

Response to referee #3's comments

Major revisions:

- 1) In Section 2.1, a brief review of the “old” WEB-DHM has been given, including the general model structure and the soil model.
- 2) In Section 2.2, the frozen soil parameterization (particularly the soil thermal properties) has been better formulated and clarified.
- 3) In Section 3, the datasets descriptions have been improved.
- 4) In Section 4.1, all the parameters have been better optimized.

First, by using the original WEB-DHM without the frozen scheme, the land surface parameters and two van Genuchten parameters were optimized using the observed surface radiation fluxes and the soil moistures at upper layers (5, 10 and 20 cm depths) at the DY station in July. Second, by using the WEB-DHM with the frozen scheme, the frozen soil parameters were calibrated using the observed soil temperature at 5 cm depth at the DY station from 21 November 2007 to 20 April 2008; while the other soil hydraulic parameters were optimized by the calibration of the discharges at the basin outlet in July and August that covers the annual largest flood peak in 2008.

- 5) The Figure 2 (“soil model of WEB-DHM”) is added. The new calibration and validation results have been used to make/ update Figures 4-12.
- 6) The advantages and shortcomings of the implemented frozen soil parameterization with respect to different solutions available in the literature have been discussed in the conclusion part ([Lines 476-494 in the revised manuscript](#)).

Responses to general comments:

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. 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.

Answer:

According to the reviewer 2's comments, two references ([Bonan et al. 1996](#); [Poutou et al. 2004](#)) (Lines 44-45 in the revised manuscript) have been added in the review of the frozen soil parameterization in land surface modeling.

Bonan, G.: A land surface model (LSM version 1.0) for ecological, hydrological, and atmospheric studies: technical description and user's guide, NCAR Technical Note 417, NCAR, Boulder Co., USA, 1996.

*Poutou, E., Krinner, G., Genthon, C., and de Noblet-Duconré, N.: Role of soil freezing in future boreal climate change, *Clim. Dyn.*, 23, 621-639, 2004.*

According to the reviewer 1's comments, another two references ([Stocker-Mittaz et al., 2002](#); [Ye et al., 2009](#)) (Lines 49-50 in the revised manuscript) have been added in the review of cold region hydrology modeling.

*Stocker-Mittaz, C., Hoelzle, M., and Haeberli, W.: Permafrost distribution modeling based on energy-balance data: a first step, *Permafrost Periglacial Processes*, 13, 271-282, 2002.*

*Ye, B. S., Yang, D. Q., Zhang, Z. L. and Kane, D.: Variation of hydrological regime with permafrost coverage over Lena Basin in Siberia, *J. Geophys. Res.*, 114, D07102, doi:10.1029/2008JD010537, 2009.*

(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.

Answer: This part has been removed.

(3) 2.2 Treatments of snow

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

Answer: The assumption follows the Equation 32 in Sellers et al. (1996a).

Sellers, P. J., Randall, D. A., Collatz, G. J., Berry, J. A., Field, C. B., Dazlich, D. A., Zhang, C., Collelo, G. D., Bounoua, L.: A revised land surface parameterization (SiB2) for atmospheric GCMs, Part I: Model Formulation, J. Climate, 9, 676-705, 1996a.

In the revised manuscript, descriptions about “treatments of snow” (including the method by Yamazaki (2001)) are also removed, because that the introduction of the Yamazaki method changes little on the simulated results (snow depth, soil temperature and moisture profiles, and river discharge), and the other snow descriptions can be found in Sellers et al. (1996a).

(4) 2.3 Frozen soil parametrization

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?

Answer: Yes, it is energy conservative.

