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Hydrology and Earth System Sciences Discussions

Interactive comment on "Simulation of spring snowmelt runoff by considering micro-topography and phase changes in soil layer" *by* T. Nakayama and M. Watanabe

T. Nakayama and M. Watanabe

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Anonymous Referee #1 (Hydro. Earth Syst. Sci. Discuss., 3, S731-S733, 2006)

Recommendations for paper improvement:

Q.1-1. Section 3 should be rewritten to be understandable. Maybe it would be better to combine this section with appendix A to exclude inconsistencies in variable definitions, undefined variables, and equation discrepancies, e.g., Eq (15a) and (A12a), meaning of g and γ, etc.

Ans.1-1. Because there are many equations published in the previous paper

(Nakayama and Watanabe, 2004) in appendix A, I did not combine section 3 with appendix A. Instead, I changed and revised the manuscript to exclude inconsistencies in variable definitions, undefined variables, and equation discrepancies.

(A12a):

$$q_h = fracK_h \sin\theta_h(x)\gamma h_h , \qquad (0 < h_h < d_h)$$

$$\rightarrow q_h = \frac{K_h \sin\theta_h(x)}{\gamma} h_h , \qquad (0 < h_h < d_h)$$

P2108: ρ_l (kg/m3) and ρ_i (kg/m3) $\rightarrow$ ρ_l (kg/m^3) and ρ_i (kg/m^3)

P2109: 'ψ_s (m) is the soil matric potential at saturation;. . . ' changes to 'g = gravitational acceleration (m/s^2); ψ_s (m) is the soil matric potential at saturation;. . . '.

Q.1-2. Numerous statements in Sections 4 and 5 should be edited, e.g., phrases

'So, we supposed that the hydraulic conductivity increases temporarily in the thawing layer due to the macropores and desiccation cracks of the soil in the previous research at the end of winter' (page 2111), '

'ε is the thermal quality' (page 2111)

'The above equations (7)-(10) include the phase changes in the unsaturated layer, and can simulate the penetration of the frost front in the Sect. 3.4.' (page 2110) .

Ans.1-2. We would like to change to the following phrases;

'So, we supposed that the hydraulic conductivity in the thawing layer increases temporarily double (=2k_f) during this period because the macropores and desiccation cracks of the soil occur as reported by the previous research.' (page 2111)

'ε is the thermal quality (=θ_i/(θ_i+1.09θ_l)) calculated by the simulation results of Eqs. (7-10)' (page 2111)

'The above equations (7)-(10) include the phase changes in the unsaturated layer. The simulated values can be used to calculate the penetration of the frost front as described

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in the Sect. 3.4.‘ (page 2110)

Q.1-3. It is not easy to understand true meaning of the phrase in the Discussion section

‘This model developed a multi-layer surface runoff submodel including the effect of micro-topography and meteorology, includes the phase change transitions in soil moisture, and also considered the effect of the snow layer and the frost/thaw soil layer on spring snowmelt runoff.’ (page 2121).

Ans.1-3. We would like to change to the following phrases;

‘This model developed to include the effect of micro-topography, phase change transitions in soil moisture, frost/thaw processes, and multi-layer surface-runoff process, in order to evaluate spring snowmelt runoff process.’ (page 2121).

Q.2. The authors often refer to not well established assumptions that they use without clarification what and how exactly they use, e.g., ‘So, we supposed that the hydraulic conductivity increases temporarily in the thawing layer due to the macropores and desiccation cracks of the soil in the previous research at the end of winter (Chamberlen Gow, 1979; Benoit et al., 1988)’ (page 2112) but there is no approach itself.

Ans.2. We added the explanation of approach in the manuscript, as described in Ans.1-2.

Q.3. It is not clear how a weighting factor of 1.1 was applied to divide ‘the vertical dimension into 20 layers Ĕ’ to get the upper layer at 2m depth and the 20th layer -250m from the sea surface.

Ans.3. We would like to change to the following phrases;

‘The vertical dimension was divided into 20 layers with a geometric progression (weighting factor = 1.1; finer at the upper layers). The upper layer was set at 2 m depth, and the 20th layer was defined as an elevation of –250 m from the sea surface.’ (page 2113).

