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Hydrology and Earth System Sciences Discussions

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 Referee A. Gelfan for his valuable comments on our manuscript, which will improve the quality of the paper greatly.

As pointed out by the Referee, it is true that the specific year (1992) is excluded rather arbitrarily from calibration period and the validation period is a little short (one year only). The sensitivity analysis should be carried out to give more insight on the model behavior and hydrological processes in the study area. Also, the corresponding author feels really sorry for not performing a careful proof reading and the typo in the original

manuscript. Major improvements have been made in the revised manuscript according to the Referee's comments.

Comment 1 *Areal averaged fluxes are calcuated by multiplying the corresponding point fluxes on the empirical coefficients. The suggested approach to coming up from the point scale to the watershed scale looks oversimplified for it is very difficult to obtain stable values of similiar coefficients as a result of calibration. The Referee suggest the authors to give some more consideration on this subject. For example, it would be perfect to show sensitivity of the spatial heterogeneity coefficients and other parameters of the model to changes in the number of years used for calibration.*

The authors totally agree that the "sub-grid" spatial heterogeneity of topography and other factors will impose huge influence on the areal averaged fluxes (e.g., solar radiation, turbulent exchange on the glacier and snow surfaces) and a single empirical coefficient might not be sufficient to account for such influence. Due to the complex nature of this sub-grid heterogeneity problem and the fact that our purpose in this paper is to demonstrate the capability of the generalized cold regions REW-based model for streamflow simulation as well as the small area ($28.9km^2$) of the study watershed, we here prefer to use some kind of simple method. Although, the Referee's advice to carry out some sensitivity analysis is absolutely helpful. In the revised manuscript, we adopt a relatively simple method, one-at-a-time perturbation approach, to carry out the sensitivity analysis for all the parameters subject to calibration, and the results do show the high sensitivity of within REW variability parameters on the runoff simulation (see Sect. 4.4 in the revised manuscript).

Comment 2 *Processes of the overland, subsurface and channel flow are not described by the presented model. It seems that there is an implicit assumption that the water excess, which is generated in the corresopnding zone, reaches the outlet of the watershed for the time step of simulations. If this is the case, then, in order to help a reader to validate this assumption, the Referee suggest to give an information on the time of concentration for the watershed.*

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Actually, the balance equations for the bare soil zone, vegetated zone, sub-stream network zone, and main channel reach are coupled with the special zones for cold regions including the snow covered zone, glacier covered zone and unstatured zone and hence the overland, subsurface and channel flow are indeed simulated in the model and in our case study. This point is not explicitly stated in the original manuscript and some sentences are added in Sect. 2 of the revised manuscript.

Comment 3 *Taking into account that only one year of the observations was used for the model validation, the Referee suggests mitigating the last sentence of the Abstract to "the obtained results show the prospects of the developed approach but do not confirm its applicability yet".*

We agree the Referee's opinion although we extend the validation period to 3 years. We revised the last sentence of the Abstract as follows:

The results of the 5-year calibration and 3-year validation analyses show that THModel can indeed simulate runoff processes in this glacier and snow-dominated catchment promisingly, which shows the prospects of the REW approach and the developed closure schemes for cold regions processes.

Comment 4 *Heat exchange between glacier and soil is omitted assuming water exchange (infiltration) between these zones is small. However, what is the reason for omitting the heat conduction? Please clarify the physical meaning of the heat exchange between the glacier (or the snow pack) and the sub-stream network which is not taken into account in the existing snow models.*

The depth of glacier is typically large. For example, the depth of east wing of No. 1 glacier in Urumqi River reaches up to 133m (He *et al.*, 2004). The glacier is, therefore, assumed to stand above not the unsaturated layer but permafrost layer directly. Also, the glacier area is usually small and the temperature is lower. In our case study area the glacier covers less than 20% although the glacier melting is dominating. This is the reason we omit both the water exchange and heat conductivity term between the

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glacier and unsaturated zone.