Because the force-restore model (Deardorff, 1977) of the heat balance in the soil surface and deep soil is kept as the same. Only the effective heat capacities for soil surface and snow-free soil (C_g and C_d), are modified to account for the latent heat of fusion or the change of soil thermal conductivity (see Lines 161-194 in the revised manuscript):

$$C_g \frac{\partial T_g}{\partial t} = Rn_g - H_g - \lambda E_g - \frac{2\pi C_d}{\tau_d} (T_g - T_d) - \xi_{gs} \quad (8)$$

$$C_d \frac{\partial T_d}{\partial t} = \frac{1}{2(365\pi)^{1/2}} (Rn_g - H_g - \lambda E_g) \quad (9)$$

Deardorff, J. W.: *Efficient prediction of ground surface temperature and moisture, with inclusion of a layer of vegetation*, *J. Geophys. Res.*, 83, 1889-1903, 1977.

(5) P 6901 equation 11. f ice. . . This equation is one of the key factors that affect the simulated runoff, as shown later in the paper. Is this equation new? Please cite the source of this equation.

Answer: It is a new equation developed in the study.

(6) 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.

Answer:

T_d is solved by the force-restore method, while the effective heat capacity for snow-free soil C_d has been modified to consider the frozen soil effect (Lines 173-182 in the revised manuscript).

d_s is set as 0.6 m, according to the measured soil temperature fluctuations in winter at the DY station (Table 1 in the revised manuscript).

The descriptions of frost and thaw depths (ζ_f and ζ_t) have been added (Lines 195-214 in the revised manuscript).

In the revised manuscript, a new and simple method is used to estimate soil temperature at 5 cm with $T_{soil,D_1} = \eta T_g + (1 - \eta) T_d$. (Lines 198-200 in the revised manuscript)

(7) - 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.

Answer:

The descriptions of frost and thaw depths (ζ_f and ζ_t) have been added (Lines 195-214 in the revised manuscript). After determining the position of freezing front, the sublayer soil temperature in the root zone and deep soil are estimated by a simple function of frost depth. Therefore, the mean temperatures at root zone (5~25 cm) and deep soil (25~125 cm) can be averaged from the soil temperatures at different depths. In order to reduce ambiguity, within the revised manuscript, these temperatures have been replaced with the soil temperature at 5 cm and the deep soil temperature (T_{soil,D_1} and T_d).

(8) 4.1 Model calibration

P 6905, line 20 Please move the first introductory lines before the paragraph parameters and split them into “land surface parameters” and “soil hydraulic parameters”. In this way the text organization is clearer.

Answer: In the revised manuscript, the new calibrations have been done.

First, by using the original WEB-DHM without the frozen scheme, the land surface parameters and two van Genuchten parameters were optimized using the observed surface radiation fluxes and the soil moisture profile at the DY station in July. Second, by using the WEB-DHM with the frozen scheme, two frozen soil parameters were calibrated using the observed soil temperature at 5 cm at the DY station from 21 November 2007 to 20 April 2008; while the other soil hydraulic parameters were optimized by the calibration of the discharges at the basin outlet in July and August that covers the annual largest flood peak in 2008. With these calibrated parameters, the WEB-DHM with the frozen scheme was then used for a yearlong validation from 21 November 2007 to 20 November 2008.

Therefore, the first introductory lines have been moved to the beginning of Section 4.1 according to your suggestion; while the paragraph “parameters” is split into two parts: (1) “4.1.1 Parameters optimized through the WEB-DHM without the frozen scheme”; (2) “4.1.2 Parameters optimized through the WEB-DHM with the frozen scheme”.

(9) 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?

Answer: Sorry for the mistake. The root depth ($D_r = D_1 + D_2$) and top soil depth ($D_s = D_1 + D_2 + D_3$) are defined according to the field observations (Li et al., 2009).

As shown in Figure 2 in the revised manuscript, D1 is the surface soil layer (0~5 cm); D2 is the root zone (5~25 cm in the study); and D3 is the deep soil (25~125 cm in the study). To my understanding, the force-restore method only considers the soil temperatures at soil surface (T_g) and deep soil (T_d), and does not explicitly use soil depths.

