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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 #2

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This manuscript presents an assessment of the effects of climate change on mean and extreme streamflow in two small Belgian catchments. I find the paper fairly clearly structured and written. The presented results are very similar to those of previous studies and do not add much new in that sense. In addition, I do have some crucial concerns regarding the methodology and the hydrological set-up, that are listed below. The revisions that would be necessary for publication are of such nature that I cannot recommend to accept the manuscript in its current state.

General comments

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

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Interactive Discussion

Discussion Paper

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 mention 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.

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 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.

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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? The fitting of the extreme value distribution and its ability to extrapolate to such extreme values probably adds a high degree of uncertainty. In addition, the p99-values from all ensemble members are averaged and compared with the p99-value obtained from the average climate forcing. While this is a useful exercise to illustrate the non-linear reaction of the extremes, they are not the most useful values for analysis of changes in extreme values due to climate change. The real extremes would be the minimum and maximum of the p99-ensemble. In addition to that, the use of the delta approach prevents a proper analysis of extreme values, as only monthly mean values were perturbed.

There are some references in the list that are not mentioned in the text, and also there are some long and confusing (and sometimes grammatically incorrect) sentences, of which some examples are listed below. Please check all this carefully and change accordingly.

Specific comments

Introduction 5034/25-26: Add a reference to the source of these numbers, of mention the data used to calculate them.

5035/10-15: This sentence is a bit long and somewhat confusing, please reformulate.

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).

5036/11: In the reference list, there are two instances of Dankers et al., 2009. Which one is meant here?

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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.

Hydrological setup

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.

5038/25: Roulin et al. 2001,2002 refers to one report with two year numbers. Which is it?

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?

5040/15-19: Do you have any idea (or references) about the magnitude of this error?

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.

Results

4045/10-15: As mentioned before, daily streamflow time series of observations and simulations would be helpful.

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4045: Tables 3 and 4: Why not merge these? They contain very related and similar information and could easily be combined.

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?

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

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.

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?

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)

Technical corrections

5035/19: dissapears → disappears

5038/4: "...consists OF crops"

5044/17: "River sTreamflow results"

References Christensen, J. H.; Boberg, F.; Christensen, O. B. & Lucas-Picher, P. (2008) "On the need for bias correction of regional climate change projections of temperature and precipitation", Geophys. Res. Lett., 35 , L20709, doi:10.1029/2008GL035694

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