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Q.4. While I can agree that the model simulations are compared reasonably well to limited measurement data, the author' statement 'reproduce excellently' (pages 2116-2122) is clear overestimation considering that the determination coefficient equals only 0.4 for ground water and 0.74 for runoff. This statement also contradicts to the author' statements in Discussion section 'Ĕ this discrepancy of runoff is due to an imperfectness of the heat-budget and hillslope surface-runoff submodel of the NICE-SNOW. While the simulation reproduces well the phase changes in the unsaturated layer in winter periods, the simulated soil temperature overestimates the observed value in winter season.'

Ans.4. We would like to change to the following phrases;

'agree excellently' →'agree well' (page 2118)

'the NICE-SNOW reproduces excellently' →'the NICE-SNOW reproduces well' (page 2121)

'The simulated value reproduces excellently' →'The simulated value reproduces well' (page 2121)

'This discrepancy of runoff is due to an imperfectness of the heat-budget and hillslope surface-runoff submodel of the NICE-SNOW. While the simulation reproduces well the phase changes in the unsaturated layer in winter periods (Fig. 7), the coefficient of variation (CV) of simulated results underestimates that of observed values in winter season, particularly, in the mire (A-1) (Table 2).' (page 2123)

Q.5. There are no results to support the conclusion Ĕ the local effect of snow depth and the frost depth disappears in the snowmelt runoff discharge of catchment in the same way as some previous researches'. Fig. 8g that the authors refer to does not support this conclusion too.

Ans.5. We would like to change to the following phrases;

'In this study, it suggests that the local effect of snow depth and the frost depth may dis-

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appear in the snowmelt runoff discharge of catchment because there are not clear differences in the simulation results of river discharge with and without micro-topography effect in the same way as some previous researches'. (page 2122)

Q.6. There is no meaning referring to Fig. 3 in these contexts, pages 2107 and 2110.

Ans.6. We would like to change to the following phrases;

'because the micro-topography and meteorology affect greatly the local snow/frost depths (corresponding to microtopography in Fig. 3).' (page 2107)

'and calculates the phase changes of ice and liquid fractions of soil moisture (corresponding to phase changes in Fig. 3);' (page 2108)

'We suppose that the soil A-layer (Takasao and Shiiba, 1980) is divided into two layers during the winter season (corresponding to two-layer model in Fig. 3):' (page 2110)

Q.7. Figures 7 and 8 need to be scaled up or redone to be helpful.

Ans.7. It is possible to scale up both Figures 7 and 8 because we submitted the electric files with the higher resolution in the original paper.

Anonymous Referee #2 (Hydro. Earth Syst. Sci. Discuss., 3, S1050-S1055, 2006)

[1] The review of comparable work is insufficient. It groups some models that are clearly different. It also leaves out major work by other researchers which is clearly relevant for this paper and has to be mentioned! Incl. work by Grayson, Bloeschl etc. as discussed in detail below. The authors should include a proper review of comparable studies in the beginning of their paper and should also discuss their results in the context of these studies.

Ans.[1]. We categorized the previous models correctly by the helps of reviewer's comments in the manuscript. Furthermore, we would like to change to the following phrases;

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"Because there are few studies about the process-based modeling including the slope and shading characteristics of microtopography and the phase-change transitions in soil moisture, it is very important to evaluate the hydrologic process in the catchment in relation to the micro-processes of soil water." (page 2106)

→ *"Because there are not so many studies about the process-based modeling including the slope and shading characteristics of microtopography and the phase-change transitions in soil moisture, it is very important to evaluate the hydrologic process in the catchment in relation to the micro-processes of soil water." (page 2106)*

"Some researches show the predominance of snow effect (Shanley and Chalmers, 1999) and the importance of frozen effect (Stahli et al., 2001) in runoff response." (page 2104)

→ *"Some researches show the comparative analysis of physically-based snow model (Jin et al., 1999; Molotch and Bales, 2006), the predominance of snow effect (Shanley and Chalmers, 1999), the importance of frozen effect (Stahli et al., 2001) in runoff response." (page 2104)*

Furthermore, please add the following references;

Jin, J., Gao, X., Yang, Z.-L., Bales, R.C., Sorooshian, S., and Dickinson, R.E.: Comparative analyses of physically based snowmelt models for climate simulations, J. Climate, 12, 2643-2657, 1999.

