Hydrol. Earth Syst. Sci. Discuss., 8, C1994–C1998, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C1994/2011/


Hydrology and Earth System Sciences Discussions

HESSD

8, C1994–C1998, 2011

Interactive Comment

# *Interactive comment on* "Skill assessment of a global hydrological model in reproducing flow extremes" *by* N. Candogan Yossef et al.

**Anonymous Referee #3**

Received and published: 6 June 2011

General comments

This paper investigates the ability of a global hydrological model PCR-GLOBWB to reproduce flow extremes using the example of 20 large river basins located all over the World. The model performance is evaluated using different efficiency measures and skill scores. The paper is well written and features a clear structure. However, the methods and terminology applied are not common in the community addressed by the title which makes the paper initially difficult to access. According to the introduction, the main objective is to evaluate the potential skill of the model to forecast flow extremes. Although stated by the authors that the prospects for forecasting hydrological extremes are positive, the results lag behind expectations. The main strength of the contribution lies in the application of skill scores for model validation (or testing), which are rarely used in hydrological modelling yet are an interesting way of judging our models' performances.

Comments chapter 1

Global hydrological and land surface models are introduced in chapter 1. Here, a discussion on studies focussing on hydrological extremes is missing, e.g. Lehner et al. 2006, Hirabayashi et al. 2008. (Hirabayashi, Y., Kanae, S., Emori, S., Oki, T. & M. Kimoto (2008): Global projections of changing risks of floods and droughts in a changing climate. Hydrological Sciences Journal, 53(4), 754-772.)

p. 3472, line 14-16: The sentence stated by the authors is rather subjective and the results obtained in this study, however, cannot be generalised.

Comments chapter 2

Chapter 2.1

PCR-GLOBWB operates on a 0.5 by 0.5 degrees resolution but sub-grid variability is taken into account. Does this mean that soil type, land cover and other input data are considered for model calculation on a higher resolution? Is runoff generation calculated on sub-grid level? On the other side the river routing is based on DDM30.

Chapter 2.3

The simulated daily river discharge is summed-up to monthly values for any further analysis. The question which then arises is why this advanced approach of temporal downscaling of climate input was chosen. At least for some of the GRDC stations observed daily discharges are available. Here a comparison between daily and monthly results would be highly interesting.

Comments chapter 3

Chapter 3.1



HESSD

8, C1994–C1998, 2011

Interactive Comment

In large-scale modelling the usage of correction factors is common. However, does it make sense to bias-correct station discharges? With the method applied the water balance is not closed, i.e. discharges do not correspond to any other state variable in the upstream area. Thus the bias-corrected discharges are only valid at the given location and have no significance for the rest of the river basin.

p. 3476, line 16: the term climatology is confusing as it refers to monthly mean discharge.

Chapter 3.2

In this chapter GS is introduced but the description should be more detailed.

Chapter 3.3

The authors should describe how they derived the 5-yr return levels for floods and droughts.

p. 3480, line 27 to p. 3481, line 12: This paragraph should be shortened and only concentrate on PSS and why it was chosen.

Comments chapter 4

Chapter 4.1

p. 3481, line 24ff: I don't observe reasonable agreement in the graphs shown in figure 2 using uncorrected results which is also confirmed by the efficiency criteria listed in table 2. Additionally, it becomes visible in figure 4 as well.

p. 3482, line 19-20: A better agreement after correction should be the purpose of this exercise.

Chapter 4.2

The thresholds of Q25 and Q75 set for anomalous flows are rather low as 50% of all values are actually considered "anomalous". Furthermore, how do the observed



HESSD

8, C1994–C1998, 2011

flow anomalies relate to precipitation anomalies of the given month (and the previous month)? I get the impression that precipitation anomaly compared to mean climatology is a strong predictor for the occurrence of anomalous flows as defined here which would also true for any forecast.

Chapter 4.3

The outcomes of the PSS reflect again, that the model is able to reproduce floods better than droughts. The methodology applied to assess the skill in reproducing floods and droughts (PSS) is reasonable but comprises some weakness which should be discussed as well. Here, the weakness of PSS is hidden in the second term of the equation (eq. 4) in which the wrong simulation results are considered. Due to the fact that the number of events not simulated and not observed (i.e. no-no combination in Table C1) is rather high, this second term will be always very small. Consequently, this term shows almost no effect on the PSS results which means that the wrong simulation results are not reflected. For example, Table 4 indicates a perfect score of 1 for floods for the Mackenzie but Table C1 also shows that three more flood events were simulated. Thus, the PSS might not be the right choice here.

p. 3484, line 5: What is meant by an unskilled forecasting system? This comparison is useless as a skilled system should be always better than an unskilled one.

p. 3484, line 20: reasonable to high skill is achieved within the framework of the analysis used (PSS). A high skill in being able to reproduce floods is somewhat overstated.

Comments chapter 5

The conclusions given in the paper are rather subjective and limited to the analysis and scoring systems applied. Simulated discharges of four out of 20 river basins are in reasonable agreement with the observations which means that 80% of the hydrographs look quite bad. This is completely neglected by the authors. However, it is recommended to improve the processes in the model instead of using a bias-correction that affects the water balance afterwards.

The skill to reproduce anomalies should be discussed in the light of using Q25 and Q75 whereas it is necessary to clarify the definition of “anomalies”. Because an anomaly is an unusual or unique occurrence, it is questionable whether the chosen Q25 and Q75 are representative. Instead, the authors should go for Q10 and Q90 (or Q5 and Q95) which would better fit the terminology.

The authors should avoid the comparison with an unskilled system that does not exist. The conclusion derived to apply the model for forecasting hydrological extremes seems to be overstated as this cannot be observed from the results given in the text and thus should be reconsidered. The ability to reproduce past hydrographs and extremes by other global hydrological models is well-known and published. Several studies on future projections of hydrological extremes are existent as well.

Comments on References

Lehner et al. 2006 is missing Please correct spelling for Vörösmarty et al. 2000

Comments on Tables and Figures

Table C1 and C1 (continued) contain the same contingency tables.

Figure 2: very small and scarcely be legible. Figure 3: same as for Figure 2. Other colours should be used for better distinction. Figure 4: why are different colours used? Not explained in the text or figure. Figure A1 is redundant as it should be clear that maximum daily discharges exceed maximum monthly discharges.

Please use a consistent spelling, either “McKenzie” or “Mackenzie”, in the figures and throughout the text.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 3469, 2011.



HESSD

8, C1994–C1998, 2011

Interactive Comment