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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Subgrid parameterization of snow distribution at a Mediterranean site using terrestrial photography" *by* Rafael Pimentel et al.

Anonymous Referee #1

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In this paper the authors use terrestrial photogrammetry of a small area (30 by 30 m) to measure snow depth (h) and the associated snow cover fraction (SCF). They then use the results to define accumulation and four styles of depletion (DC) curves. These curves are ingested into a snow melt evolution model in a way that updates the fraction of area over which the model is applied. The results are found to improve the model performance.

This is a clearly written paper and a nice tidy study. It has two main conclusions which need to be explored a bit more. The first is that there were 4 styles of melt over the domain and these were a function of the antecedent history of snowfall as well as the time of year. That is an interesting and potentially useful finding, but it was derived for a near postage-stamp sized domain. So the question is "How robust and general is the finding?" Are the four styles universal for the Sierra Nevada, all

snow landscapes, or just the local area? The current set of conclusions are really a reiteration of the abstract. . .instead the conclusions should be about these styles and what they might mean in a more general way. That would make the conclusion relevant to snow researchers not working in the Sierra Nevada. It would also greatly strengthen the paper if the authors had any data from nearby (but not in the training area) domains that could be used to validate that the DC styles have at least local widespread validity. I have reason to think they might.

The second finding is that the ingestion of the DCs into the melt model improved the model. This is not totally novel, nor is it surprising, but it is useful. I would have liked to see a bit more quantitative assessment of the extent of the improvement. The metrics are all there, but, for example, how much better would the improvement have been if a sigle style of DC was used for all cases.

So in summary, I find this paper worth publishing, but I would ask that the authors revise the text in ways that address in greater detail the styles of melt depletion observed, whether those styles can be extended beyond the training domain, and if so, how far, and delve a little deeper into just how much improvement the ingestion of the DCs made to the model (for example, what if the model was just adjusted with a fixed linear depletion. . .would it compete well with the 4 styles?).

Detailed comments follow:

Page 1, Line 31: I disagree: cold northern regions also have extremely heterogeneous snow covers due to both wind-drifting and canopy interception.

Page 2, Line 5: There could be considerably more discussion in the text on the microtopography of the domain. With respect to the above comment, see: See Sturm & Holmgren (1994). Effects of microtopography on texture, temperature and heat flow in Arctic and sub-Arctic snow. Annals of Glaciology, 19(1). You will see there that northern landscapes also have heterogeneous snow.

Page 2, line 26: Here the authors speak of processes. . ...this is a good lead in to what is needed in the expanded discussion about the 4 DCs: what processes of melt (and albedo etc.) are different in each curve and why? Perhaps a table of these processes differences would be useful. They have made a good start on this in the current text, but have not really made a succinct summary of the styles and the reasons for them.

Page 5, Equation 2: It is not totally clear from the text (or the figure) how these regressions of mean depth vs. pole depth were derived. It is not even clear how many poles were in view and measured. Some additional details would help here.

Page 6, Line 11: In the equation precipitation is R not P. Correct.

Page 8, Lines 10 to 20: Good. More of this is needed.

Page 10, Line 15 and Page 12, Line 5: These assertions have no backup. . .no evidence that they are true. It would be very helpful to show that this is the case. . .or at least that there is some evidence it is true. Just stating so doesn't make it so.

Figure 5: Axis Labels are too fuzzy and small to read. Also, add a table (or schematic) that summarizes how the 4 melt curves differ and why.

Figure 6: The decision diamonds in the figure don't seem quite right. For example, what if the snow is >0.6 m and older than 30 days? This figure needs some more thought and revision.

Figure 9: Nice figure. I think looking at it, some of the physical reasons for the 4 DC styles are suggested.

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