

# Response to referee #1's comments

## General comments

In this paper the authors carry out a competent comparison of a simplified one-layer snowpack model with inadequate representation of basic snow properties (snow density, thermal properties and albedo) with a 3-layer model with more realistic parameterizations taken from the published literature. An evaluation of the two versions shows (not surprisingly) that the model with more realistic parameterizations provides better simulations. This finding may be of interest to people still using WEB-DHM but there is nothing in the paper that advances understanding of snow modeling in terms of snow processes or validation data. The authors' claim that much of the improvement is related to the introduction of a 3-layer representation of the snowpack is debatable as 1-layer models with realistic parameterization of important snow processes provided comparable performance to WEB-DHM-S at the two SnowMIP sites evaluated in the paper. The authors' claim that the evaluation of WEB-DHM-S for two snow seasons at two mid-latitude European Alp locations represents a "benchmark for applying WEB-DHM-S in cold regions" is also debatable as the evaluation did not look at the ability of the models to capture interannual variability in snow cover conditions, did not evaluate the ability of WEB-DHM-S to simulate spatially varying snow cover (the 1-D, no vegetation, mountain SnowMIP runs represent some of the simplest possible cases), and did not examine the ability of the models to simulate snow cover properties over important land cover types such as taiga and tundra that make-up a large fraction of Northern Hemisphere snow covered lands. The paper is for the most part well-written and well-presented but there is nothing presented here that has not been published previously and there are no new insights provided by the authors of relevance to the wider snow modeling community.

**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.

The content of the abstract and introduction will be modified in the revised manuscript to address the uniqueness accordingly. The sentence with "benchmark for applying WEBDHM-S in cold regions" will be modified in revised manuscript to avoid debates.

## **Specific comments**

1. Abstract line 11: the claim that Col de Porte and Weissfluhjoch have different climates is a bit of a stretch. They are both located in the European Alps in relatively sheltered locations not subject to blowing snow. Is there any reason the authors chose not to run their models at the two other sites included in SnowMIP where the snow climate was indeed quite different (Goose Bay and Sleepers River)? Snowmelt runoff data were not available at the latter sites but most of the other evaluation data were.

**Answer:** Col de Porte (CDP) and Weissfluhjoch (WFJ) have different climate in the sense of meteorological dataset (details in section 3). We decided to validate the model with best forcing and validation dataset based on the previous literatures and found CDP

and WFJ would be the best candidate out of four SnowMIP sites. We will present the results for Goosebay and Sleepers site too in the revised manuscript.

2. Page 3483, line 6: The snow model review in Brun et al. (2008) would be an appropriate addition here.

Brun, E., Z-L. Yang, R. Essery, and J. Cohen, 2008: Snow-cover parameterization and modeling. Chapter 4 in: Snow and Climate: Physical Processes, Surface Energy Exchange and Modeling, R.L. Armstrong and E. Brun (eds.), Cambridge University Press, Cambridge, UK, 222 pp.

**Answer:** The reference will be added in the revised manuscript.

3. Page 3483, line 22: The term “simple snow models” is not precise. In this context you are specifically referring to models that represent snow as a single layer.

**Answer:** The term “simple snow models” will be replaced by “single layered models”.

4. Page 3483, line 23: The phrase “capture the real snow physics.... thaw cycles” is unclear. I think what you are trying to say is “1-layer representations of a snowpack have difficulty capturing diurnal freeze-thaw cycles which results in errors in the simulation of snow surface temperature and the timing and amount of snowmelt”.

In this section you also need to recognize the sensitivity of the models to uncertainties in the forcing and initial condition data. Uncertainties in precipitation phase in particular can have a strong impact on performance as seen with the Sleepers River simulation in SnowMIP.

**Answer:** The manuscript will be revised according to the comment. Discussion on sensitivity of the model to uncertainties in the forcing and initial condition data will be added.

