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Hydrology and Earth System Sciences Discussions

Interactive comment on "Multi-variable evaluation of hydrological model predictions for a headwater basin in the Canadian Rocky Mountains" *by* X. Fang et al.

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Response to referee J. Parajka's comments:

General comments

1. The paper aims to assess the understanding of hydrological processes in cold regions by using a flexible physically based modeling platform. As it is discussed in Parajka et al. 2013, the choice and cross-validation of appropriate model structure is still a big challenge, particularly in PUB context. The objective of the paper is to examine this question, however, in the current form, the results need to be more clearly linked and

discussed with respect to the choice and parameterization of particular model structure.

Response to general comment 1: One of the central tenants of the PUB Decade was to improve prediction by improving understanding and reducing calibration (Sivapalan et al., 2003). This paper makes a clear contribution to this objective by the following: i. we have now added model falsification to the evaluation of the model – this takes advantage of the flexible structures possible in CRHM. By falsification we deliberately remove hydrological process representations from the proposed model and evaluate the degradation in the model performance; ii. parameterisation of the model is primarily from physical measurement or in accordance to physical laws – not by optimization. The model in this paper is not calibrated and shows the capabilities of physically based models that are designed to demonstrate the prediction possible with our hydrological understanding of the system. In this manner it is similar to the physically based modelling approach demonstrated by Semenova et al (2013). iii. Regionalization approaches are used for certain parameters that cannot be locally measured and imply the transference of model parameters from a basin that is expected to behave similarly to the basin of interest. The similarity measure can be based on spatial proximity, basin attributes, or similarity indices Blöschl (2005). There are several regionalisation techniques, however nearly all the studies follow the same approach (e.g. Blöschl and Sivapalan, 1995; Abdulla and Lettenmaier, 1997; Fernandez et al. 2000; Littlewood 2003). Typically, regionalisation techniques involve the definition of relationships between model parameters and basin attributes. For instance the use of water storage capacities from other mountain basins (Clow et al., 2003; McClymont et al., 2010) to Marmot Creek is a regionalisation based on hydrological similarity. In general the regionalisation used in this model is that based on detailed process research as proposed by Kuchment et al. (2000) where model structure is chosen based on analyses of runoff generation processes at a research basin. This is now noted in the text.

2. The authors describe in detail the modeling of different processes, but it is not

always clear how they relate to the observations used in evaluation. For example, authors conclude that the major snow-related processes (e.g. snow interception, sublimation, unloading, snow redistribution, etc) were well represented by selected model structure, but from presented results (e.g. figure showing total snow water equivalent) it is not clear whether and why it is the case? (How much are particular processes contributing to the overall snow mass balance at particular places? Does the particular parameterization of snow interception and/or redistribution dynamics contribute to the overall agreement between model simulations and observations? What is the role of different landscape and vegetation characteristics and their parameterization?) I would strongly suggest that authors carefully redesign the figures, in order to clearly demonstrate and justify the interpretations made about the choice and parameterization of selected processes.

Response to general comment 2: To demonstrate how the parameterization of snow interception and/or redistribution of snow by wind contribute to the overall agreement between model simulations and observations, other snow mass balance components were added to the figures. These include cumulative snow sublimation of intercepted snow, snowmelt, and snowfall, which have been added to Figure 6 for the forest environment. Cumulative snow sublimation from both blowing snow and intercepted snow, blowing snow transport and snowmelt as well as cumulative snowfall were incorporated in Figure 7 for the alpine environment. The revised figures show role of the parameterization of snow interception and sublimation for forest environment and blowing snow transport and sublimation for alpine environment in model predicted snow accumulation on the ground.

3. Secondly, I would suggest to discuss in more detail the reasons for relatively poor streamflow predictions. Particularly, it would be interesting to know the reasons for "unexplained spikes" in simulated hydrographs and why was the model unable to adequately reproduce the hydrographs at sub-basin scale?

Response to general comment 3: Several sensitivity tests were conducted to diagnose

the spiky hydrographs particularly at sub-basin scale. It was found out that the spiky hydrographs were caused by fast moving sub-surface flow, whose travel time was not accounted for in the original model simulations. In Marmot Creek Research Basin, streamflow is generated from the fast moving sub-surface flow of melt water and rainfall infiltration; the sub-surface flow moves through the shallow and top soil layer that is highly permeable and travels along steep slopes to the streams in Marmot Creek. To address this, a method to estimate the sub-surface flow travel time was added and described in the Section “3.2.7 Routing parameters”. Simulated hydrographs after this addition show improvement and large spikes disappeared. Figure 10 has been updated with this addition.

