Response to the comments of the Reviewers

Following major revisions were made to address the general comments of all the Reviewers.

Major Revisions

1. Two open sites; Goose Bay (GSB) and Sleepers River (SLR) of SnowMIP and one open/forest site: Hitsujigaoka of SnowMIP2 were added for simulation. (Section 3.3, 3.4 and 3.5 and 4.0)
2. Inter annual variability of snow processes were studied at Goose Bay (1969-84) and at Col de Porte (CDP) by adding simulation for 1996-97.(Section 4.1)
3. Sensitivity analysis for incremental process representation to WEB-DHM for two snow seasons at CDP site and one season at WFJ were added (Section 4.5).
4. Impact of forest canopy on snow processes at Hitsujigaoka forest site was studied.(Section 4.6)
5. Table 2 (Meteorological characteristics of study sites) and Table 3 (BIAS and RMSE for SnowMIP1 sites) were updated. Table 4 (BIAS in simulating the first, maximum, minimum in mid season, one prior to the last and last SWE observations at CDP, WFJ and SLR sites) and Table 5 (Different set of simulations for incremental process representation) were added.
6. Previous figure 2 was removed and figures 2 to 10 were updated and modified. Figure 2 (Snow depth, SWE and density at CDP, WFJ and SLR sites), Figure 3 (Snow depth at GSB site), Figure 4 (Surface temperature at CDP, WFJ site), Figure 6 (Snowmelt runoff), Figure 7 and 8 (Incremental process representation), Figure 9, 10 and 11 (Snow depth at Hitsujigaoka site) are the major figures added/updated.
7. The time-slice evaluation of the model in simulating the first, maximum, minimum in the mid season, one prior to the last and last SWE observation at CDP, WFJ and SLR site are presented. (Line 423-435; Section 4.1, Table 4)

Response to Reviewer # 1

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: Revised. (Line 20-22).
We presented the simulations at Goose Bay, Sleepers River, Col de Porte (1996-97) and Hitsujigaoka open and forest site 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 was 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: Revised with the use of "single layer snow models" in stead of "simple snow models" (Line 62-63).

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: Revised. (Line 62-63, 67-70)

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 was revised. (Line 71-75)

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 was made to all four SnowMIP1 sites and one SnowMIP2 site as said in the beginning.

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

Answer: Revised. (Line 297-299)

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

Answer: Revised. (Line 287)

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: The comparison of site meteorology at WFJ and CDP was removed.

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

Answer: Redundancy was omitted in the revised manuscript. (Line 315)

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: This overestimation is due to the failure in capturing the rapid decrease in SWE during 21 January to 27 January. The uncertainty in the precipitation phase is not a major in this case and the reason may be the snowmelt due to rapid increase in albedo by dust and fallen leaves. (Line 401-404)

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

Answer: Qualitative expressions were removed in the revised manuscript.

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

Answer: Actually, the length of snow cover season corresponds to the length of snow depth simulation. But this sentence is removed in the revised manuscript to avoid the confusion.

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

Answer: Revised. (Line 387)

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

Answer: Qualitative expressions were removed in the revised manuscript.

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 cold bias is associated with the deficiency in Monin-Obukhov similarity theory to calculate the turbulent fluxes in a highly stable condition and the uncertainty in the roughness length of the snow surface. (Line 475-477)

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 behind this bias is that the observed albedo for new snow is around 0.95 whereas the simulated maximum albedo is 0.84. (Line 490-492)

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

Answer: Revised. (Line 512)

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 was made in the revised manuscript. (Section 4.5)

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: The sentence was revised. The current albedo scheme needs improvements in accounting the effect of snow type and dynamic evolution of grain size too. (Line 601-602)

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: Revised. (Line 609-613).

Response to Reviewer # 2

Minor comments

2.2.1 The information on layer subdivision is largely repeated from 2.2

Answer: Revised by removing the section "Sub layer division"

2.2.2 Give some reference on how the surface fluxes are calculated. This will have a large influence on the surface temperature. How is the grain size used in the radiation extinction coefficients specified? Equations (11) and (12) for the canopy and surface snow layer temperatures contain another unknown: the snow layer 2 temperature. Show how the full system of equations is solved.

Answer:

Surface fluxes are calculated using the formulations of SiB2 (Sellers et al., 1996). (Line 170-172)

Sellers, P. J., Randall, D. A., Collatz, G. J., Berry, J. A., Field, C. B., Dazlich, D. A., Zhang, C., Collelo, G. D. and Bounoua, L.: A revised land surface parameterization (SiB2) for atmospheric GCMs, Part I: Model Formulation, J. Climate, 9, 676–705, 1996.

