Authors' reply to interactive comment posted by Dr. Jan Seibert on behalf of one of his students regarding the HESS Discussion paper "Simulating cold-region hydrology in an intensively drained agricultural watershed in Manitoba, Canada, using the Cold Regions Hydrological Model"

Dear. Dr. Seibert and student,

We appreciate the comments from your student and would like to address major points in the document, as follows:

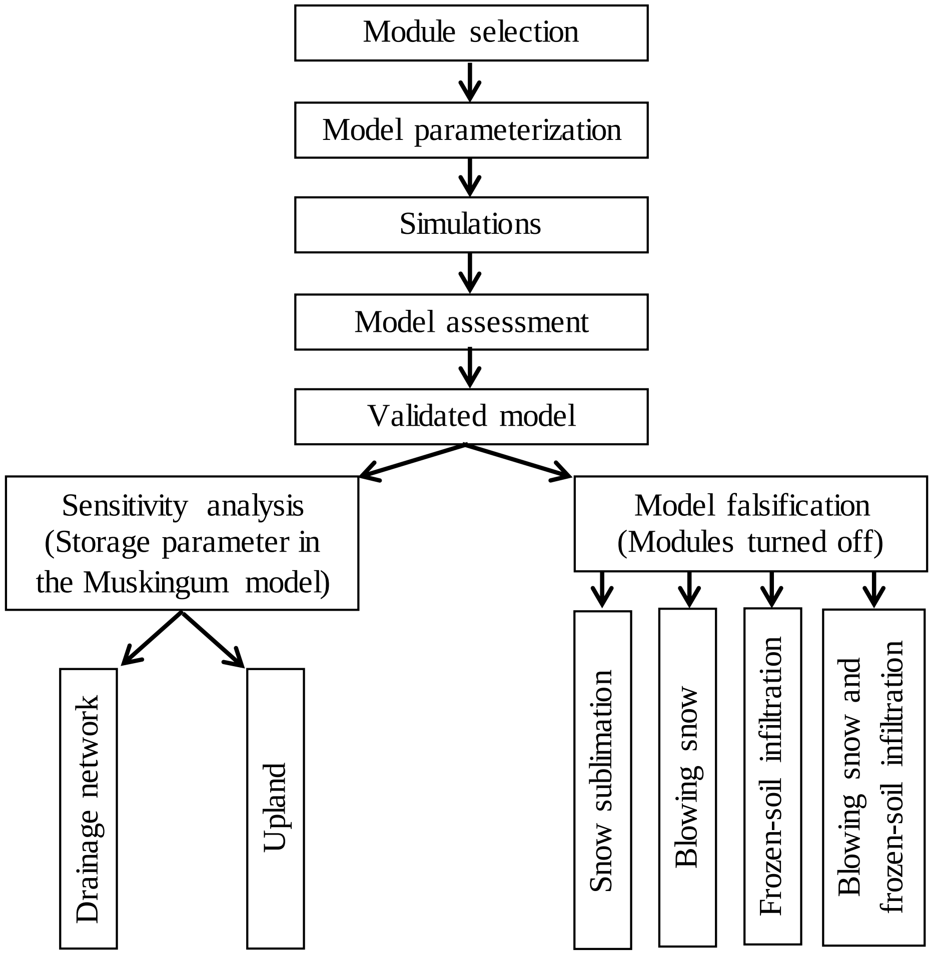
Main assessment

1. Reviewer: *The literature review however shows that mainly well-known as well as for national concerns developed concepts and models are applied which is not convincing regarding the actuality and importance of the paper. It is not clear how this topic is addressed on a global scale.*

   Authors: It is acknowledged that the paper deals with a very specific setting, which is a distinctive combination of cold-regions physiography and land use. However, important insights can be gained from application of the model in this environment. Much research attention has been focussed on cold region agricultural environments, but significant challenges exist in modelling hydrological processes on the Canadian Prairies and other similar agricultural regions globally where runoff events are mostly concentrated during the snowmelt (Shook and Pomeroy, 2010;Liu et al., 2014, 2013). Many of the most productive agricultural areas in these regions are characterized by relatively flat topography, low-permeability soils, and intensive agricultural practices. As a result, scaling results and application of models developed for other agricultural regions tends to result in poor model performance. A recent discussion by Wheater and Gober (2015), based on observations or simulations done in a case-study basin on the Prairies, highlight the importance of such efforts since a single basin can "embody many of the challenges" faced by other regions. Those authors cite the work done in South Tobacco Creek watershed, whose area is only around 73 km^2 but where work has been developed to improve local and regional modeling capability for BMPs. While South Tobacco Creek watershed has many unique features and is not discussed in a global context, work done there has furthered the scientific knowledge of hydrological processes in cold regions in a broader sense. Likewise, the work reported in the present manuscript can have the same impact and fill some of the current knowledge gaps in cold agricultural regions.