Specific comments

Comment 1: Constant environmental lapse rate: Please justify the value (0.75) and consider to discuss the effect of model input(s) uncertainty on the results (model structure complexity versus data availability). Response 1: The value of 0.75 for the environmental lapse rate is an average seasonal value for Marmot Creek. Since we have a relatively large number of meteorological stations for a small basin the measurement elevations are not extrapolated to substantially different elevations and so a simple mean lapse rate is considered adequate. The increase in model complexity and parameter requirement from a synoptic lapse rate or seasonal lapse rate is not a warranted complexity in this case. However in cases with fewer weather stations we agree that it is important to consider.

Comment 2: Hillslope module parameters: This section is rather long in comparison to other parts. How relevant it the level of detail with respect to the overall objectives? Response 2: The Hillslope Module is new module recently added to the Cold Regions Hydrological Modelling platform (CRHM) and not described in any publication. This is the first attempt using this module for simulations and evolutions in Marmot Creek; thus this section has been given a more detailed description.

Comment 3: Snow evaluation- "the timing of snowmelt was excellent ..": Please provide more details on why it is the case? What parameters/process representation are important? Response 3: Cumulative snowmelt has been added to figures to demonstrate snowmelt episodes over season. With model falsification and evaluation of degraded model performance we can now show that blowing snow and canopy processes are important processes in for snowmelt timing.

Comment 4: Figures: Please consider to demonstrate more clearly how selected model structure improves process representation. Please consider also to show shorter periods, when necessary (the entire time series are difficult to read). Figures showing the variability along some interesting transects (e.g. showing snow redistribution) might also be an alternative. Response 4: Both Figure 6 and 7 have been revised, and major snow-related processes have been added to the figures.

Comment 5: Fig.6, 7: Please consider to show snow simulation as a line. Response 5: Yes. Simulated snow accumulation has been shown as line.

Response to referee #2's comments:

General comments

This is a well written manuscript showing the evaluation of relatively well simulated soil moisture, snow water equivalent and poorly simulated ground water level and streamflow. As other variables (soil moisture, snow water equivalent) are relatively well simulated, it is necessary to deepen our understanding why the streamflow are poorly simulated. I strongly recommend the authors to re-analyze and investigate the poor simulation of streamflow and provide more insight about the causes and mechanism of such poor streamflow simulation. After addressing this issue, the manuscript shall be considered for publication in HESS.

Response: Investigations and sensitivity test runs were carried out to diagnose poor simulation of streamflow and the spiky hydrographs particularly at sub-basin scale.

Results of investigations and sensitivity tests reveal that the spiky hydrographs were caused by fast moving sub-surface flow, whose travel time was not accounted for in the original model simulations. In Marmot Creek Research Basin, streamflow generation is normally from the fast moving sub-surface flow as result of melt water and rainfall infiltration. Sub-surface flow moves through shallow and top soil that is highly permeable and travels along steep slopes to the streams in Marmot Creek. To address this, a method to estimate the sub-surface flow travel time was added and described in the Section “3.2.7 Routing parameters”. Simulated hydrographs after this addition show improvement through the reduction of the large spikes. Figure 10 has been updated with this addition.

Specific Comments/Suggestions

Comment 1: Page 12832, line 28, Please clarify “Upper clearing, Upper forest and level forest”. Response 1: Yes. It is clarified by rephrasing “Upper Clearing, Upper Forest and Level Forest stations”. These are three of those seven hydrometeorological stations shown in Figure 1.

Comment 2: Please consider in shortening the chapter 3.2.6 Hillslope module parameters. Equations 1 to 4 can be represented in a single equation. Response 2: Yes. Equations 1 to 4 have been combined to a single equation.

Comment 3: As the value for each parameter for different forest and sub-basins are almost same, I recommend to reorganize Table 2. For example, it can be written as soilrechrmax 250 soilmoistrmax 550 (425 at subalpine forest and 750 at confluence). Response 3: Yes. Table 2 has been reorganized.

Comment 4: Please provide RMSD, NRMSD and MB for each snow season in Table 3. Response 4: Yes. This is done. Table 3 has been updated.

Comment 5: Please revise figures 6 and 7 by showing line graph. Response 5: Yes. This is done. Simulated snow accumulation has been shown as line in updated Figures

6 and 7.

Comment 6: Why there is large discrepancy of SWE simulation in 2009/10 (Fig. 7 c,d,e)? Please explain. Response 6: There modelled blowing snow transport exceeded that measured – this might be due to errors in precipitation observation at this elevation or more likely to a different wind direction regime in this year from most years. The blowing snow parameterization follows a fixed routing of blowing snow and so when this varies due to rare weather events the parameterization will fail. This is noted in the discussion of this figure.

