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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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Can I first thank the reviewers for a detailed analysis of the paper and 3 very well considered reviews. The paper requires some considerable revision and in the responses by the authors to the reviews they clearly recognise this directly. The paper did not score positively in the review process and therefore at the very least major corrections are required to bring this up to a standard that could be published in HESS. The reviewers in general recommended rejection / major changes. With regard to the responses I believe it's fair to say for most minor points the authors do seem to provide sensible replies and effective changes to the document, so these should be able to be incorporated into the manuscript based on these responses. Regarding the overall issues with

the paper I wish to note my initial editor assessment of the paper, that suggested "We also have a paper that really hasn't advanced in any way a relatively basic procedure of delta change to reflect bias, nor is the real issues of doing this mentioned well, and certainly I would say when it comes to extremes the paper has not really tackled these issues. So I would say I find the current paper a little on the poor side....." I believe this has also been brought out by some of the reviewers comments for example reviewer 1 states "This manuscript does not reveal any new information nor does it provide new insights or developments in the ïňĄeld of climate change and hydrology". Reviewer 2 stated "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" and finally Reviewer 3 wrote "More majorly, however, there are a few fundamental flaws to the methodology used in the paper that need to be addressed, or at least examined". I am highlighting these comments to set the scene and ensure the authors fully appreciate there is a general consensus that the paper is not acceptable in it's current form and there are very major edits that need to be done to rectify this. Specifically I shall concentrate on the main issues where an improvement needs to be developed in the methodology, not just in terms of presentation changes and different ways of discussing the findings (as I noted above the authors gave some positive responses to the various detailed comments that are not discussed here but would need to be completed in addition).

1) Rev 1, Rev 2 and Rev 3: The paper does not deal with extremes using the simple delta change methodology for monthly means: The authors appear to be suggesting they will change this method and discuss extremes, this is welcome. However such results need to be consistently applied, it is not acceptable in my view to not have a methodology that deals with extremes in the study catchments presented. If for some reasons methods of quantile change are not appropriate for the Ourthe catchment then the authors need to implement a scheme that is justified and they can defend by reasoned discussion. I did not see how the comments they made about the heterogeneity of the basin excluded a better methodology begin developed above mean bias correc-

tion, you could counter argue due to these basin characteristics the method chosen is in any case nonsensical. But the paper will not pass review unless the extreme changes are dealt with for all the results obtained

2) Rev 1: 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 conïňĄdence intervals for the different experiments and lacks rigorous statistical signiïňĄcance tests: I have to say I did not find the authors response compelling in the reply. If there are issues with the PRUDENCE data on which the paper is based then there is a credibility issue to the science presented OR the authors drop the fact they are analysing, with appropriate methods, the contributions of different scenarios. This must be improved (method and clarity of techniques) or dropped, it could be almost suggested in the authors response there is not the appropriate evidence to do a proper job on this!

3) Rev 2: There is concern about the use of the hydrological model and how this has been regionalised: This is what is being used to express the impacts of change and so it is a critical issue for the study. I agree with the reviewer this has not been dealt with well and I also do not think the author response is satisfactory to this issue (in fact it was very weak given the importance). Other colleagues have dealt with climate change impacts by fully dealing with the uncertainties in model predictions and/or using an ensemble of models to comment on how the results differ by using different models to explore the impacts. Using only one model simulation and obtained from a regionalisation process is not a satisfactory basis to comment on the impacts of change in my view and the view of the reviewers. So I think the credibility of the model choice and calibration needs a more detailed analysis to justify it's use and the potential uncertainties in the modelling process.

4) Rev 2 and Rev 3: How can you express 100 returns from extrapolation from a shorter model run and what implications does this have?: I believe the authors need to justify their approach more than their response suggested they might. There is a difference

between the need to show such results for Belgium and how this is developed and justified in a science paper. It may well be the authors could add a context to the results they are trying to show for national strategic reasons but this should not change the focus for a journal paper on appropriate scientific methods. There are clearly some difficulties in extrapolating to longer return periods than data is available for and again there are various ways of approaching this in the literature (i.e. continuous simulation for a longer run period using a rainfall generator). I suggest you check the use of more than one type of distribution in your results to see how much this changes the results presented. It concerns me that the response from the authors suggest they will not deal with this important issue and justify their current analysis on what needs to be reported. I do not think this is satisfactory

Given the rest of the authors responses I would say they tackle the reviewer comments well. But a strategy needs to be developed for the above 'critical points'. There is a suggestion here that 1) has been considered but as I said requires consistency in the approach for the example areas. So there are major corrections here needed that mean an improvement to the methodologies chosen, kind regards, Jim

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