Molotch, N.P., and Bales, R.C.: Comparison of ground-based and airborne snow surface albedo parameterizations in an alpine watershed: impact on snowpack mass balance, Water Resour. Res., 42, doi: 10.1029/2005WR004522, 2006.

[2] The authors' should improve the language of the paper. It would probably help a lot if they could find a native speaker to help them with this effort.

Ans.[2]. We revised the manuscript by adding this list of changes in order to improve the language of the paper.

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[3] It is currently not clear from the information provided, how the model was set up. How many grid cells are there and how many parameters? Have some of the parameters been estimated through calibration? Using what data? Etc.

Ans.[3]. We would like to add the information about how the model was set up, how many grid cells are there, and how many parameters in the manuscript. So, we would like to change to the following phrases;

"The simulation area is 50 km wide by 80 km long, covering the whole Kushiro River catchment (Fig. 1). This area is discretized into a grid of 100 x 160 blocks, with a grid spacing of 500 m. The simulation was conducted on an NEC SX-6 supercomputer from 1 January 2001 to 31 December 2002 by using the interpolated forcing data at each grid from the observed data and the AMeDAS data (Table 1). A time step of Δt=1 hour was used." (page 2115)

About the model calibration and validation processes, we would like to change to the following phrases;

"which includes some grass roots in the upper level and consists mainly of silt and ash deposits in the deeper levels. Details are written in Nakayama and Watanabe (2004)." (page 2113)

→ "which includes some grass roots in the upper level and consists mainly of silt and ash deposits in the deeper levels. These soil parameters such as soil layer, hydraulic conductivity, porosity, et al., were calibrated by using the soil sampled data and the previous researches, as described in Nakayama and Watanabe (2004)." (page 2113)

[4] Another important point that the authors ignore is that their model is tremendously complex with lots of parameters. They nonetheless have conclusions that clearly point to particular model components. I find this surprising considering the vast amounts of uncertainty that must be present in a model of the type used here. The authors should discuss the issue of uncertainty in the context of their paper, and the problems

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of analyzing very complex physically-based models.

Ans.[4]. We added newly a table (table 1) listing important model parameters in this study, including their units. The reader can understand easily the important model parameters in this study.

Furthermore, we would like to change to the following phrases;

"To reproduce this effect more correctly, it is necessary to conduct more observation in the vertical direction of local area and simulate by including the frozen/melted cycle in the surface runoff with finer mesh resolution in the vertical direction." (page 2123)

→ *"To reproduce this effect more correctly, it is necessary to conduct more observation in the vertical direction of local area and simulate by including the frozen/melted cycle in the surface runoff with finer mesh resolution in the vertical direction. The above necessities are closely related to how better to input various model parameters with spatial heterogeneity, to reproduce the observed snowmelt processes, and to evaluate problems of analyzing very complex physically-based models such as NICE-SNOW." (page 2123)*

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Table 1: List of important model parameters and units used in the NICE-SNOW model.

Symbol	Physical definition	Value	Unit
W_a	Observed total solar radiation	-	W/m^2
W_b	Total solar radiation perpendicular to the ground surface	Eq.(1)	W/m^2
θ	Slope angle	Calculated from elevation data	-
$\theta_{l,j}$	Liquid water content of each soil layer j	Eqs.(7-10)	m^3/m^3
$\theta_{i,j}$	Ice content of each soil layer j	Eqs.(7-10)	m^3/m^3
L_f	Latent heat of fusion	334.7x10^3	J/kg
T	Soil temperature	Eqs.(A1-A3)	$^\circ$C
ξ_f	Frost depth	Eq.(12)	m
ξ_t	Thaw depth	Eq.(13)	m
κ_f	Thermal conductivity of frost soils	Cherkauer & Lettenmaier, 1999	W/m/K
κ_t	Thermal conductivity of thawed soils	Cherkauer & Lettenmaier, 1999	W/m/K
T_f	Freezing point of water	272.16	K
α	Depth ratio of the A_1-layer to the A-layer	Eqs.(7-13)	-
k_f	Hydraulic conductivity of frost soil	Eq.(16)	m/s
ε	Thermal quality (= θ_i /(θ_i +1.09θ_l))	Eqs.(7-10)	-
E_i	Impedance parameter	3	-

Specific Comments

P2104:

“Many models of snowmelt runoff“ - This is a very strange collection of models listed by the authors here. First, some of these are rainfall-runoff models with a snow component, rather than being snowmelt runoff models. Second, HEC is a group of models ranging from hydraulic to hydrologic, a more specific definition is needed.

