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Hydrology and Earth System Sciences Discussions

Interactive comment on "Hydrological modelling for meso-scale catchments using globally available data" *by* A. Gafurov et al.

A. Güntner (Referee)

guentner@gfz-potsdam.de

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General comments:

The paper addresses an important aspect in hydrological modelling, i.e., the need to run hydrological models for larger river basins with coarse input data due to limited data availability. In particular, the only available data for many model applications at the regional scale are global-scale data sets that are freely available in the internet. With this topic, the study falls within the scope of HESS. It is basically a good approach taken by the authors when trying to assess the quality of model results that can be achieved with globally available data by comparing results from a river basin with good data coverage (the Neckar basin, Germany), and a data-scarce region (the Chirchik basin, Uzbekistan). However, the paper in its present form lacks of concision and

completeness in describing the scientific background and the applied methods and data. In my opinion, the presentation of results in many parts is not comprehensive and clear enough to support interpretation and conclusions. Besides the model application for the Chirchik basin, the study lacks of any novelty in terms of concepts, data or conclusions.

Specific comments:

1) The rigorous evaluation of model performance when using globally available data should include the comparison with model results that were obtained when running the model with the optimum data sets. In this study, this could be feasible for the Neckar basin where the model could be run with precipitation and temperature input data sets based on the existing dense station network. Only then it will be feasible to distinguish between deficiencies of model results that are specifically due to the low resolution of global input data sets and those that are inherent in the model structure. This may help to avoid interpretations that are too specific for the example model used in the study (HBV) and to reach more generally valid conclusions about the performance that can be expected with the global data sets.

2) The authors conclude that "the study ... gives useful results for water management and planning issues ..." (page 2222, line 19). Also in other places of the paper the authors claim that (e.g., page 2218, line 21: "Comparison of standard deviations also show that the results can be used for water management and planning purposes." In my opinion, these conclusions cannot be supported by sufficiently the presented results. There are mainly two concerns: 2a) Besides Table 1 with a comparison of mean and variability of simulated and observed (daily?) discharges at 5 gauging station, the discussion of results does not contain any further quantitative measures that allow assessing model performance. 2b) The question if model results are sufficient to support water management heavily depends on the type of intended management application. What do the authors have in mind? For instance, applying model results in the Neckar basin could be very critical and not useful for many management issues

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given the model deficiencies shown for the flow duration curve both in low flow periods and for flood events (Figure 9). For the Chirchik basin, for example, overestimating discharge simulations during the low flow season (Figs. 15, 16, 17) may prevent from management applications.

3) The implementation of a glacier module into the HBV is not an original development of this study, as the notion in the abstract and the description in the main text might indicate. For example, HBV-ETH (Braun & Renner, 1992; Braun et al., 2000) includes a glacier melt model based on the degree-day method that is very similar to the one proposed in this study.

4) In general, I see a mismatch in the manuscript between the (comparatively high) detail of presentation of the (well-known) HBV model, including three Figures with model structure (5, 10, 11), equations and model parameters (Chapter 4), and the (small) weight that is attributed to these model details in the presentation of results and in the discussion. As the focus of the paper should be the evaluation of global data sets in model applications rather than technical details of the specific model, I suggest to shorten these parts and, instead, trying to add a discussion on the validity of the results about the use of global data in conceptual models, independently from the specific model used here (see also my specific comment 1). (Apart from that, it could be mentioned in the model section what is specific about the HBV-IWS model in comparison to other HBV versions.)

5) I have major doubts on the validity of the precipitation-elevation relationship for the Chirchik basin as presented now (Fig. 14). I cannot see why the station data presented here indicate an increase of precip with elevation. There is practically no change with elevation visible (minimum precip, for instance, occurs at an elevation of about 1700m). The authors must add additional information on the precipitation station or from other sources to support their relationship (Do particular locations of the individual stations partly explain the irregular pattern with elevation (e.g. lee position)? What about the time periods for which observations are available for the individual stations?). Location

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of stations should also be included in Figure 2.