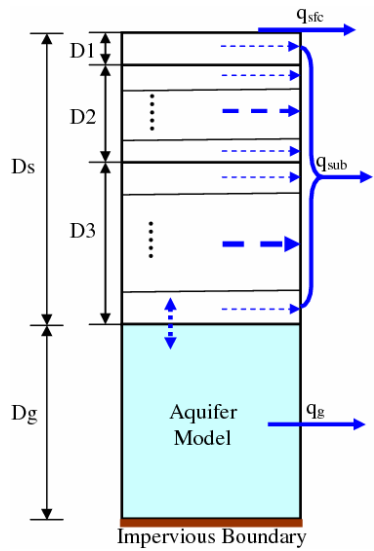


Figure 2. Soil model of the WEB-DHM.

Li, X., Li, X. W., Li, Z. Y., Ma, M. G., Wang, J., Xiao, Q., Liu, Q., Che, T., Chen, E. X., Yan, G. J., Hu, Z. Y., Zhang, L. X., Chu, R. Z., Su, P. X., Liu, Q. H., Liu, S. M., Wang, J. D., Niu, Z., Chen, Y., Jin, R., Wang, W. Z., Ran, Y. H., Xin, X. Z. and Ren, H. Z.: Watershed Allied Telemetry Experimental Research, *J. Geophys. Res.*, doi:10.1029/2008JD011590, 2009.

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

Answer: Revised. (Line 321 in the revised manuscript)

(11) 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.

Answer: Sorry for the mistake. I did this comparison since we have not monitored the soil surface temperature in the DY station.

In the revised manuscript, I have deleted the comparison of radiations except for the shortwave radiation, and rewritten the relevant text in the revised manuscript (Lines 359-362) as: “The *BIAS* and *RMSE* for the simulated upward shortwave radiation at the DY station are -3.8 W m^{-2} and 32.6 W m^{-2} , respectively. It should be mentioned that the measurements of upward longwave radiation in the station were found erroneous for all periods, and was not used for model evaluation in the study.”

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

Answer: In the Binggou watershed, the permafrost distributes at the region higher than 4000 m, with the air temperature lower than 0 °C in 9 months (September to next May) (Yang et al., 1993). The elevation of the DY station is 4146.8m. As a result, there is permafrost in the DY station.

From the measured soil temperature profiles in July at the station, it is found that the soil temperature is below 0 °C stably at the depth of 120 cm. Therefore, in the DY station the deep soil is still frozen in July.

Yang, Z., Yang, Z., Liang, F., and Wang, Q.: Permafrost hydrological processes in Binggou Basin of Qilian Mountains, Journal of Glaciology and Geocryology, 15(2), 235-241, 1993. [In Chinese]

(13) 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?

Answer: In the old manuscript, the results were obtained using the WEB-DHM with the frozen scheme. In the revised manuscript, this part is removed.

(14) 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?

Answer: There is only one soil type (FAO, 2003) in the small Binggou catchment, and thus no spatial variability of soil properties is considered in the study. The two van Genuchten parameters (α and n) calibrated for the DY station is then used for the entire catchment.

FAO: Digital soil map of the world and derived soil properties, Land and Water Digital Media Series Rev. 1, United Nations Food and Agriculture Organization, CD-ROM, 2003.

(15) 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.

Answer: The Figure 4 in the old manuscript has been removed. Because that in the new manuscript, the new calibrations in summer are performed with the original WEB-DHM.

