

Interactive comment on “Frozen soil parameterization in a distributed biosphere hydrological model” by L. Wang et al.

Anonymous Referee #3

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General comments

The paper presents the inclusion of a simple frozen soil parameterization scheme for the spatially distributed hydrological model WEB-DHM. The validation in a high mountain watershed in China both for the surface water and energy budgets at the plot scale and for the runoff is considered. The subject is appropriate for HESS and the paper addresses a relevant topic, since frozen soil parameterization is often inadequately represented in hydrological models. The paper is quite well written and organized.

However, in my opinion, the paper needs major improvements in the description of the frozen soil parameterization and in the part where results are discussed.

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While the new frozen soil parameterization seems to only marginally improve the model performance in simulating the plot scale soil temperature and soil moisture dynamics, the model shows satisfactory improvements in the simulation of the catchment scale runoff. The reasons of those differences should be better discussed. The model does not correctly predict the melting time of the entire soil column. This might be related to some shortcomings of the modified force-restore method used in the model to compute the soil temperature or to an oversimplified snow melt scheme.

Moreover the advantages (simple and fast) and shortcomings (several empirical approaches are used) of the implemented frozen soil parameterization with respect to different solutions available in the literature should be better discussed. This will improve the impact of the paper and help a user of the model to choose the right frozen soil parameterization depending on his/her purpose.

Therefore, I recommend the publication after a major revision.

Specific comments

1 Introduction

More recent literature can be cited, see also comments of reviewer #2.

2 Model description

2.1 Surface Radiation Budget

This paragraph is not so informative. I suggest either to describe more in detail the radiation parameterization or to skip it.

2.2 Treatments of snow

P 6899 line 15. The assumption that $T_{\text{soil}}=T_{\text{snow}}$ is a quite strong assumption, even if it makes sense in a one layer model. Please comment this point.

2.2 Frozen soil parametrization

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P 6900 line 15. The unfrozen water content ($\theta_{liq,j}$) is assumed as a simple power function of soil temperature . . .

Is this empirical approach, combined with the force-restore method, energy conservative?

P 6901 equation 11. f_{ice} . . . This equation is one of the key factors that effect the simulated runoff, as shown later in the paper. Is this equation new? Please cite the source of this equation.

2.3.2 Soil thermal properties

P 6902-6903. This part is the physical basis for the study and needs to be further clarified and extended.

- How is T_d calculated?
- How is d_s , the effective depth that feels the diurnal change of temperature, calculated?
- The depth of seasonal frost penetration is an important parameter, but it does not appear in any equation. Please provide an equation or more details on its use in the frozen soil scheme.
- Assuming that at 5 cm below the surface the diurnal change can be supposed as a perfect periodic relationship with time is a strong hypothesis. Please justify it.
- *P 6903, line 10.* The force-restore method calculates the time evolution of T_d and T_g . How are those temperatures used then to calculate the frost/thaw depth and how are T_d and T_g related with the deep soil and root soil zone temperatures? Please provide more details on this part, providing the reader with the basic information to follow the approach.

4.1 Model calibration

P 6905, line 20 Please move the first introductory lines before the paragraph parame-

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ters and split them into “land surface parameters” and “soil hydraulic parameters”. In this way the text organization is clearer.

P 6906, line 5 It is not clear how parameters as root depth and top soil depth can be optimized using only soil temperature observations. Are such depths the same as the ones assumed for the two layers of the force-restore method or are they different?

P 6906, line 13 “long term”. I would prefer “year-long”. “Long term” sounds as referred to a record of many years.

4.1.2 Calibration results

P 6907, line 13 “*Rlu was estimated from the observed surface soil temperature at 5 cm*”. How was Rlu estimated? Soil surface temperature can be very different from soil temperature at 5 cm.

P 6907, line 15 Is there permafrost in the DY station? Is deep soil still frozen in July?

P 6907, line 15 “soil temperature at surface layer, root zone and deep soil were all well reproduced by the calibrated model”. Are the results obtained with the model with or without frozen soil scheme?

P 6908, stream flow calibration. There is no comment on the degree of variability of soil properties in the catchment. Which assumption has been made? Is the station used for point scale calibration representative of the entire catchment?

Figure 4 The model seems to under-estimate the diurnal variation at the surface, while it shows some diurnal oscillation at the deep soil level. This is also a problem observed in Figure 7. Besides soil thermal parameters, have ground heat flux and canopy fraction been checked? Usually the diurnal amplitude is very sensitive towards such factors.

Figure 5e-g Why does the model show a marked diurnal soil moisture oscillation at the deeper layers, whereas observations do not? Is this related to freezing–thawing cycles or to root water extraction?

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Is the strong variation observed in layer 80 cm related to the frost depth change?

4.2 Model validation

4.2.1 Soil temperature at the DY station from 21 November 2007 to 20 November 2008

P 6908 and Figure 7

It seems that the model substantially misses the observed soil temperature dynamic during summer and especially in autumn (see also Reviewer #2 comments).

I suggest to better parameterize/calibrate the snow-melt module of the model, as well as the canopy fraction.

4.2.2 Soil moisture at the DY station from 21 November 2007 to 20 November 2008

P 6909 and Figure 8a-d

It seems that important processes are missed by the model, i.e. looking at 5 and 10 cm soil moisture observations it seems that the snow melt timing is missing. The snow melt in the model should be improved.

Figure 8a-d

Soil thawing time seems to be completely missed by the model. Since frost depth dynamic modeling is one of the key features of the Li an Koike (2003) model implemented here, the absence of the thawing front dynamic seems quite unsatisfactory.

Why does the model without frozen soil perform better at 120 cm depth? Is there permafrost there?

4.2.3 Discharges at the Binggou gauge from 17 January to 20 November 2008

Figure 10

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In my opinion, showing how the model overestimates runoff with a constant K_g is a very nice and clean result. Since this is the most original part of the paper it can be better underlined.

5 Concluding remarks

I would not say that the model reproduces soil moisture “much better”, but only “slightly better”.

My impression is that such a simple frozen soil scheme is fine to improve the capability of the model to capture the basin averaged runoff production, but it does not give very satisfactory results in reproducing the point scale frozen soil dynamics.

Technical corrections

Table 1. It is not so relevant from my point of view and it can be skipped.

P 6907, line 10 “measured” measured

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 6895, 2009.

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