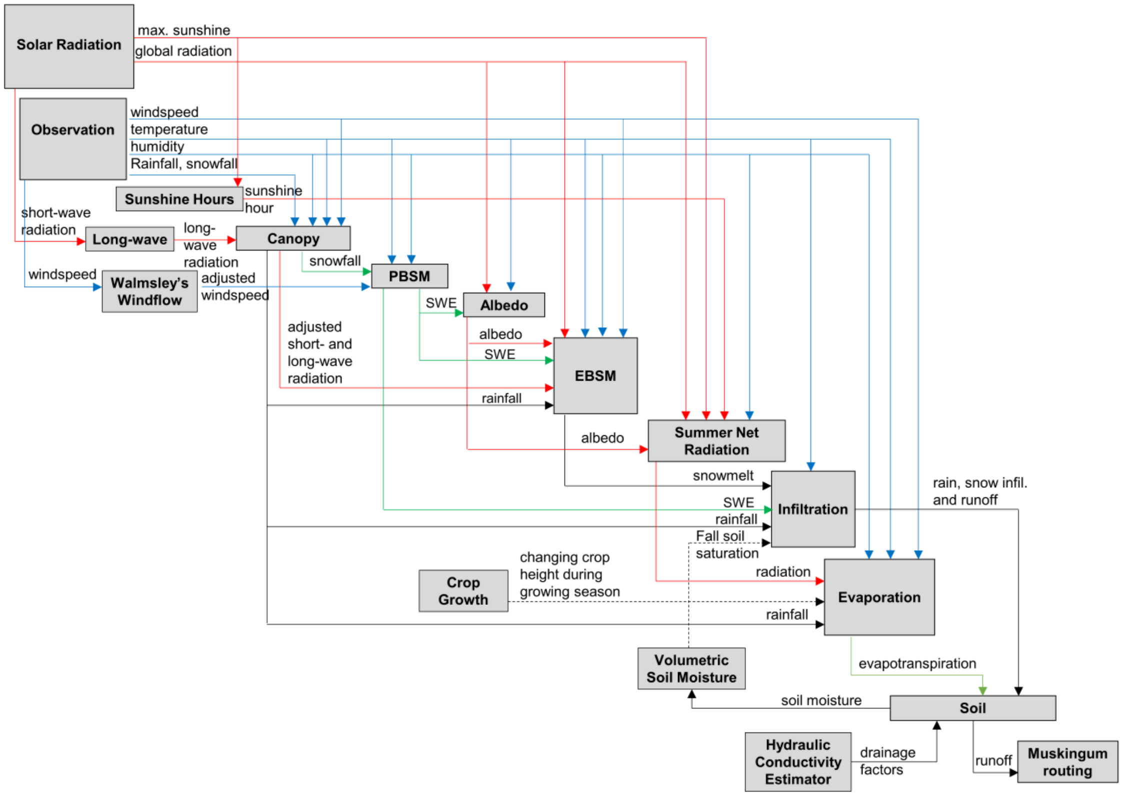
2. Reviewer: *The paper is challenging to read, as it describes every step within its technical details and has no structural overview (process chart).*

Authors: A flowchart showing the workflow used in the analysis has been added as supplemental material in the revised manuscript (figure is reproduced below for convenience). The sub-sections in Material and Methods sections match the description in the figure.



Major points

3. Reviewer: *For a reader this paper is hard to follow; which step, which model-part is described where. Therefore, include a process chart and use same terms. Plus, insert graphs and tables, where they are mentioned.*
   Authors: A process chart of the model developed for this exercise has been included in the supplemental material of the revised submission (figure reproduced below for convenience). Regarding figures and tables placement, it is standard procedure for these elements to be placed at the end of the manuscript during the review process. Proper placement will take place at the typesetting stage, when the document will have its final format defined.



4. Reviewer: *The Falsification used in this research tries to assess model performance by removing modules and characterize the resulting model performance. This approach has in my opinion two weaknesses: i) The time period falsification is conducted includes a period where no statistical measures for model performance have been done. This is critical regarding the informative value of the falsification; ii) Evidence of falsification is not given through this approach or it is at least not discussed. Discussing only the model performance differences, the proof of falsifiability is still not given*.
Authors: Regarding the first point, statistical performance can only be assessed when observed data is available. In the case of stream discharge, which was the variable used for model assessment, records were only available from March to October because most streams in the Canadian Prairies are frozen during the winter. However, the processes evaluated in the model falsification, which will influence stream discharge, take place at the onset of winter. For example, soils can be frozen as early as November in the study area, depending on weather conditions. Similarly, blowing snow can take place as soon as the precipitation phase changes from liquid to solid, which can happen as early as October. Development of these dynamic conditions over the winter will strongly affect stream discharge, as discussed in the manuscript. Thus, beginning the falsification only when stream discharge data is available would not allow for the processes to

be simulated. Regarding the second point, evidence of falsification was not extensively discussed for two reasons. First, due to the manuscript length, presentation and discussion of the variables pertaining to the falsified processes were not included. Second, there was no observed data to be assessed against the simulations of these variables. This is why the impact on modelled flow was utilized for comparison.