6) Page 2221, line 13: I do not agree with the assumption in the form given now that the model can produce a reliable daily flow duration curve if good results for the monthly flow duration curve are obtained. Streamflow variability at the daily scale may be considerable different from the monthly scale and not adequately represented by the model even if monthly means are reasonable. If sticking to their assumption, the authors should explain their reasoning more extensively.

7) In general, the authors give too less credit to existing work in the area of their study. Both in the introduction and when discussing their results, references should be made to studies that used a similar approach in hydrological modelling or that evaluated the quality of globally available data.

Additional specific remarks:

9) Abstract and Introduction: The authors state that the objective of the study was “to use globally available data for water balance modelling ...”. I think, however, that the really interesting objective is to evaluate the performance that can be achieved with a conceptual model given these input data. This is also actually examined in the paper. Also at the end of the Introduction chapter (page 2212, lines 1ff), the authors should clearly state the objectives instead of summarizing some of their results.

10) page 2210, line 26: what is meant by “regional discharges”?

11) page 2211, line 11: The authors mention some global data sets, actually those that haven been used in their study. As an introduction, one might give a somewhat broader overview on existing sources, and give reasons that lead to the selection of these data sets in particular.

12) equation 1, line 21: “Pnm was obtained by aggregating daily data to monthly taking the arithmetic mean” ? For precipitation, shouldn’t the monthly sum be taken ?

13) page 2214, line 1: “...some observations ... were available”. Please specify which

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data were available, Mention the stations, number of stations and length of data period, include the location of stations in Figure 1 and 2.

14) page 2214, line 9, Figures 3 and 4: Not clear. What is compared to what in these figures? Why is their some time shift between the red and blue line in Figure 3?

15) page 2214, lines 17-21: For what is the land use information used in model parameterization? How can it be stated in this context that its accuracy is acceptable? Please explain.

16) no reference is made in the text to Figures 6 and 7.

17) The manuscript contains several vague statements that have to be specified. For example: - page 2216, line 26: “based on almost universally available data” What means almost ? - page 2218, line 7:”...the model works well enough” Well enough for what? - page 2222, line 19:”...the model is relatively good...” relative to what?

18) page 2218, line 17: “The elevation alone differs more than 3 times between the basins”. This makes no sense.

19) Conclusions, page 2221, line 25: “...five main gauging stations with a large drainage area”. The various gauging stations have not been shown before, just a list in Table 1, which does not allow to say that a 1000 km^2 basin is large in this context.

20) Conclusions, page 2221, line 26: “In addition, model performance increase with the increase of drainage area.” Statement not confirmed by results in the main text.

21) Conclusions, page 2222, line 3:”The data from global databases was more successful in low-lying regions...” I can’t see where this has been shown in the data or results section. Possibly my misunderstanding is also due to an insufficient description of data, stations and their characteristics.

22) What is the sense of Figure 1 and 2? A simple presentation of a colourful DEM is not sufficient, add, for instance, location of precip, discharge, met stations, (sub-)basin

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boundaries, boundaries of grid cells of global data sets. Figure 2: explain Charvak.

23) Figure 3 and 4: Time period missing.

24) Figure 8: Time axis missing. Hydrograph lines are too dense to be able to evaluate model performance.

25) The line of presentation throughout the manuscript needs to be revised as it is in many places not well structured and makes it hard to read the paper. For example: - Page 2213, lines 11-13. Repetition - Page 2213, line 21: “each 1 km^2 grid cell” Why? It is explained only afterwards (page 2216) that a DEM with this resolution was created as the basic spatial reference. - Page 2214, end of data chapter: Information on DEM should be moved to the top before talking about variations of climate data with elevation and interpolation. - page 2217: line 5: “...each sub-catchment”: not mentioned before. Which sub-catchments? Only explained afterwards in lines 7ff. - page 2219, line 20-21: “...similar to the observations”. Which observations? The data are partly explained later at lines 29ff of this page only.

References:

Braun, L. N. & Renner, C. B. (1992): Applications of a conceptual runoff model in different physiographic regions of Switzerland. Hydrological Sciences Journal 73/3, 217-231.

Braun, L. N.; Weber, M. & Schulz, M. (2000): Consequences of climate change for runoff from Alpine regions. Annals of Glaciology 31, 19-25.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 2209, 2006.