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Hydrology and Earth System Sciences Discussions

Interactive comment on "Improving the snow physics of WEB-DHM and its point evaluation at two SnowMIP alpine sites" *by* M. Shrestha et al.

Anonymous Referee #1

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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-latitudinal 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.

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.

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.

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.

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.

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

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.

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

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

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.

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

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?

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

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

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

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

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?

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?).

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

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

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

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was parameterized based on snow grain characteristics as well as snow age.

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.

Response to HESS specific evaluation criteria:

1. Does the paper address relevant scientific questions within the scope of HESS?

The paper does not directly address any scientific questions as it did not pose any.

2. Does the paper present novel concepts, ideas, tools, or data?

No. The parameterizations and evaluation data are taken from the published literature.

3. Are substantial conclusions reached?

Yes but mainly because of the unrealistic treatment of snow cover in WEB-DHM, and then the conclusions are really only relevant to WEB-DHM users (or land surface schemes with equally poor representations of snow cover).

4. Are the scientific methods and assumptions valid and clearly outlined?

Yes

5. Are the results sufficient to support the interpretations and conclusions?

To some extent. The authors do not discuss the sensitivity of the snow models to errors and uncertainties in meteorological driving data and to the specification of initial conditions and site factors. It was also unclear how much of the improvements in snow simulations came from the 3-layer representation and how much came from improved parameterizations of snow density and albedo.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

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Yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes for the proper credit. There is no evidence of new or original contributions in the paper.

8. Does the title clearly reflect the contents of the paper?

Yes

9. Does the abstract provide a concise and complete summary?

Yes

10. Is the overall presentation well structured and clear?

Yes

11. Is the language fluent and precise?

Yes. The authors may want to tone down some of the more exuberant qualitative expressions used to describe the improvements in model performance e.g. "commendable", "remarkably improved"

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

No

14. Are the number and quality of references appropriate?

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Interactive Comment

Yes with the addition mentioned above in the specific comments.

15. Is the amount and quality of supplementary material appropriate?

n/a

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 3481, 2010.