

Interactive comment on “Diagnostic calibration of a hydrological model in an alpine area” by Z. He et al.

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Remarks:

I have read this manuscript and find in many aspects sound but in some aspects rather poor. I really think that this manuscript was already ready for HESSD in the form it has been submitted and will therefore request major revisions for it.

As the authors mentions, the ideas of separating the hydrograph in order to confine equifinality is focus of current research and different approaches have been proposed. This contribution aligns within these efforts and has some merits, since

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it is simple and, as also noticed by B. Schäfli in her review, potentially easy to be transferred.

I also appreciated the field effort that is declared here in order to validate the estimation of temperature gradients in the region.

Concentrating on the poor aspects, when I read in an abstract “dominant runoff processes” (DRP) I expect a paper dealing with DRP (e.g. Schmocker-Fackel et al., 2007; Uhlenbrook et al., 2004). What I find here is interesting, but in my opinion should be declared as dominant mechanism leading to water availability for runoff-generation. This being snow-melt, glacier-melt and (storm) rainfall (and combinations). The separation according to the “Date-Index” DI should then be maintained in order to discriminate the low-flow season, where runoff occurs by water release from the subsurface and this because deep percolation is occurring in the periods where DI is equal “1”. Here I am surprised, that among the four parameters selected for calibration there is none linked to the groundwater-flow. I think this is because the processes leading to groundwater recharge occurs outside the season where groundwater-flow governs runoff-generation.

It is also surprising, that while the author make efforts in order to separate the hydrograph in different sub-samples, they trust a single measure of agreement in order to evaluate the model performance and they make also the frequently made assumption that the Nash-Criterion (NS) is the universal measure for this (see also the comment of B. Schäfli in this respect). NS is dominated by the SM+GM+R period and as Schäfli and Gupta (2007) demonstrated the low reliability of NS as measure of agreement in areas with strong seasonality in the runoff hydrograph. In Viviroli et al. (2009) we were also thinking about how to consider different processes in the model calibration and we propose a step-wise calibration guided by multiple objective functions and by iterative (and sequential) pair-wise calibration of tuneable parameter

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selected under consideration of the process they are associated with (snow-melt, glacier-melt, infiltration, surface-runoff, interflow).

Some specific points (to complement the issues raised by the other reviewers):

1) Remove all the links to “runoff generation processes” and replace it with “source of water available for runoff generation”.

2) Table 5: Is this the magnitude of improvement you were expecting when designing this study? What if you take instead of NSE a Benchmark efficiency, where you compare the simulation against the seasonal runoff (Schäfli Gupta, 2007). This might be sufficient to lead your parameters to be right for the right reason (Kirchner, 2006).

3) 1262– 3,6: The visual inspection confirms your statement. Maybe you have some place on Figure 2 next to the legend to declare a measure of agreement for the red and dotted-red lines with respect to the black line.

4) 1272-1273: You declare that you reach good simulation results except for some large storm runoff events in summer. I inspected figure 10 and I have to admit, that I was not able to find any event characterized by rapidly rising and falling peak that was simulated with your model. Again, you speak of dominant runoff generation, but your model fails in simulating any situation linked with storm-runoff triggered by storm rainfall. I think that your current perceptual model of this area has some missing components that you should investigate. The hydrographs you simulate merely reacts to weather periods characterized by rising and sinking temperatures.

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Final considerations:

I think this manuscript has potential, but work is needed to make it more ripe. I think that the design of the experiment can be improved by selecting multiple measures of agreement. I think also that the model should demonstrate to be able to cope with storm runoff before declaring success of this experiment. I would be happy if the authors can do a big effort and submit revised version of this manuscript.

Best regards

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