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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.

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The paper presents an extension of representative elementary watershed (REW) approach (Reggiani et al., 1998, 1999) for cold regions by proposition of the set of closure schemes for the cold region hydrological processes. Using simplified assumptions, the authors developed parsimonious parameterizations for the melting and accumulation of glacier and snow (based on the heat balance and the degree-day methods, respectively) as well as for the freezing and thawing of soil (based on the "maximum unfrozen-water content" approach). The proposed closure schemes were implemented

within THModel (Tian, 2006) and applied to describe hydrological processes at the small catchment (Urumqi River, China) where snow and glacier processes are dominating. The model was calibrated and validated using 5 years of observations (4 years for calibration and 1 year for validation) at the catchment under consideration and the promising results were obtained.

This is an exploratory study developing the idea to create (on the basis of the REW conception) an approach which could be alternative for the current generation of physically based, distributed hydrological models. The paper contains interesting ideas which could make a useful contribution to the exciting problem of the watershed modeling, and, though I hold more optimistic viewpoint on the potential of the existing physically based models in the solution of this problem, I definitely recommend the paper for publication in HESS.

There are some comments I suggest that the authors take into account.

1. Areal averaged fluxes (solar radiation, turbulent exchange on the glacier and snow surfaces) are calculated in the paper by multiplying the corresponding point fluxes (derived from the point meteorological data) on the empirical coefficients (see Eqs. 16; 36 as well as explanations after the Eq. 30). Some of the coefficients are tuned through the calibration; others are assigned from the literature. The suggested approach to coming up from the point scale to the watershed scale looks oversimplified; from my own experience it is very difficult (if ever possible) to obtain stable values of similar coefficients as a result of the calibration. Maybe it is not the case for the presented model or due to the specific conditions of the Urumqi basin, however I suggest the authors to give more consideration on this subject. For example, it would be perfect to show sensitivity of the both coefficients mentioned above and other parameters of the model to changes in the number of years used for the calibration.

2. Processes of the overland, subsurface and channel flow are not described by the presented model. On my understanding, there is an implicit assumption that the water

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excess, which is generated in the corresponding 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, I suggest to give an information on the time of concentration for the watershed.

3. Taking into account that only one year of the observations was used for the model validation, I suggest mitigating the last sentence of the Abstract. In my opinion, the obtained results show the prospects of the developed approach but do not confirm its applicability yet.

4. P. 3634

"...i.e. ice melting (e_sup_g_sub_lg)..." should read "...i.e. evaporation (e_sup_g_sub_lg)..."

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 snowpack) and the sub-stream network. To my knowledge, this term is not taken into account in the existing snow models.

5. P. 3635:

What is "the equivalent depth of the n-zone". Is it the areal averaged depth of snow? Please clarify

I suggest to replace "snow phase" by "solid phase"

The last term of the mass balance equation (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.

6. The terms "y" and "omega" are under the derivative in Eqs. (2)-(4) and are not in

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Eqs. (5), (6). The same is in Eqs. (7)-(9) in comparison with Eq. (10), etc. Please verify.

7. P. 3637: Table 1 contains some constants from the other papers which are not shown in the Nomenclature Section. I suggest 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. However if the authors prefer to keep the Table, I suggest to add the corresponding constants to the Nomenclature.

8. P. 3640: The coefficient of unit transformation is not necessary in Eq. (17) because this equation does not contain any numerical coefficients.

9. P.3644: The heat content of the g-zone is missed in the melting rate equation (31).

10. P. 3646: To me, 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?

11. P. 3637:

(under Eq.(38)): "Equation (36)" should read "Equation (37)"

Eq. (37) is not the same as one suggested in the paper (Zhao, Gray, 1997). Please verify.

12. P.3653: 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. I suggest using the data of 1992 (maybe, without some inadequate days) for the model calibration.

13. P.3656. "The modeling shows" The sentence is not completed

14. Table 5: In order to assist in enhancing the readability of the table, I suggest to add the numbers of Eqs., where the corresponding parameters are used. Also, I could not find some calibrated parameters in the table, e.g. the coefficient from Eq. (37)

15. References in the text are not fully corresponding with the reference list.

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Concluding Remarks

1. Does the paper address relevant scientific questions within the scope of HESS? YES 2. Does the paper present novel concepts, ideas, tools or data? YES 3. Are substantial conclusions reached? YES 4. Are the scientific methods and assumptions valid and clearly outlined? YES 5. Are the results sufficient to support the interpretations and conclusions? NOT COMPLETELY (see comment #3) 6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientific (traceability of results)? YES 7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? YES 8. Does the title clearly reflect the contents of the paper? YES 9. Does the abstract provide a concise and complete summary? YES 10. Is the overall presentation well structured and clear? YES 11. Is the language fluent and precise? YES 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? NOT COMPLETELY 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? YES 14. Are the number and quality of references appropriate? YES 15. Is the amount and quality of supplementary material appropriate? YES

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Interactive Comment