Comment 7: The observations for soil moisture are for 0-25 cm. At what depth, the soil moisture is modeled? Please relate clearly the modeled value to observations for analyzing the biases. Response 7: Yes. The simulated soil moisture is for depth to 25 cm below surface. It has been clarified.

Comment 8: Please discuss a little about the future strategies for improving the groundwater simulation in CRHM in section 4.3. Response 8: We hope to couple CRHM to a more physically realistic groundwater simulation model. In the meantime we have made improvements to the physical basis of the groundwater flow calculations from the original version of the text.

Comment 9: Please consider not using qualitative words in streamflow simulation section – generally matched, quite comparable etc for poor simulation results. Response 9: These words have been removed.

Comment 10: Please provide yearly NSE, MB, RMSD in table 5. Response 10: Yes. This is done. Table 5 has been updated.

Response to Editor FF Fenicia's comments:

Comment 1: This paper is essentially a case study. It is clear that your paper will be of interest for future research on the selected study area. Can you please clarify any relevance of your work outside the scope of the hydrological research on the Cana-

dian Rocky Mountains? Response 1: We found this statement to be perplexing as we thought that HESS was an international journal with interests outside of Europe. In any case the scope of HESS is irrelevant here as the Canadian Rockies are a tall, cold, snowy mountain range with many similarities to the Alps, US Rockies, Himalayas, Andes, and Pyrenees. These regions are the 'water towers' of the world, providing streamflow for lowland regions where water demand often exceeds local runoff generation – this includes much of Europe. The model developments shown here are being applied by our collaborators in the Alps, Pyrenees, Tibet, US Rockies and so are of great relevance to snowy mountains around the world and hence to the vast majority of the human population that relies on mountain water for food, economy and life itself. An example is the paper by Lopez-Moreno et al., (2012).

Comment 2: Given the large body of research on the selected study area and on the modelling tools used in this paper, it is not clear to me what this paper brings in addition. For example, you state that objective (i) is to "propose a comprehensive physically based model...". But to my understanding, the model already exists, it is here only being applied. Objective (ii) is to "evaluate the model performance against the field observations...". Is this something new, and why? Response 2: Yes, Cold Regions Hydrological Modelling platform (CRHM) already exists. However, CRHM is not a model itself and is merely a model assembling and running platform. That is, different types (i.e. prairie, mountain, sub-arctic, etc.) of model can be created using CRHM based on the selected modules; this is the purpose of modular feature in CRHM. For the model present in this paper, it is a comprehensive model setup for a headwater mountain basin in Canadian Rocky Mountains (e.g. Marmot Creek Research Basin), and this is a collection of many previous years of research conducted in the basin. New algorithms have been developed and incorporated into modules, such as forest snow mass and energy balance module and the hillslope module. The new modules have just been tested, which is shown for the first time in this paper.

Comment 3: With respect to the analysis. Together with the table listing model parameters and their values, it would be good to add a description of how they were estimated (e.g. in situ measurements, literature, ...), and some "degree of confidence" on the parameter values. Response 3: The methods used for estimating parameters were given in Section 3.2 “Model parameter estimation”. This entire section was dedicated to describe how parameters were estimated. Many estimated parameters values (e.g. forest snow mass and energy balance module, blowing snow, long-wave radiation module parameters) were derived from previous studies in the basin; “degree of confidence” on the parameter values was addressed in publication from those studies. This is the method proposed by Kuchment et al. (2000) for prediction with improved understanding using runoff generation process studies. For this paper, estimated parameters from those previous studies are assumed valid for model application in the same basin.

Comment 4: It is difficult to judge whether the application of this model was successful or not, because there are no terms of comparison. It would be interesting, for example, to compare the simulation at Cabin creek with the parametrizations used for the other locations, to check if indeed the parametrization of Cabin creek works better than that of, e.g., Marmot creek. Response 4: It is indeed quite difficult to judge the model application would be successful at other locations and situations given the length of this paper. However, model falsification runs have now been conducted to demonstrate whether the model parameterizations are appropriate for Marmot Creek Research Basin. That is, model parameterizations which accounted for forest canopy snow mass and energy, blowing snow, evapotranspiration, and sublimation have been deleted in the model falsification runs. Results show that model falsification worsens model performance (MB, NSE, etc.) in predicting snow accumulation and streamflow compared to the current model parameterizations. A new section for this model falsification has been added in the paper to address the importance of current model parameterizations in Marmot Creek.

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