In the revised manuscript, the soil temperatures are generally well reproduced using the WEB-DHM with the frozen scheme (Figure 8):

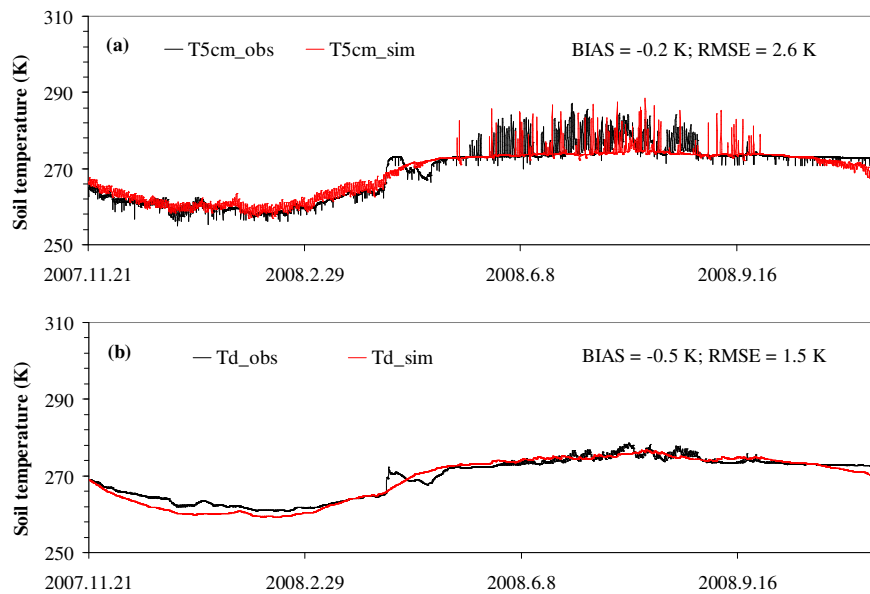


Figure 8. Hourly observed and simulated temperature at 5 cm T_{5cm} (a) and temperature of deep soil T_d (b) at the DY station, from 21 November 2007 to 20 November 2008 by using the WEB-DHM with the frozen scheme.

(16) 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?

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

Answer: Yes, the strong soil moisture variation observed in layer 80 cm is caused by the thawing process.

The marked diurnal soil moisture oscillation at the deeper layers is attributed to the wrong parameterization of soil temperature profile in the old manuscript. In the revised manuscript, the parameter calibration in summer is done for the original WEB-DHM (Figure 5 in the revised version). The simulated soil moisture does not show a diurnal signal in Figures 5e-5g. Due to a lack of frozen soil physics, the thawing process at deeper layers (around 80 cm) is missed by the original WEB-DHM. It also reveals that it is necessary to incorporate a frozen soil scheme.

