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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

Anonymous Referee #2

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General Conclusions

This is an interesting paper about modelling runoff from snow dominated areas. It is clearly a relevant area of research in which advancements are needed. HJowever, the paper has currently a few major points that require attention:

[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.

[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.

[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.

[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 of analyzing very complex physically-based models.

The authors should address these points in detail before the paper would be acceptable for publication.

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.

“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 (Semadeni-Davies, 1997).“ - This is too vague. What empirical relations?

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!

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!

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

P2105:

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

P2106:

“Because there are few studiesĚof 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.

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

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

HESSD

3, S1050–S1055, 2006

they calibrated, were they based on soils data, were they based on experience etc.

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.

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?

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

P2114:

“inputted” not the correct form of the word.

“are written” - are described

P2115:

“to facilitate” - better to 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!

“affected by the almost saturated groundwater” - I don’t understand this sentence.

“by the Eq.” - should be using Eq.

P2117:

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

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!

P2124:

The last paragraph (starting with “The effect ofĚ”) 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.

Figures 5-7:

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

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HESSD

3, S1050–S1055, 2006