

Response to referee #3's comments

General Comment

This is a thorough and well written description of recent changes made to the WEBDHM model. Improvements in performance (RMSE if not necessarily Bias) as a consequence of these changes are evident through its application at two unvegetated alpine sites. Although the addition of process representations from the current literature, in a methodical and well cited manner, results in an improvement in performance, it is still the consequence of the application of parts of other models (BATS, SSiB3, SVAT, SNTHERM, CROCUS etc) to WEB-DHM. As a result, the improvement in performance is not a surprise. In short, this manuscript is an excellent technical report detailing changes to the latest version of WEB-DHM, but it falls short of being either a significant development in snow process modeling, or a thorough evaluation of why the additional process representations work so well. Consequently, I regret to suggest that the manuscript is not acceptable for publication in its current form. Suggestions to enable publication of a revised manuscript are listed below.

Answer: We agree that the realistic snow parameterization and validation dataset have been taken from published literatures but we confirm that the snow physics of WEB-DHM has been improved. Many models with realistic parameterization are basically developed for climate model or one dimensional land surface model and we coupled SSiB3 snow physics to 2-D distributed hydrological model which considers lateral flow distribution. From this point, it has a significant contribution; however, the model is validated for one dimensional only. We believe that spatial modeling of snow cover with poor snow physics may not provide correct results. Before application to 2-D, we would like to validate our system on point scale to understand the snow processes more accurately. The simulation results can be improved by calibration/optimization of parameters in old WEB-DHM but the model may not be able to simulate internal physics of snow processes well at all. Hence we believe the importance of physics is utmost. We appreciate the reviewer's question about the scientific contribution of this paper. However, scientific contribution includes innovative application of existing knowledge too. We would like to thank you for constructive comments/suggestions.

We intend to upgrade the existing manuscript quality fulfilling the requirements of a scientific paper with some uniqueness for publication in HESS. The revised manuscript will include the inter-annual variability of snow process, simulation at two more SnowMIP sites (Goosebay and Sleepers). In addition, the forest snow processes will be evaluated using one SnowMIP2 site. Sensitivity analysis for incremental process representation and its thorough evaluation will be made. Regarding the realistic parameterization to old model, the sensitivity of parameterization will be added in the revised manuscript which will give more insight. The old model with realistic albedo parameter will also be driven and results will be discussed in the revised manuscript.

Comments/Suggestions

1. Why is only one annual cycle used from each site when more data are available? Inter-annual analysis would strengthen the evaluation.

Answer: Inter-annual variability of snow processes will be evaluated in the revised manuscript.

2. Why are two alpine sites chosen for evaluation? It would be more appropriate to choose sites with different snow and hydrometeorological conditions (maritime, continental etc).

Answer: More sites (Goosebay and Sleepers River) will be added for evaluation in the revised manuscript.

3. Considering WEB-DHM is a distributed 'biosphere' model why were the impacts of forest canopies on snow processes not tested (as remarked on in the conclusions)? Data from sites used as part of SnowMIP2 may be available for such an analysis.

Answer: Impacts of forest canopies on snow processes will be tested in the revised manuscript with the use of SnowMIP2 dataset.

4. A more quantitative description is required of the improvement in model performance (e.g. in section 4.1 performance improvements are referred to as ‘a very acceptable manner’ or ‘remarkably less’, neither of which really help the reader determine the magnitude of the improvement).

Answer: Revisions will be made on this topic.

5. Although statistics for the entire winter are available in Table 3, and the plots visually show an improvement, a breakdown of the times in the winter where observed and modeled estimates of parameters diverge and converge would improve the analysis.

Answer: The manuscript will be revised by incorporating such analysis.

6. As the authors have such a thorough understanding of their model physics, a sensitivity analysis of change in performance through incremental addition of process representation would be an excellent way of critically assessing the impact of the changes made to WEB-DHM. This would be of high interest to the wider snow modeling community and would allow the authors to quantitatively demonstrate to what extent each improvement of the model gets it right for the right reasons.

Answer: Very good suggestion. We will exercise the sensitivity analysis for the incremental addition of process representation. The manuscript will be revised accordingly.