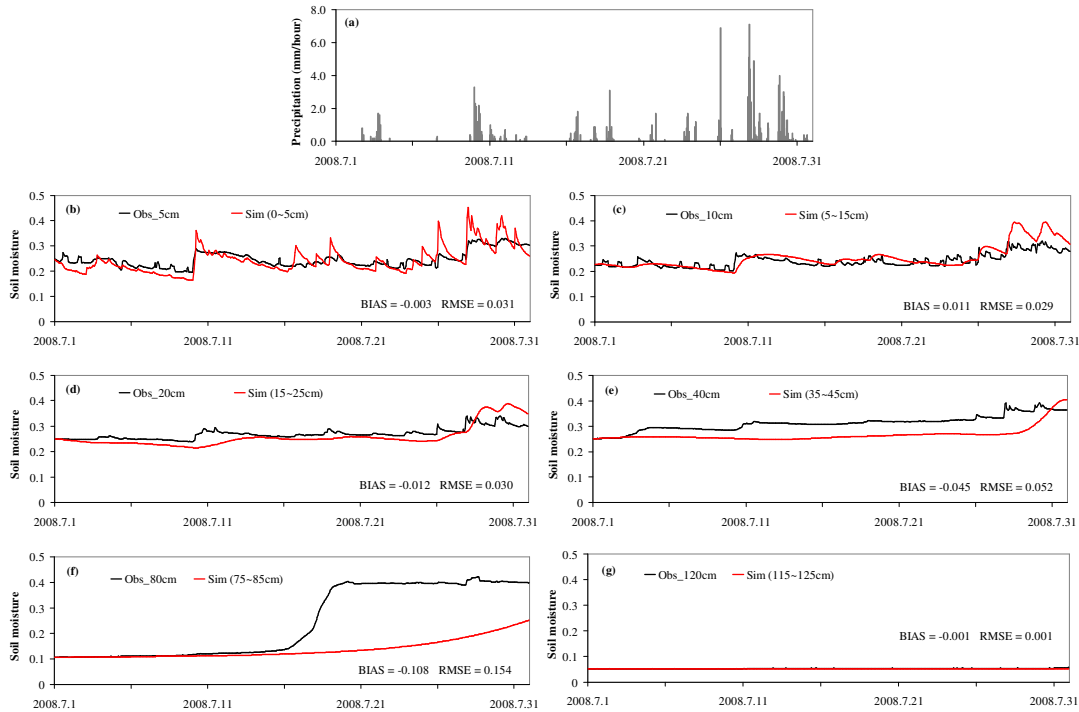


Figure 5. Hourly precipitation (a), and the simulated and observed hourly volumetric liquid soil moisture at 5, 10, 20, 40, 80, and 120 cm (b-g) at the DY station in July 2008, by using the WEB-DHM without the frozen scheme.

(17) 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.

Answer: In the revised version, Figures 8 and 9 have shown that the WEB-DHM with the frozen scheme can reproduce the soil temperatures and snow depth with acceptable accuracies.

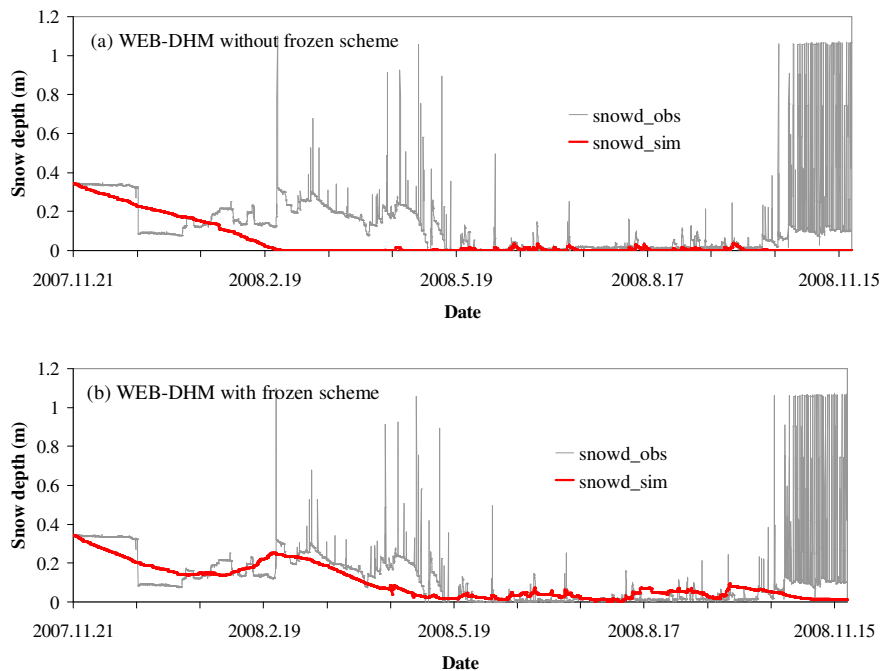


Figure 9. Hourly snow depth at the DY station from 21 November 2007 to 20 November 2008, simulated by the WEB-DHM without and with the frozen scheme. Here, the snow depth is assumed as five times of the snow-water equivalent, and the large fluctuations of the observed snow depth at the station were caused by strong wind blowing.

(18) 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 snowmelt in the model should be improved.

Answer: In the revised manuscript, the WEB-DHM with the frozen scheme generally reproduces the yearlong soil moisture well, with relatively accurate snowmelt timing (Figure 10).

