

Interactive comment on “Discharge simulations performed with a hydrological model using bias corrected regional climate model input” by S. C. van Pelt et al.

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Paper: Pelt et al., Discharge simulations performed with a hydrological model using bias corrected regional climate model input

GENERAL COMMENTS

This manuscript report interesting findings of interest for the scientific community, and as such it deserves to be published. The manuscript is generally well written. The methodologies are generally sound, although I have some reservations regarding some of the results coming out of the HBV simulations (see specific comments below). In

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addition, there are a number of minor aspects that need to be improved, see below. I therefore recommend that the paper be subject to major modifications before it is accepted for publication in HESS.

SPECIFIC COMMENTS

1. Page 4593, line 2: Insert 'of' after 'results'
2. Page 4593, last two paragraphs: This is more a description of methodology (and duplication with chapter 3) than text for an Introduction. This could be shortened and refocused on specifications of objectives for study.
3. Page 4596, section 3.1.1: Has the HBV model been calibrated? If so – how was this done? And are the results shown in Fig 4 for calibration period or for an independent validation period? More information must be provided on this. Information on and assessment of HBV model parameters are also required.
4. Page 4596, section 3.1.1: With my knowledge to the HBV model I am surprised that the HBV model simulation is so biased as can be seen in Fig. 2. It looks like lacking/poor calibration.
5. Mwet: It is not clear to me what Mwet is. According to the caption of Table 2 Mwet is the number of wet days, but that does not make sense with the numbers in the table. Is it the average number of rainy days in sequence? Please provide a clear definition and provide the unit for Mwet.
6. Page 4598, PET: Why not calculate PET directly from the regional climate model from climate variables and e.g. a Penman equation instead of using an empirical equation like (3) that apparently has not been tested.
7. Chapter 4, bias corrections for 15 sub basins: Only the average of the bias correction is shown. What is the difference among the 15 sub basins?
8. Page 4602, Table 3 – correct results?: It is very surprising that a difference of less

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than 1% in annual precipitation between observed and two bias corrected precipitation series in the control period can generate 5 and 7% differences in simulated runoff. I do not understand why and I am sceptical about the result, because it is counter-intuitive and not in agreement with results I have seen from similar calculations. It could be argued that the 5-7% is generated due to differences in precipitation pattern/structure between the observed and the two bias-corrected series. However, a calculation in a recent Danish PhD thesis (Like van Roosmalen, June 2009, University of Copenhagen) showed a difference of only 1.6% in average simulated runoff in the control period between two different bias corrected methods of which one was a delta change correction (i.e. same precipitation structure/pattern as observed series) and the other was a direct method (in principle of the same type as the MV method used here). The Danish regime is not much different from the Meuse. The authors should examine the results closer and explain how this can happen. Is there any difference in PET series? Are there unrealistic parameter values in the HBV?

9. Page 4602, Table 3 – implications if results are correct: If the result is correct (cf above comment) the result is scary, because it implies that the bias correction generates a large uncertainty of direct consequence for climate change impact assessments. 5-7 % ‘error’ on the runoff simulation in the control period is significant, but this is further reinforced by the 10% variation between the result of the two corrections methods for the scenario periods. This uncertainty generated by the bias correction methods must be discussed and emphasised as a key finding in the conclusions.

10. Fig. 3: I am a bit surprised that the bias corrected values show so much remaining bias at all, because the bias correction apparently have been done for five day periods (section 3.2.1). Probably I have misunderstood something, but this is not clear to me.

11. Fig 4: This figure deals with precipitation, but this is not shown in the figure nor mentioned in the figure caption. This should be made clear.

12. Reference list: The reference list is not in accordance with the references in the

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text. For example Jacobs et al. (2007) is slacking in the list; Leander et al. (2005) on line 2 of page 4605 should probably be 2007 or 2008 instead; and De Wit (2007) is lacking in the list. Please check and correct all references carefully.

13. General issue on bias correction: The two bias correction methods have been fitted/calibrated to fit the climate in the control period. What is ultimately interesting in a climate change context is to which extent the bias correction fitted for the control period also will be correct for the scenario period. This should be discussed somewhere in the paper.

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