware that this would degrade the model performance. However, the climate change impacts have been found to be qualitatively similar everywhere in the basins considered and this is the reason why we show only one representative case from each basin.

Q: The authors state that the delta approach prevents effects of climate model biases. While this is true to some extent, it has some important disadvantages. It perturbs only the mean monthly values, and it is assumed that other moments or percentiles of the distribution are changed in the same way. Given the non-linear behaviour of hydrological extremes, as is illustrated by the analyses regarding the commutativeness, this is a dangerous thing to do, and it should be very well kept in mind when interpreting results

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of the extreme value analysis in this manuscript. Direct usage of RCM output (using a bias correction) does take into account changes in all moments of the distribution, and is in principle thus better suited to assess changes in extreme streamflow. This should at least be mentioned.
A: See "Response concerning the referees' comment on methodology" found in our reply to the referee 1 report.

Q: Another aspect that worries me is the analyses regarding extreme river flows. In the model validation, the estimation of the 100-year return values between observations and simulations are compared. To derive these values, a probability distribution is fitted. Why choose a value that is so much larger than the extent of the data period?
A: There has been much interest from Belgian colleagues to show results in this range of return period for impact studies. Actually this has been explicitly requested by other teams in the context of the project study.

Q: The real extremes would be the minimum and maximum of the p99-ensemble.
A: Of course we can include those as well in our analysis.

Q: Introduction 5034/25-26: Add a reference to the source of these numbers, of mention the data used to calculate them.
A: These numbers come from RMI synoptic stations. This will be reported in the article.

Q: 5035/10-15: This sentence is a bit long and somewhat confusing, please reformulate.
A: Will be addressed in a revised version of the paper.

Q: 5035/From line 28: I do not completely agree with this. Even in the simulation of global mean temperatures there is a large range in GCM results (about 3 degrees; see e.g. Reichle et al, 2008 or Covey et al., 2003).
A: Will be addressed in a revised version of the paper.

Q: 5036/11: In the reference list, there are two instances of Dankers et al., 2009. Which

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one is meant here?
A: It is the second one.

Q: 5037/2 (and later): The authors claim to estimate the uncertainty based on the choice of the emission scenario. However, not only are the numbers of both scenarios simulations different (31 versus 10), but also it is very unclear which models are forced using which scenarios, making it very difficult to make statements about the uncertainty from emission scenarios.
A: The models used in the creation of the A2 and B2 series of simulations are the same, with the B2 being a subset of A2 from this point of view. The most important difference here is the number of simulations in each SRES scenario considered and the nature of the scenario themselves but this is how the PRUDENCE database is built. There are already comments on this in the article but we will try to improve on it.

Q: 5037/21: Figures 1 and 2 should be switched, as Figure 2 is referenced first.
5038/8-9: Be more precise here, mention the exact fractions and annual precipitation.
A: Both will be addressed in a revised version of the paper.

Q: 5038/25: Roulin et al. 2001,2002 refers to one report with two year numbers. Which is it?
A: Actually, this consists in two separate reports and both are pertinent here. We will split this reference.

Q: 5040/9-11 and Figure 3: In Figure 3 two control simulations are presented for the Gete, one showing the full series and one corresponding to the observations. The full series (not corresponding to the observations) seems to be closer to the observations throughout the year. Do the authors know why this is?
A: The better fit of the complete series seems to be just a coincidence.

Q: 5040/15-19: Do you have any idea (or references) about the magnitude of this error?
A: As we said in the reply to Referee 1, it is very difficult to tell since the corresponding competent authorities apply corrections to the series. The content of this paragraph

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will be examined again.

Q: 5042/21-24: See general comments about the delta approach. Here, some references should be added about the model bias. Concerning stationarity approach: it is known that this assumption is very questionable (Christensen et al., 2008). It does, in that respect, not matter whether the delta approach or direct GCM output is used as they both assume stationarity. An important disadvantage of the delta approach, however, is the fact that it only perturbs the monthly averages and not those in other moments of the distribution (i.e, extremes) that can change very nonlinearly.
A: See "Response concerning the referees' comment on methodology" found in our reply to the referee 1 report.

Q: 4045/10-15: As mentioned before, daily streamflow time series of observations and simulations would be helpful. **Q:** 4045: Tables 3 and 4: Why not merge these? They contain very related and similar information and could easily be combined.
A: Will be addressed in a revised version of the paper.

Q: 5047/21: It is not clear what exactly this calculation to obtain overall maximum/minimum value is. Are these just the lowest/highest values that occurred in all simulations? How are they not actual simulations?
A: We explain it in the text. Probably improvements in the phrasing are necessary. This is how it works. For a given month, we have an ensemble of values. From these values we just find the minimum, maximum and mean and we proceed in the same way for every month to construct the curves. These curves do not correspond to actual simulations in the following sense: if for example the maximum of January comes from simulation 3, there is no guarantee that the same simulation will give the maximum for the other months too and of course this does not happens. In other words, the monthly max/min series does not come from the same simulation.

Q: 5047/26: This is not very surprising and hardly a result, given the very asymmetric distribution between A2 and B2 simulations.

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A: We agree that it is necessary to state this more properly.

Q: Figs 6 to 11: Here it would be useful to add the max/min values for the reference simulation, as it also interesting to discuss how extreme the changes are wrt to the range of natural variability in the control period.
A: Will be addressed in a revised version of the paper.

Q: 5049/9-16: The control values for low flow at Halen and high flow at Angleur are suspiciously similar – they do not seem to be in Figures 8-11. Maybe a typo?
A: High and low flows are defined in terms of the 0.95 and 0.05 percentiles as numbers of days above or below these percentiles. Consequently, in both cases there is the same 5% of exceedance. In a year (365 or 366 days) this translates to around 18.3 days. Regarding Figures 8-11, the distribution of the events may change for two different basins but the sum must always be around 18.3 for the control simulation.

Q: 5050/Fig 12: This Figure does not add much, as the differences between the lines are not visible.
5051/11: “Especially” –> “except for” (would seem more logical)
5035/19: dissapears ! disappears
5038/4: “...consists OF crops”
5044/17: “River sTreamflow results”
A: All will be addressed in a revised version of the paper.

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