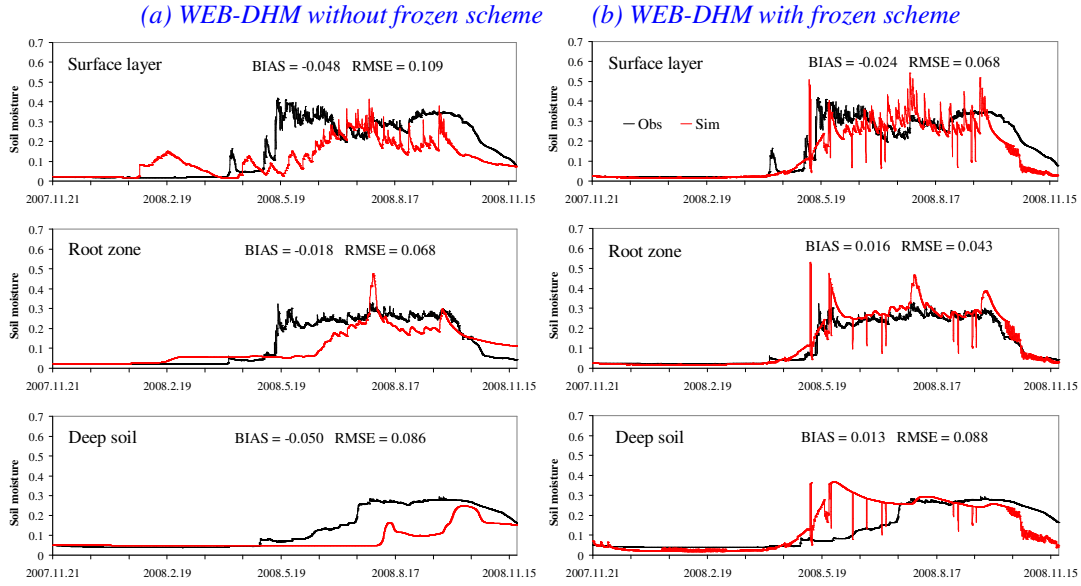


Figure 10. Hourly volumetric liquid soil moisture averaged at surface layer (0-5 cm), root zone (5-25 cm), and deep soil layer (25-125 cm) at the DY station from 21 November 2007 to 20 November 2008, simulated by the WEB-DHM without and with the frozen scheme.

(19) 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 and 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?

Answer:

There is permafrost layer at 120 cm depth at the DY station (with an elevation of 4146.8m).

In the revised manuscript, the WEB-DHM with the frozen scheme generally performs well in simulating the thawing front (Figure 7), and the soil moisture profile (Figure 10).

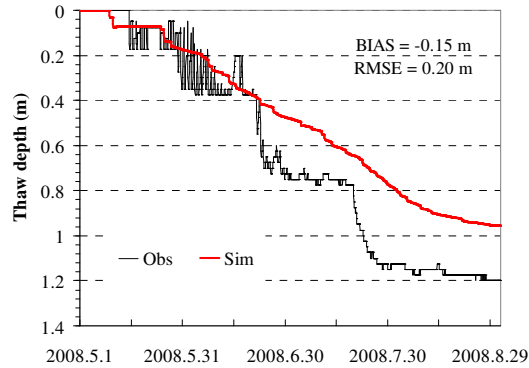


Figure 7. Hourly observed (interpolated from the observations of soil temperature profile) and simulated thaw depth at the DY station, from 1 May to 31 August 2008, by using the WEB-DHM with the frozen scheme.

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

Figure 10

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.

Answer: Thank you for the comments.

(21) 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.

Answer:

In the revised manuscript, the WEB-DHM with the frozen scheme does reproduce soil moisture much better than the original WEB-DHM (Figure 10). Furthermore, Figures 7-10 show that the WEB-DHM with the frozen scheme generally gives satisfactory results in reproducing the point scale frozen soil dynamics, besides the basin averaged runoff production (Figures 11 and 12).

Technical corrections

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

Answer: Removed.

(23) P 6907, line 10 “meansured” measured

Answer: In the new manuscript, this sentence is removed.