For the heat exchange term between glacier/snow pack and the sub-stream network, to our mind all mass exchanges must be accompanied by the heat exchanges, i.e., the convectional term. However, this term is rather small compared to conductive term, which is confirmed by our simulation in the case study (but not presented in our manuscript), and thus not taken into account by most existing snow models as pointed out by the Referee. Thanks for the Referee's reminding, we also omit this and other convectional terms in the revised manuscript.

Comment 5 *What's the equivalent depth of the n-zone. Is it the areal averaged depth of snow? Please clarify. The Referee suggests replacing "snow phase" by "solid phase". The last term of the Eq. (9) is written as the combination of the evaporation and melting terms. However, the melting rate is already included through the terms of runoff and infiltration in Eq. (9). Please verify.*

The equivalent depth of the n-zone, y^n, is actually the areal averaged depth of snow. The authors realize the former description would probably lead to some misunderstanding compared to some terms, e.g., snow water equivalent, and chang it to the description suggested by the Referee.

The snow phase is replaced by solid phase as suggested by the Referee in the revised manuscript.

About the combination term e_{lg}^n, the sentence 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"*. So it should mean the combination of evaporation and sublimation. Sorry for the typing error.

Comment 6 *The terms "y" and "omega" are under the derivative in Eqs. (2)-(4) and are not in Eqs. (5) and (6). The same is in Eqs. (7)-(9) in comparison with Eq. (10), etc. Please verify.*

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The equations are right. The difference between the mass balance equations and the heat balance equations are due to the mathematical derivation. See Eqs. (A15)–(A38) in Appendix A of Tian et al. (2006) for detail.

Comment 7 *Table 1 constains some constants from the other papers which are not shown in the Nomenclature Section. The Referee suggests to remove the Table especially because it does not give any information which is necessary for better understanding of the specific subject of the paper.*

Thanks for the Referee's comment, we remove the Table 1 from the revised manuscript because it does NOT give more information for better understanding the subject of current manuscript.

Comment 10 *The physical meaning of Eq. (35) is not clear. Does it mean that rain falls on the snow covered area, whereas snow falls on the snow-free area only?*

Sorry for the typo. The original Eq. $e^{nT} = e_l^{nT} \times \omega^n + e_n^{nT} \times (1 - \omega^n)$ should be $e^{nT} = e_l^{nT} \times \omega^n + e_n^{nT} \times \omega^n$.

Comment 12 *It doesn't look reasonable to exclude the data of 1992 from the calibration period, especially taking into account the scarcity of the available data. The Referee suggests using the data of 1992 (maybe, without some inadequate days) for the model calibration.*

Thanks for the Referee's comment, we include the data of 1992 in the model calibration in the revised manuscript. Also, we extend the validation period from one year (1995) to three years (1995-1997).

Comment 13 *In order to assist in enhancing the readability of the Table 5, the Referee suggests to add the numbers of Eqs., where the corresponding parameters are used. Also, where are some calibrated parameters in the table, e.g., the coefficient from Eq. (37).*

We add the Eqs. numbers to the Table 5 in the revised manuscript according to the

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Referee's comment. The coefficient c_{INF} from Eq. (37) is not subject to the calibration and set to a constant, i.e. 1.0, in our case study and is, therefore, not included in the calibrated parameters table (Table 5). Some explaination sentences are added in the revised manuscript, see Eq. (19) in Sect. 3.2.

Other comments, concerning spelling and style were taken into account in the revised manuscript.

References

[1] He, M., Sun, B., Yang, Y., and Jiao, K.: Ice thickness determination and analysis of No.1 Glacier at the source of Urumchi River, Tianshan by ground penetrating radar, J. East China Insti. Tech., 27, 235-239, 2004.

[2] Tian, F., Hu, H., Lei, Z., and Sivapalan, M.: Extension of the representative elementary watershed approach for cold regions via explicit treatment of energy related processes, Hydrol. Earth Syst. Sci., 10, 619–644, 2006a.

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