Ans. We would like to change to the following phrases;

“Many models of rainfall-runoff with a snow component”

Please remove “HEC (Hydrologic Engineering Center) (Hydrologic Engineering Center, 1997)” in the manuscript.

“These semi-physical surface runoff models can predict generally well the spring peaks and recessions, but cannot evaluate quantitatively both the snow and frozen effects on

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spring runoff because of the dependence on various empirical relations (Semadeni-Davies, 1997)." - This is too vague. What empirical relations?

Ans. We would like to change to the following phrases;

"These semi-physical surface runoff models can predict generally well the spring peaks and recessions, but cannot evaluate quantitatively both the snow and frozen effects on spring runoff because of the dependence on various empirical relations such as degree-day approach, percolation term, and drainage term, and because of the imperfectness of phase changes in unsaturated layer (Semadeni-Davies, 1997)." (page 2104)

The authors then call the SAC model a physically based model. However, the SAC and the NWSRFS are actually the same models! The model is either semi physically based or it is physically based, but not both!

Ans. Please remove "SAC (Burnash et al., 1973)" in the manuscript.

Models like VIC and NOAHS (and SAC for that matter) do not explicitly consider topography in their formulations. Again, the authors should be a lot more specific, rather than making these broad statements!

Ans. Please remove "Noah (Ek et al., 2003) and VIC (Variable Infiltration Capacity) (Liang et al., 1994; Cherkauer and Lettenmaier, 1999)" in the manuscript.

"By the way" is an inappropriate phrase for use in a journal paper.

Ans. Please remove "By the way" in the manuscript.

P2105:

"Though the mean Ĕ typhoon seasons (Fig. 2)." - What is the connection between the figure and this sentence?

Ans. We would like to change to the following phrases;

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"Though the precipitation is smaller at snowment season than at typhoon season, the river discharges at both seasons flow more nutrients and sediments into the mire, which is estimated to play an important effect on these drying phenomena (Fig. 2)."

P2106:

"Because there are few studies ą Eof soil water" - There might be few studies, but the authors should still mention some. I think there have been plenty of studies on this subject including the work by Guenther Bloeschl, by Rodger Grayson and by Roger Bales. The authors should include references to the work of those authors and compare their work to the work of those authors. Particularly since the used spatially distributed models that actually include topography in detail.

Ans. We revised to include references to the works by Guenther Bloeschl, Rodger Grayson, and Roger Bales in the manuscript. Details are described in the above Ans.[1].

"the seepage between river and groundwater" - I think seepage would refer to the flow from river to groundwater, rather than flow in both directions.

Ans. We would like to change to the following phrases;

"the water flux between river and groundwater" (page 2106)

P2108-2110:

While the authors provide details about the model equations, they left me confused about what the model parameters are. The authors should include a table listing all the model parameters, including their units. This would be more important than the currently included table 1 with details about measurement locations. In addition, the authors should mention how estimates of these parameters were obtained, e.g. were they calibrated, were they based on soils data, were they based on experience etc.

Ans. We added a table (table 1) listing important model parameters in this study, in-

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cluding their units. We would not like to remove the original table 1 with details about measurement locations if the reviewers agree to do it because this table includes important information about measurement locations. And please change the original table 1 to table 2, and the original table 2 to table 3, in the manuscript.

Furthermore, we would like to change to the following phrases;

"κ_f and κ_t (W/m/K) are the thermal conductivity of frost and thawed soils;" → "κ_f and κ_t (W/m/K) are the thermal conductivity of frost and thawed soils estimated by the method of Cherkauer and Lettenmaier (1999);" (page 2110)

Details in the model calibration and validation processes are described in the Ans.[3].

P2122:

Having a section called 'measurement for validation' suggests that the other dataset was used for calibration? Is this correct? If so, details on the calibration process (manual versus automatic, objective functions etc.) should be included.

Ans. Details in the model calibration and validation processes are described in the Ans.[3].