5. Reviewer: *Review gives a hint for incomplete investigation of existing research. Try to include as well international research and look for similar approaches.*
   Authors: Research from Europe and Asia has been cited in the introduction. However, studies using similar approaches or even dealing specifically with agriculture in cold regions at the scale used in the present study were not found in the literature review process.

6. Reviewer: *Material and Method: This chapter is too detailed and can be shortened. Find out which information is really considerable and important to understand your Model process. The rest can be shown in a process chart or can be described as an adaptation of existing concepts/models etc.*
   Authors: The number of analyses included in the manuscript made it difficult to shorten the length of the Material and Methods (M&M) section after adding additional detail that was requested by reviewers. However, a few paragraphs of the M&M section have been shortened or included as supplemental material and a process chart have been added as per suggestion.

Minor points

7. Reviewer: *Introduction: Research content and aims are mentioned, question and hypothesis are missing*
   Authors: Research questions and hypothesis have been added in the last paragraph of the Introduction section, as suggested

8. Reviewer: *Implementation of land-use split method to define HRU's: Use of this approach has to be justified and evaluated. Both is done insufficient.*
   Authors: The land-use split methods has been evaluated in previous modelling exercises in the study area using SWAT, which is documented in the manuscript with the proper sources cited [i.e. Yang et al. (2014)]. This approach is justified since it facilitates and expedites model setup and parameterization. This explanation is given in the section dealing with HRU definition, which is now presented as a supplemental material with the revised version of the manuscript (c.f. answer to question #6). The validity of the land-use split method to represent crop rotations in the study area is confirmed by the satisfactory simulations of two

independent model exercises [i.e. the present analysis and the one done by Yang et al. 2014] performed with different tools running at different time-steps. Also, the semi-distributed nature of the model, where HRUs are not spatially represented, decrease the effect of fine representation of crop rotations on model results. Evaluation of this technique is out of the scope of the present work and would itself be a standalone manuscript. That said, the current representation of agricultural areas using the land-use split method constitutes an advance in simulating this type of land use in a modelling framework in CRHM, since past applications of the model treated cropland in a simplified and static manner.

9. Reviewer: *Assessment of the model: Besides the falsification the model assessment period for the statistic metrics (March to October) makes no sense without i.e. meteorological explanation of the catchment. It seems like the assessment for snow related runoff modelling is conducted in snow free periods.* Authors: Meteorological explanation of the catchment is a very important topic. Its complexity does not allow a complete treatment within the limited space in the manuscript at hand; however, aware of the importance of the weather inputs to model simulations, the authors have prepared a companion manuscript to be submitted to the ESSD journal that describes the uniqueness of the datasets used to force CRHM in the present analysis. The manuscript is nearly complete and will be submitted soon. We have also noted in the revised manuscript that runoff producing snowmelt events do not occur over the course of winter in this environment as in warmer climates, rather snowmelt runoff only occurs with the onset of spring. Regarding assessment, as explained in the answer to question #4, model assessment was conducted between March and October, which encompasses the end of the snow period (i.e. snowmelt) throughout spring, summer, and fall. Selection of this period was dictated by data availability. However, the most hydrologically important period (i.e. spring) was largely driven by winter conditions. Thus, the good simulations during spring reinforce the robustness of the simulations of hydrological processes over the winter.

10. Reviewer: *Ch. 3.4: The model adaptation mentioned in this chapter is hidden and not explained in the method part. Implement it earlier and bring it into a context.* Authors: A description of the sensitivity analysis is included as a new sub-section in the Material & Methods section of the revised manuscript.

11. Reviewer: *Discussion Line 555ff.: Here it is mentioned that low flow conditions are important for land use management in Prairies. At the same time, it is mentioned, that low flow has only low impact on nutrient transport. Though, why are low flow conditions important? This explanation isn't clear enough.*

Authors: The section mentioned by the reviewer specifically refers to drought conditions, which are important from a water resources perspective. The source cited in the manuscript [i.e. Fang and Pomeroy (2007)] state economic loss of the order of billions of dollars in the region due to drought, which makes it important from a land use perspective (e.g. summer fallow to save soil moisture). However, from a nutrient perspective, the smaller magnitude events account for very little of the overall export. This section of the manuscript has been rewritten to clarify this point.

References

Liu, K., Elliott, J. A., Lobb, D. A., Flaten, D. N., and Yarotski, J.: Critical factors affecting field-scale losses of nitrogen and phosphorus in spring snowmelt runoff in the canadian prairies, Journal of Environmental Quality, 42, 484-496, 10.2134/jeq2012.0385, 2013.

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Shook, K. R., and Pomeroy, J. W.: Hydrological effects of the temporal variability of the multiscaling of snowfall on the Canadian prairies, Hydrol. Earth Syst. Sci., 14, 1195-1203, 10.5194/hess-14-1195-2010, 2010.

Wheater, H. S., and Gober, P.: Water security and the science agenda, Water Resources Research, 51, 5406-5424, 10.1002/2015WR016892, 2015.