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Hydrology and Earth System Sciences Discussions

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Interactive Comment

Interactive comment on "Extension of the Representative Elementary Watershed approach forcold regions: constitutive relationships and an application" *by* L. Mou et al.

L. Mou et al.

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We would like to thank the anonymous Referee #3 for his critical comments on our manuscript which drives us to improve the manuscript greatly.

The major concerns of the Referee could be summarized as follows: (1) The presentation of the equations, variables, and logic are not clear; (2) One year of validation is insufficient to show the prospects of the model; (3) The mathematic deriving procedure is over convoluted; (4) The rigorous test of a cold region hydrological model is not necessarily in simulating runoff only but at least the snow cover area and streamflow simultaneously.

To address these concerns, the authors made major improvements in the revised manuscript including:

1 *Extending the model calibration and validation periods*

The model calibration in our case study is carried out manually, and our purpose in this paper is to demonstrate the capability of a generalized cold regions REW-based model for long-term streamflow modeling in high mountainous cold regions. This is why we didn't care so much for the length of validation period in the orignal manuscript. Thanks for the Referee's comments, we realize the importance of the length of calibration and validation period. In the revised manuscript we extend the length of calibration and especially validation periods significantly, i.e., including the data of 1992 in the calibration period which is excluded in the original manuscript, and extending the validation period from one year (1995 only) to three years (1995-1997). The rationality we chose the data from 1990-1994 for calibration is that this period covers versatile climate conditions (dry and wet, warm and cold, see Table 2 in the revised manuscript) from which we could expect the calibrated model parameters with lest bias. It should be noted here that the additional two-year validation period includes a 50-year return period flood in 1996 (Aziguli *at al.*, 1999) which damaged the flow measuring facilities and altered the landscape and especially river hydraulic conditions to some extent. Although the model can capture the general pattern of the hydrograph in 1997 we did get the lower evaluation merits especially R^2 value (0.49) compared to 1995 (0.73), see Sect. 4.4 in the revised manuscript.

2 *Model sensitivity analysis is carried out to give more insights of model behavoir and hydrological processes*

The Referee suggests the authors to focus on the goal of multivariable simulation. We totally agree that the multivariable simulation can indeed provide some kind of useful method to diagnosing the model which will give people more insights to understand

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the model and more confidence to apply the model. However, this would require more efforts and more detailed field data such as snow course measurements. Moreover, our study area is dominated by glacier melting and so the multivariable simulation like runoff and snow water equivalent or snow cover area doesn't look like a good choice. Generally, it's difficult for us to do the multivariable in this initial stage of model development as well as due to the data availability although we do present all the important internal variables such as snow water equivalent, glacier and snow melting, soil water/ice content and soil temperature dynamics in the manuscript.

To give more insights of model behavior and hydrological processes in our study area, the model sensitivity analysis is carried out by using one-at-a-time perturbation approach in the revised manuscript (see Sect. 4.4). The sensitivity analysis results (see Table 7 in the revised manuscript) show that runoff simulations are highly sensitive to precipitation partitioning and spatial heterogeneity parameters which can be explained via the facts that the case study area has a small area (28.9km^2) and thus a short residence time and that glacier melting is dominant.

3 *Simplification of the mathematical part*

We acknowledge that we gave too much details on the mathematical reasoning especially in the review section (Sect. 2) of the original manuscript. In the revised manuscript, we greatly reduce the length of review section. The main balance equaitons for special zones in cold regions are given in Table 1 of the revised manuscript briefly. Also, we remove some trivial equation transformations and variable descriptions to make the logic more concise and fluent.

4 *More interpretation of model results*

The authors didn't give much insightful interpretation and description of model results in the original manuscript. In the revised manuscript, the discussion text of model results is greatly enriched. Some interesting conclusions are drawn. For example, we found that the infiltration capacity of the soil is dependent on, and has a memory

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of the antecedent energy content, as measured by the average temperature over the cold season, and therefore, the actual amount of snowmelt runoff in each year will be dependent, to first order, on the average air temperature over the warm season, and the average temperature over the antecedent cold season (see Sect. 4.3 and 4.4 in the revised manuscript).

5 *Correction of the typos and vague statements*

The corresponding author feel really sorry for the nonproficient English and not performing the rigorous proof reading which result in the vague statements and some typing errors. Some of these may lead to misunderstanding of our logic and probably bringing about the Referee's conclusion that the manuscript *"include contradictory (or no explanation whatsoever) of variable and parameter units, balance equations that do not balance, and mistakes in logic"*. Here are a list of vague statements and typos in the original manuscript, which are pointed out detailed by the Referee A. Gelfan, and revised in the new manuscript:

(1) The processes of the overland, subsurface, and channel flow are not described in the orignal manuscript and the relationships between these processes and the special processes of cold regions are not expressed clearly either. In the revised manuscript we add some sentences to make this point more clear, see Sect. 2 of the revised manuscript as well as the authors' reply to the comments of the Referee A. Gelfan.

(2) In Page 3634 of the original manuscript, *"... i.e., ice melting* (e_{lg}^{g}) *..."* should be *"... i.e., evaporation* (e_{lg}^{g}) *..."*.

(3) In Page 3635 of the original manuscript, *"... the equivalent depth of the n-zone ..."* should be expressed more clearly by *"... areal averaged depth of snow ..."*.

(4) Also in Page 3635 of the original manuscript, *"... the the last term on the r.h.s. is the combination of* e_{lg}^{n} *and* e_{ln}^{n}*."* should be *"... the combination of* e_{lg}^{n} *and* e_{ng}^{n}*"*.

(5) In page 3646 of the original manuscript, *Eq.* $e^{nT} = e_l^{nT} \times \omega^n + e_n^{nT} \times (1 - \omega^n)$ *should*

be $e^{nT} = e_l^{nT} \times \omega^n + e_n^{nT} \times \omega^n$.

(6) In Page 3647 of the original manuscript, *"... Eq. (36) ..."* should be *"... Eq. (37) ..."*.

References

[1] Aziguli, Qi, B., and Yu, W. M.: Analysis of the Urumqi River '960719' flood, Urban Roads Bridges & Flood Control, (1), 35–39, 1999.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 3627, 2007.

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