P2112:

"There are no observed data of groundwater level in winter because it is difficult to set up the equipments." - There are no continuously installed groundwater wells?

Ans. There were no continuously installed groundwater wells in this mire during 2001-2002. We would like to observe the groundwater level in the near future.

P2113:

The explanation of the spatial resolution of the model should be placed earlier in the text. In connection with a discussion of the model equations and parameters. I would also like to know how many grid cells the model has and how the parameters were

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distributed throughout the grid - particularly if any type of calibration was included.

Ans. We added a table (table 1) listing important model parameters in this study, including their units. Details in the model set-up are described in the Ans.[3] and [4].

P2114:

“inputted” not the correct form of the word.

“are written” - are described

Ans. We would like to change to the following phrases;

“inputted to” → “assimilated with”

“are written” → “are described”

P2115:

“to facilitate” - better to achieve

Ans. We would like to change to the following phrases;

“to facilitate” → “achieve”

P2116:

“The simulation reproduces well the observed values at both places (r2Nobo=0.556, r2Toma5 =0.727).” - The authors make quality assessments like this one throughout their paper. This is very dangerous because it can be very subjective! I for example do not believe that an r2 of 0.556 means reproducing the data well at all! If the authors want to include statements like the one above, then they should clearly define (in a Table or in the text) how they judge quality based on r2 values. For example, state that you believe everything above 0.5 is good, everything above 0.7 is excellent etc. Anything below 0.5 is poor and so forth. Then the reader can judge for himself whether he agrees with this ranking scheme or not. Simply using quality statements is not sufficient!

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Ans. We would like to change to the following phrases;

"The simulation reproduces well the observed value at Tomakomai (r^2_{Toma}=0.727), but does not so well at Noboribetsu (r^2_{Nobo}=0.556)."

"The NICE-SNOW cannot simulate correctly the snow depth affected by the almost saturated groundwater in mire, which is also correlated with the overestimates of soil temperature written in the following section 5.3." - I don't understand this sentence.

Ans. We would like to change to the following phrases;

"The NICE-SNOW cannot simulate correctly the snow depth in mire because it does not completely include the effect of almost saturated groundwater on the snow depth, which is also correlated with the overestimates of soil temperature written in the following section 5.2." (page 2116).

"by the Eq." - should be using Eq.

Ans. We would like to change to the following phrases;

"by the Eq." → "by using the Eq." (page 2116).

P2117:

"and the NICE-SNOW can reproduce the frost depth on the northern slope is larger." - I don't understand this sentence.

Ans. We would like to change to the following phrases;

"and the frost layer on the northern slope is deeper than that on the southern slope."

P2118:

"The simulation values agree excellently with the measured values all through the two years depending on precipitation and micro-topography both in the mountainous areas (r2G-13=0.649), around the mire (r2G-23=0.719) and in the mire (r2G-29=0.408)," - A value of 0.4, and even a value of 0.65 is really NOT excellent!

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Ans. We would like to change to the following phrases;

"The simulation values agree well with the measured values all through the two years depending on precipitation and micro-topography both in the mountainous areas (r^2_{G-13}=0.649), around the mire (r^2_{G-23}=0.719) and in the mire (r^2_{G-29}=0.408),"

P2124:

The last paragraph (starting with "The effect of Ĕ") and Figure 2 are really out off place in this paper since none of the rest deals with this issue. I suggest the authors focus on the transport of water here and leave the rest to their next paper where they can then deal with solute transport properly.

Ans. We agree the reviewer's comments in some points. But we think this last paragraph and Fig.2 are very important to discuss in this manuscript. This paper insists the effect of snowmelt runoff on the downstream mire shrinking by the newly developed model in order to clarify the cold season runoff generation mechanism. We are now researching the solute transport and trying to submit the journals in the next paper. So, we would like to leave this last paragraph and Fig.2 if the reviewers agree to do it.

Figures 5-7:

The authors need to explicitly explain how the observations and simulations were aggregated for these plots. Is this a simple comparison of the mean values? If so, what are limitations of using such a simple approach?

Ans. Figures 5 and 6 are not comparisons of the mean values, but those of the instantaneous values. The simulation of heat budget was conducted at the time step of 1 hour. The snow depth and frost depth were measured every one-two weeks, as described in the manuscript (page 2112).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 2101, 2006.