Grain size diameter is specified as a function of density following Anderson (1976). (Line 189-190)

We are very sorry to make confusion. $T_{sn}(Z_2)$ is not an unknown parameter in the equation 12. Equation 12 is revised.

$$\left[-\frac{\partial R_{nsn}}{\partial T_c}+\frac{\partial H_{sn}}{\partial T_c}+\frac{\partial \lambda E_{sn}}{\partial T_c}\right]\Delta T_c+\left[\frac{C_v\times Z_3}{\Delta t}-\frac{\partial R_{nsn}}{\partial T_{sn}(Z_3)}+\frac{\partial H_{sn}}{\partial T_{sn}(Z_3)}+\frac{\partial \lambda E_{sn}}{\partial T_{sn}(Z_3)}+K_{eff}\right]\Delta T_{sn}(Z_3)$$
$$=R_{nsn}-H_{sn}-\lambda E_{sn}+G_{pr}-K_{eff}\times\left[T_{sn}(Z_3)-T_{sn}(Z_2)\right]^{t-\Delta t}+\frac{Z_3\times H(Z_3)}{\Delta t}$$
$$-\frac{C_v\times Z_3\times\left[T_{sn}(Z_3)-273.16\right]}{\Delta t}+\frac{f_{ice}^{\;t-\Delta t}\times M_{snow}^{\;t-\Delta t}(Z_3)\times h_v\times\rho_w}{\Delta t}$$

$T_{sn}(Z_2)$, $T_{sn}(Z_1)$, T_g and T_d are the variables with slow change and are solved explicitly using a forward numerical scheme. (Line 204-205).

The detail of the solution for Equations 11 and 12 is provided in the revised manuscript. (Line 213-220 in the revised manuscript)

2.2.3 It is not clear here how IFj and Rj are calculated.

Answer: IF_j (ms^{-1}) = $min\ (O_j, P_{avs})$. O_j is the outflow flux rate which is the liquid water drained to the underlying layer as the total liquid water in layer exceeds its liquid water holding capacity (C_r). Liquid snow mass fraction, f_{liq} = (1-f_{ice}) is used to calculate the total amount of liquid water. P_{avs} is the pores available in the layer. R_j is calculated as the difference between IF_j and O_j. (Line 230-235 in the revised manuscript)

2.2.5 Snow albedos are given for direct and diffuse illumination in visible and near infrared bands. How are they used, since these radiation components are not available in the forcing data?

Answer: The proportions of the various spectral and angular fractions of short-wave incoming radiation are estimated from formulations used in SiB2. (Sellers et al., 1996)

Sellers, P. J., Randall, D. A., Collatz, G. J., Berry, J. A., Field, C. B., Dazlich, D. A., Zhang, C., Collelo, G. D. and Bounoua, L.: A revised land surface parameterization (SiB2) for atmospheric GCMs, Part I: Model Formulation, J. Climate, 9, 676–705, 1996.

4.2 Are the UDG or snow pit measurements of snow depth used in calculating the error statistics? What is the “desired accuracy”?

Answer: UDG is used to get the error statistics for snow depth and snow pit measurements are used to get the error statistics for snow density. “Desired accuracy”, the qualitative expression was omitted in the revised manuscript

4.4 Figure 8 does not show energy conservation, which should be exact in both models.

Answer: The text was removed in the revised manuscript.

4.5 Against what criteria was the fresh snow albedo calibrated?

Answer: Manual calibration of fresh snow albedo was done to minimize the difference of simulated and observed snow depth.

Response to Reviewer # 3

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 were evaluated in the revised manuscript for Col de Porte (1996-97, 1997-98) and Goose Bay (1969-1984).

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: Two more open sites (Goose bay and Sleepers River) of SnowMIP1 and one open/forest site (Hitsujigaoka) of SnowMIP was 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 was tested at Hitsujigaoka forest site of the SnowMIP2. This site is selected to meet the different hydro-meteorological condition (maritime). (Section 3.5 and Section 4.6)

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: Qualitative expressions were removed in the revised manuscript.

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 was revised by incorporating the time slice evaluation of model in simulating the first, maximum, minimum in the mid season, one prior to the last and last SWE observation at CDP, WFJ and SLR site. (Line 423-435, Table 4)

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: Sensitivity analysis for incremental process representation was added in the revised manuscript. (Section 4.5)