

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

**Anonymous Referee #2**

Received and published: 18 December 2009

### Summary

The study describes a spatially distributed hydrological model (WEB-DHM) where a new algorithm has been implemented describing soil freezing. This process is essential since it affects the infiltration, storage and routing of water. Modeling results (runoff, soil temperatures, moisture, radiation components) are compared for one year to measured parameters in a high mountain watershed in the Heihe River Basin, China. The paper surely warrants further consideration since it addresses an important research question and includes an interesting data set. However, I encourage re submission only after major revision due to the following reasons: (i) the paper fails to show “a better performance” (as stated by the authors) compared to the original model without

C2943

a frozen scheme. (ii) no rigorous model sensitivity test is provided (iii) a large set of optimization parameters is used with no physical basis which prevents using the model in future applications.

### General model (description) comments

-Why is section 2.1 (surface radiation budget) and 2.2. Treatments of snow outlined in detail, but other model description just based on citing other papers (for example, Sellers et al. (1996) for formulation of land surface processes and Wang et al. (2009) for later flow, river routing)? Is section 2.2. (Treatments of snow) a new model addition and why this method by Yamazaki (2001) is chosen? My suggestion is to give a brief overview of parameters and processes of the “old” WEB-DHM, including land cover, lateral and vertical flow in frozen and unfrozen soil, and distribution of fluxes in mountain terrain.

-The authors use a simple empirical function to describe the freezing soil characteristics (formulas (7, 8)), but do not provide any material properties or measured data (for example, soil freezing characteristic curves).

### Detailed comments

Page 6899, line 16: What is  $T_g$ ? Page 6900, line 1- this sentence is unclear- should this describe the infiltration into frozen soil? Page 6900, section 2.3. Why is this approach chosen and not for example the approaches that have been published by others (Woo et al. 2004; Poutou et al. 2004; Bonan et al. 1996). Page 6901, line 5: How are the empirical parameters in van Genuchten’s determined? How is  $f$  (hydraulic conductivity decay factor, line 15) determined? Page 6901, line 10:  $K_{sat,j}$  is represented using the assumption of an exponential decrease in hydraulic conductivity with increasing soil depth. Is this assumption valid for this landscape? For example, in some periglacial settings the opposite is true. Please provide details on soil material properties. Page 6902, line 6: How is  $R_{ng}$  different to  $R_n$  in formula (1)? Page 6902, lines 5& 12: the physical basis and derivation from formula (14) to (15) are not clear.

C2944

Page 6903. Datasets for the study area. This section needs reorganization; cut out the section of general hydrological processes (lines 1-15) and move it to results. A new organization should include: description of study site (physical setting topography (altitudinal extent), surface characteristics (vegetation), soil/bedrock material, climate parameters such as annual air temperature, precipitation, snow water equivalent, permafrost/seasonally frozen ground distribution ..) Line 25: Lower limit of permafrost coverage is about 3400 m- is this within the watershed? Please provide details on permafrost coverage, depth and thermal state and/or seasonally frozen soil. Page 6904, line 5 ff. Non quantitative phrases should be avoided, such as “drops a lot at night”; “runoff becomes very large/rather small”. Page 6904, lines 1-22. This paragraph lacks a figure that describes the general hydrology of this basin, i.e. start/end of snowmelt, freeze/thaw in soil. Alternatively, numbers should be provided (i.e. percentage of annual runoff as springmelt; soil starts thawing around April and is completely thawed by August, etc.) Page 6904, lines 19 ff: Rock and gravel soils are classified as agriculture/grassland soils? Page 6904, lines 22: What is a “typical frost desert soil”? Page 6904, lines 29: Forcing data- what are the errors for radiation and humidity by not correction for elevation and topography? According to Figure 2, the climate station is located on a high altitude location and thus not representative for the entire basin. Page 6905, lines 6 ff. This and further information should be merged into one section on model parameters. Page 6905, line 2: You mean reflectance. Page 6906, line 5: what is “top soil depth”? Page 6906, line 12: moisture set to 0.58 and 0.017 for one entire year- are these average values? The year includes seasonal freezing and thawing, thus a seasonal range of soil moisture is expected. Page 6906, lines 10 ff: This section warrants further analysis; what is the “trial and error” method by which the optimization was done? Do these parameters make any physical sense (table 2)? A sensitivity analysis should be performed to quantify the importance of the model parameters. Page 6906, lines 25 ff: Define BIAS? Page 6907, line 2 ff: “after calibration of a few land surface parameters” is too vague. Page 6907, lines 8 ff: Using the soil temperature at 5 cm depth (where the signal is clearly dampened) as surrogate surface longwave radi-

C2945

ation from the surface introduces a large error! It is not clear why the authors are using this approach. Page 6907, line 3: Timing and magnitudes of peak flows of measured vs. model discharge are different- why? Page 6908, line 1 ff: Information about the method of discharge measurements should be provided in the method section. Page 6908, lines 17 ff: The high RMSE numbers actually show that the soil temperatures are not reproduced “well”. Page 6908, line 19: Not clear what was done with the heat flux transducer data (they measure flux, not temperatures). Page 6908, line 20: Model evaluation should be done for the snow (depth and snow water equivalent).

Comments about the figures:

Fig. 5 e,f,g Why does the simulated soil moisture show a diurnal signal? Fig. 5 f shows clearly that the model does not match the actual field data during the process of thawing, indicating that the parameterization needs improvement. This is critical for accurate discharge predictions. Fig 6. The high measured discharges are not matched with the simulation. Fig 7. What is “deep soil”? The simulated diurnal fluctuations are much higher compared to measured data; furthermore, there should be almost none in the deeper soil zone. Fig 8 b. What is the reason for the moisture increase (without frozen scheme) in February? Comparing the frozen and without frozen simulations to the real measured data, I do not see a better performance of the new model, since neither one of them reproduces the seasonal freeze thaw! For some depths, the old model even performs better than the new model (see Fig. 8 g). Fig. 9. Why is there winter discharge? It is hard to interpret this figure; the y-scale should be enlarged; for measured discharge, I would prefer a line, not symbol plot. Fig 10 a. What is the reason for the peak in simulated discharge in January? I do not agree that “a constant KG” (Fig. B) improves the model. What is the source of base flow during winter?

References:

Page 6913, line 26, Stefan, 1889. This must be a wrong citation. The Stefan formula for determining of the depth of seasonal and perennial freezing thawing should be cited,

C2946

for example, by Yershov, E.D. (1990).

Style/technical comments

-The presentation of the paper is rather chaotic and requires serious organization (suggestions are provided below). -Figures are partly duplicated, for example Fig 6. is partly shown in Fig. 9 again. - The English is sometimes hard to understand; there is switching between past and present tenses; - I suggest a nomenclature in the appendix for clarification of symbols

Additional references:

Bonan, G. (1996). 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

Lunardini V. (1988) Heat conduction with freezing or thawing. Technical Report 88-1, US Army Corps of Engineers

Poutou, E., Krinner, G., Genthon, C., de Noblet-Ducondré, N. 2004. Role of soil freezing in future boreal climate change. *Climate Dynamics* 23: 621-639.

Yershov, E.D. (1990). *General geocryology*, Cambridge University press.

---

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