

Interactive comment on “Modelled sensitivity of the snow regime to topography, shrub fraction and shrub height” by C. B. Ménard et al.

Anonymous Referee #1

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The paper by Ménard et al. discusses the influence that shrub cover and topography have on snow accumulation and melting dynamics. This analysis is motivated by the fact that shrubs are generally expanding on higher latitudes (and altitudes), mainly because of warming climate, and by the current lack of sensitivity studies dealing with the effects of shrub expansion on snow dynamics. A sensitivity test is run by introducing a new model (3SOM) which improves current energetic descriptions of Land Surface Models at the sub-grid scale, validating it against point data from a small (1 km X 1 km) valley, and by using it (together with an accumulation model, DBSM) to assess distributed SWE accumulation/ablation variability as a function of topography and shrub cover.

The topic discussed is very relevant, and this is well highlighted in the Introduction.
C228

Here, they are also well documented the reasons for improving current energetic descriptions of LSM models at the very local scale. The proposed model (3SOM) focuses mainly on energetic processes, and has to be coupled with an additional model (DBSM) in order to take into account mass inputs, sublimation and/or wind redistribution. However, this coupling is partial and “sequential”, since DBSM defines the initial conditions for 3SOM, and any coupled effect (such as winter melting and/or spring redistribution and mass inputs) seems to be neglected. Although this hypothesis is clearly stated in the text (see e.g. line 25 page 227 or lines 2-6 page 234), and it is intention of the authors to develop a full coupled approach, the same looks quite a relevant assumption to me, if I have understood correctly.

On the one hand, it is reasonable since the model is used in a sub-arctic tundra valley, where winter mass losses due to melting should be negligible, on the other hand more evidences should be presented and discussed to assess whether wind redistribution and snowfalls are negligible during spring, when melting is the dominant process, but not the only one. This could help also to explain some models underestimations in SWE in Fig. 3(a) at the end of the melting season, which could be also due to these neglected processes, and not only to cold temperatures.

In my opinion, results and discussions are interesting, and therefore I would suggest the manuscript for publication, after some additional elaborations. In particular, it could be interesting to discuss in a deeper way the implications of neglecting the full coupling between the accumulation and the melting periods, and to assess quantitatively if neglected mass fluxes are relevant or not (e.g., which is the average air temperature during winter? Is there evidence of snow events in spring? etc.).

Minor comments:

1. Abstract: I would suggest reorganizing the second part (the one dealing with the results of the study) since it seems to be a bit dispersive;
2. Line 9, page 225: I think it could help adding some quantitative references to be

compared with 3 Wm^{-2} per decade, since at this stage it is difficult to understand how much relevant this rate is, if it is reported all alone;

3. Line 20, page 225: please define “snow-holding capacity” since this process seems to be quite relevant in this paper;

4. Sites and Data (Section 2): maybe consider inserting a map with the location of the GB site, and improving current Figure 1 with contours (or the elevation of relevant points). In the same Figure, four crosses are visible, which should refer to four instrumental sites (1 to 4, line 12 page 229). Nonetheless, at line 1, page 229, it is stated that station ‘3’ is actually composed by two different stations, so that the number of stations (5?) does not match with the number of crosses (5 stations, but only 4 crosses). Please clarify. I would also appreciate if you could provide some specifications about instrumental resolutions and accuracies;

5. Section 3.1: according to my opinion, current Appendix could be more significant if incorporated in the main text, namely in this Section;

6. Section 3.2: since snow plays an important role in this analysis, I think more details are due about the parameterization used to model snow settling, albedo and phases dynamics in the snowpack;

7. Lines 17-19, page 233: I would consider adding a quantitative comparison between the outcomes reported here and those by Bewley et al. (2010), such as maximum differences between the two predictions and the data etc. In this way, it would be possible to understand the improvements of your model without looking for the paper by Bewley et al. (2010);

8. Figures: as a general suggestion, I would consider reporting more labels in the Figures (such as in Fig. 9 and 10).

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