

## ***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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Received and published: 22 September 2009

We would like to thank you for your valuable comments about our study. The textual comments and minor remarks are being dealt with and corrected where needed. The other comments/major remarks we will briefly discuss below.

Major remarks

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  
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assessment of HBV model parameters are also required.

Response: The original calibration of the model of HBV by the Meuse was carried out by Booiij (2005) for more information on the calibration and the HBV parameters see Leander et al. (2005) and Booiij (2005). This information is also added to the text.

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.

Response: This study was conducted with the "official instrument" of the Dutch Government also used in many other studies (see PhD Thesis Leander for instance for references). Ongoing work by Deltares showed that the model calibration of the HBV can be improved but these results have not been published yet.

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.

Response: Mwet is the wet day average, calculated as the average amount of precipitation on days with  $\geq 0.3$  mm precipitation. The unit for Mwet is mm day<sup>-1</sup>. This information is included in the caption of Table 2.

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.

Response: Potential evaporation is a not well defined concept in coupled land-atmosphere physics. Although it can be calculated straightforwardly from the Penman-Monteith equation when a proper reference surface is defined and when the overlying atmosphere is considered to be static, but it is unclear what the physical interpretation is when atmospheric boundary layer feedback is also allowed, and atmospheric

conditions will respond to enhanced (potential) evaporation and reduce the evaporative demand. Therefore, calculation PET from an atmospheric model like RACMO is not straightforward and not included in the standard output of the model. Additionally, the effect of changing PET on high discharge volumes, for which the bias corrections studied in our paper are most relevant, is not very strong. We therefore feel that our pragmatic solution to incorporate spatial/temporal variability of PET via a linear relation with temperature is justified.

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?

Response: There is some difference in bias among the sub basins. The standard deviation of the MV bias correction method is 3.2, for the WD bias correction method 6.0 and for the uncorrected RACMO run 19.5. This indicates that the difference among the basins of the uncorrected RACMO run is much larger than for the bias corrected runs. The bias of the different sub basins follow the same trend for the MV method and the uncorrected run. The average result is not influenced by one basin in particular. For the WD method, one basin does have extra influence because the bias of this basin is significantly larger than for the other basins. This basin (11) is a basin with high precipitation amounts. A comment is made in the bias correction section.

8. Page 4602, Table 3 – correct results?: It is very surprising that a difference of less 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

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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?

Response: A percentage change in precipitation would be equal to a percentage change in run-off if these were linear related. This is not the case. Run off is the closing component in the water balance. A change in precipitation will on year basis, with a constant evaporation and a negligible storage, completely be compensated by changes in run-off. If run-off/precipitation on a year basis is 40 % (which is a reasonable estimate for the Meuse), a one percent change in precipitation will lead to a  $1/0.4 = 2.5$  % change in run-off. This means that run-off will change relatively stronger than precipitation. However the difference in this paper is higher than 2.5%. The ratio precipitation/ run off is higher in the RACMO runs than in the observed runs. This means that the evaporation of the RACMO runs is lower. Different causes could lead to this result, for example the season cycle of precipitation in the RACMO runs could be stronger, which results in lower soil moisture in summer, which causes less evaporation. An indication for this can be seen when examining the frequency of wet days in summer, for both bias correction data this number is lower than the observed percentage. Another option is that the precipitation PDF of RACMO has more extremes, which result in more frequent run offs, however this cannot be seen in the precipitation data of this study. The reference has been added and the paper elaborates more on this problem.

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

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for the scenario periods. This uncertainty generated by the bias correction methods must be discussed and emphasized as a key finding in the conclusions.

Response: Additional comments to the text have been made.

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.

Response: For more explanation on the used temperature bias corrections I refer to the article of Leander (2007). In this article the method is clearly explained. It is true a certain biased remains, but it should be emphasized, as stated by Leander (2008) that the impact of further temperature bias corrections on simulated extreme flows is probably low.

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.

Response: This remark has been taken into account in the discussion section.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 4589, 2009.