

Interactive comment on “The Hydrological response to climate change of the Lesse and the Vesdre catchments (Wallonia, Belgium)” by A. Bauwens et al.

Anonymous Referee #2

Received and published: 13 December 2010

This paper concerns the impacts of climate change on the hydrology of two sub catchments of the Meuse. This review is given in light of the comments given by the other reviewer. I agree with the points made by the previous reviewer and what is written here is intended to supplement these. I would suggest that the authors get the paper proof read and use more professionally accepted terminology in places.

Overall it is my opinion that the paper needs somewhat of a revision, with the main weaknesses derived from the following areas:

1.) I am extremely skeptical about the results presented in Table 4. How is it possible

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



that the high and low scenarios used for rainfall (the most uncertain of variables) show exactly the same changes in spring and summer. The authors need to reappraise these findings and if found to be correct need to provide a coherent reasoning as to why no uncertainty exists.

2). a lack of critical awareness in the application of off-the-shelf packages which I believe has resulted in a lack of detail on key decisions taken (the scale of which could have a major impact on the results obtained). A greater depth of critique on the steps taken and on the assumptions made would strengthen the paper significantly, where at present I think that the authors are over-simplifying the challenge they set out to tackle.

2.) Novelty of the paper: It is not clear what the added value of the paper is. Is it the addition of knowledge on future impacts to the considerable work done in this geographical area previously or is it the application of a physically based model (or both?). In relation to the former the literature review needs to summarize previous work more efficiently i.e. what scenarios, GCMs, downscaling techniques, impacts models, were used and main impacts, uncertainty ranges identified, so as a more effective and fairer comparison can be made. I would suggest that the presentation of this information in a table might help the reader position this paper with greater clarity. If the latter I think further work needs to be done to present evidence on why physical models are more appropriate than conceptual.

3.) Assessment of uncertainty: It is my opinion that the paper is weak on the assessment and quantification of uncertainty. The use of numerous GCM/RCM output is to be welcomed but the authors need to clarify assumptions and decisions taken. No account is given of uncertainty from hydro model, particularly given the use of simplified input data, grid scale lumping, calibration of transfer functions. How different would the results be if a different objective function was used for fitting during calibration, if a different calibration/validation period was used? In such a complex model has the identifiability of model parameters been tested. My own experience is that major identifiability problems are introduced in such models and can contribute significantly to

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

uncertainty ranges in future impacts.

In the application of the CCI-HYDR tool, more depth of information is required on ensemble creation. The results are presented as definitive yet no discussion or flagging is given to whether the ensembles cover the model space, how many GCMs are used to drive RCMs etc. It seems to me that ensembles are derived as a simple mean of runs considered, does this allow for greater weight to be given to those that capture current mean and variability of climate, i.e how is weight given to those runs that are obviously better at simulating current climate.

Having read response to comments of previous reviewer, might it be interesting to tease out reasons as to why validation performance is so different for the two catchments modeled?

On page 7702 the authors make the statement 'For the other target years, the interpolation and extrapolation of the changes leads to less accurate future perturbations.' How can this possibly be known??

The presentation of the methodology as 2 main steps vastly oversimplifies the process and does not give due regard to the importance of decisions making these steps.

If the paper is comparative in nature, what influence would the use of different reference periods have? Additionally, if best practice for the perturbation tool is 61-90, what is the sensitivity of future impacts to the 67-00 period used. In my experience the use of even moderately different reference period can induce large changes in the results. Has this been tested?

In the analysis of extremes it seems to me, although it is not clear, that the scenarios are built on averages..ie which model is driest on average for seasons. Does this serve to dampen out extremes and result in underestimated impacts and uncertainty ranges? In addition, how is the issue of non-stationarity dealt with for the application of statistical methods? What assumptions are made. Can we have confidence in the estimation of

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



the 100yr flood from such a dataset?

In the perturbation of the scenarios, which also strikes me as a delta change method, how applicable is this for the analysis of extremes and how robust can the results be is additions statistical characteristics are not accounted for. The authors need to be clear about the limitations associated with the methodologies employed.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 7695, 2010.

HESSD

7, C4035–C4038, 2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C4038