5. Page 3484, lines 1-3: This claim is debatable as the VIC model has a 2-layer representation of snow cover.

**Answer:** The manuscript will be modified to avoid debate.

6. Page 3484, lines 17-20: Please justify why only mountain sites were used from SnowMIP and why the evaluation was restricted to alpine environments. This would also be the place to indicate what new insights the authors expect to obtain from this rather limited 1-D evaluation.

**Answer:** Evaluation will be made to all four SnowMIP1 sites and one SnowMIP2 site which will provide more insight to the variability of snow processes in different climates.

7. Page 3494, line 11: You should indicate how precipitation amount and solid fraction were measured.

**Answer:** The detail of measurement of precipitation and solid fraction will be presented in the revised manuscript.

8. Page 3494, line 12: Change “amount, the snow/rain index” to “amount and solid/liquid fraction”

**Answer:** Revision will be made.

9. Page 3494, line 27: It is incorrect to describe WFJ as drier than CDP when it records a larger amount of winter precipitation (Table 2). It is also incorrect to classify this as a “cold” climate when the mean air temperature over the snow season is -2.9C.

**Answer:** Dry in the sense of average relative humidity and cold in the sense of average air temperature at WFJ site relative to CDP site.

10. Page 3496, lines 4-5: The phrase “owing to strong solar radiation... melting temperature” is redundant.

**Answer:** Redundancy will be omitted in the revised manuscript.

11. Page 3496, lines 12-13: Why is SWE overestimated by both models? There are a number of possible explanations: e.g. Is the precip input too high? Is the solid/liquid fraction incorrect? Is sublimation underestimated? Is bottom-melt underestimated?

**Answer:** More explanation will be presented in the revised manuscript.

12. Page 3496, line 15: What constitutes “very acceptable” performance?

**Answer:** Qualitative expressions will be removed.

13. Page 3497, line 3: Where are the results for the length of snow cover season shown?

**Answer:** Here, length of snow cover season corresponds to snow depth simulation.

14. Page 3497, line 17: Change “An accurate” to “A realistic”

**Answer:** Revision will be made.

15. Page 3498 lines 4 and 14: Be careful of qualitative expressions such as “is commendable” and is “remarkably improved”.

**Answer:** The qualitative expressions will be tone downed.

16. Page 3498 line 21-22: Where does this cold bias come from? Brown et al. (2006) included an extensive discussion of this same problem for CLASS and concluded that there were deficiencies in the boundary layer scheme under highly stable conditions. Is this the same problem with WEB-DHM?

**Answer:** The reason for cold bias will be discussed more detail in the revised manuscript.

17. Page 3499, lines 15-19: The fact that your albedo bias is identical to CLASS (a 1-layer model) does not provide any insight into possible reasons for this. The underestimation in CLASS was related to the albedo of new snow being too low (Brown, 2006). WEB-DHM appears to have the same problem in addition to an overly-rapid decrease in albedo following the deposition of new snow (snow aging too rapid?).

**Answer:** The main reason is that the albedo of new snow is simulated too low than the observed one. Observed albedo is around 0.95 but the simulated maximum albedo is 0.84. In the revised manuscript, we will discuss more on this topic following the sensitivity analysis.

18. Page 3500, line 13: Should be Table 3 not Table 2.

**Answer:** Revision will be made.

19. Page 3500, line 26: Is the improvement that remarkable given the unrealistic representation of snow cover in WEB-DHM?

**Answer:** Sensitivity analysis for realistic parameterization for original WEB-DHM will be made in the revised manuscript.

20. Page 3501, lines 3-4: There are other snow albedo datasets available if the authors care to look for them. However, before embarking on a parameterization exercise the authors should heed the conclusion of Etchevers et al. (2004) from the SnowMIP evaluation; the best snow albedo performance was obtained by models where albedo was parameterized based on snow grain characteristics as well as snow age.

**Answer:** We will look this suggestion.

21. Page 3501, line 7: This evaluation falls far short of providing any sort of benchmark for the application of WEB-DHM-S in cold regions. For hydrologic applications the spatial distribution of snow accumulation and melt are the key processes and neither of these were evaluated in this paper.

**Answer:** The manuscript will be revised by avoiding debatable texts.