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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.

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This manuscript contains an effort to justify the use of globally available climatic and geographical data as input for modelling the runoff regime of scarcely measured meso-scale basins. Through verification of the model concept and the validation of the input data on an intensely measured basin (the Neckar basin), the resulting predictive potential is transferred to a second relatively ungauged basin (the Chirchik basin). The authors apply some adaptations to the different hydrological circumstances to come up with a model performance that is reasonably in line with the few measured (monthly) discharge values.

General comments:

In my opinion, this manuscript is severely lacking scientific proof about the advantages of using global (coarsely scaled) data as an input for hydrological modelling in ungauged basins. Too many assumptions are given which are not justified whatsoever by the available data of both the Neckar and the Chirchik basin. Already, an extensive overview of weak assumptions and conclusions is given in the comments of Güntner, but I would like to specify the ones that make this manuscript not suited for publication in HESS.

1) An assumption that is totally unjustified concerns the general point that is made in the conclusions, about the practical use of global data. Because the model works well with the global data on one place with specific hydro-climatologic circumstances (where the model is generally built and tested on) does not mean that it works just as well in an area with totally different conditions and runoff behaviour. This fact is actually proven by the authors themselves, where they needed two significant changes, in the model structure (the glacier module) as well as in the input data (the rainfall lapse rate method based on elevation), to come to 'acceptable results' (see also point 2, 3 and 4). In addition to this, the second conclusion of the assumption that the model might also work for the Chirchik basin on daily basis is even further away from the truth. It is not to be expected that the model represents the daily runoff dynamics accurately under these different conditions. Moreover, possibly several different daily runoff regimes, produced by the model, can give equally good results on monthly basis (the equifinality principle). A big uncertainty that also plays a role in this and which has not been addressed is the uncertainty of measured monthly data. How is this discharge data measured, through a measurement once a week or even once a month and are these point measurements representative to produce a mean monthly discharge?

2) As Güntner already states, it is not proven that the model performs well with global data for the Neckar basin if no comparison is made with the performance of the model using locally measured input data. Table 1 gives an indication of the accuracy of the model, which seems to have significant differences in observed and simulated means

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and standard deviations of the five representative basins, with high level of failure in the Vaihingen-Enz basin. Also the interpretation of the results of the flow duration curve of Figure 9 is rather subjective, where it is mentioned that the model is representing low frequency of ‘the discharge as it was in its natural state’ (p. 2218, line 20). If so, this should have been compensated by a higher discharge during winter condition, to keep the water balance stable. However, also under high water level conditions the model is generally underestimating the discharge, which can only be explained by a bias error in the simulations. It should be a first step to prove that the additional information content of the global data is giving acceptable results in comparison with local data in order to use this data for a relatively ungauged basin.

3) The additional glacier module is likely to be essential for the low flow behaviour of the model (p. 2219, line 2-4), not to mention the possible impact on the higher flow. By just adding this module without any validation of the impact reduces the credibility of the remaining model structure. With the introduction of this module, it was assumed that the hydrological processes are better represented, but is it really true that the HBV-IWS model represents the hydrological processes accurately in these ‘unknown’ circumstances? The example of the assumption of the author that the upper Chirchik basin is all agricultural land already diminishes the representative potential of the model structure (p. 2214, line 18). Also the fact that the original snow module of the model is probably not capable of giving good results is a clear indication that the model might not be suited for this environment. In this case the word ‘probably’ is used because no effort has been made by the authors to verify this.

4) The use of a logarithmic relation in the lapse rate method to link precipitation to elevation is an assumption which suggests that this relation (visualized in Figure 14) was needed to achieve a better performance of the model. There is no fundamental reason whatsoever to plot a logarithmic line through these highly scattered points. Hence, in my point of view, the fact that the results improve when this tool is used does not prove that there is a valid relation between elevation and global precipitation data, as

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stated in page 2222 line 6-9, but it suggests that the global data is not suited as such for hydrological modelling in ungauged basins and a minimum of local data at different locations and altitudes is still needed to verify the globally estimated input data.

5) On page 2222, line 3, the statement that the model performed well for low-lying regions without precipitation correction is not proven by a visualization or a objective function value, whereas this could be an interesting result of the modelling exercise. In this case, the hydro-climatologic circumstances of the Neckar basin are possibly closer to the ones of these low-lying sub-basins of the Chirchik basin. Hence, choosing a model, which had been tested on similar conditions as in the Chirchik basin, would have given a better chance to perform well under data scarce conditions of this Chirchik basin, without the subjective adaptations that had to be made with the HBV-IWS model.

Considering all this, it is highly doubtful that global data can be used in this model structure to represent the runoff dynamics of ungauged basins and, in particular, to represent the monthly and daily runoff behaviour of the Chirchik basin. Since these two facts were the main objectives of the performed study, it is unexpected that the manuscript can be corrected in such a way that it can be accepted for publication, and I suggest the authors to reconsider their objectives and approach of this study.

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