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The Cryosphere Discussions

Interactive comment on "Inhomogeneous snow distribution and depletion patterns at grid scale in a shallow snowpack region" *by* H. Li et al.

Anonymous Referee #1

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In this paper the authors use snow water equivalent (SWE) data from a 100 by 100 m plot to examine how patterns in the SWE vary before and after the melt has begun. They model the snow cover over this plot using SnowModel, and then use that model to examine the impact of changing the solar radiation input on the SWE patterns.

The paper as written is more like a report and data exploratory analysis than a mature paper with a central thesis around which data is collected, analyzed and discussed. It is also an oddly structured paper with a far too slim and incomplete data results section, and a rambling exploration of spatial trends. Lastly, the authors work in a extremely thin snow cover, yet do not discuss how their results apply to other areas and other snow classes.

Taking this last point first, Figure 1 of the paper shows a gently sloping plot with what

looks like a grass cover, but with some quite large shrubs in the far corner of the plot, and even larger shrubs outside the plot. The local relief is potentially critical in developing snow patterns (particularly for shallow snow), but little discussion is presented concerning the ground cover characteristics. The general snow cover over the plot appears to range from 6 to 18 mm of water equivalent. If the area is not windy, that would imply a snow depth of 2 to 6 cm of snow; if windy perhaps 1 to 4 cm of snow. I can only imagine that even at peak snow depth, vegetation was protruding from the snow, if not actual hummocks. The authors do not remark on this, but it is important. A minor but important corollary: what was the effect of human trampling to make the measurements on such a thin snow pack? Surely it created a grid of compacted snow or bare ground that had ramifications for solar radiation?

Allied to this point is the simple question of how the SWE was determined. Presumably 121 cores were taken each day. What was the corer, what was the protocol for coring (core-based SWE measurements in shallow snow are notoriously hard to make with accuracy) and what was the quality assurance protocol?

Which brings me to my second criticism: the paper is about snow patterns, yet the authors do not present SWE patterns at all (with Figure 7 being the one exception). I would expect to see gridded maps of the SWE at various times or stages of melt in the result sections. The metrics that are presented (mean SWE, SCF, SDV and CV) are secondary value. . . not results. They are derived from the results. The authors need to show the results first, then these derived quantities. Also, I would question if these metrics are really "patterns". Even the PDFs shown in Figure 4 are not patterns, and once again, are not real data. They are derived (and higly smoothed) curves fit to the data. So in short, the authors need to beef up their results section with real and solid results, THEN start to explore those results using derived statistics and distribution curves.

The closest the paper comes to dealing with classical patterns is when the PAF metric is introduced. Being unfamiliar with this metric, and finding no reference to it (did the authors invent this or is it an accepted statistic?) I found myself having to work through a series of made up examples to try to understand what a PAF of 0.5 vs. 1.5 meant. The authors need to lay out what this metric tells them and us about pattern stability, and also why it bears the name "Periodical cumulative. . .". It may actually be a useful metric, but it definitely needs more discussion.

Moving on to Section 3.1: Relationships between SWE patterns and other aspects of the snow: In order for this section to be robust, we need to know the accuracy of the SWE measurements (see above), and in addition, what we might expect the relationship to be between variations in SWE over the plot, and the other metrics. Liston (2004) has explored these. Perhaps the discussion would be better informed by first thinking about what should happen where there is more and less snow on the landscape, then examining whether in a thin snow environment our expectations were met (or why not).

The cumulative weight of the comments above (along with a few additional points listed below) is that this paper seems misdirected. The authors collected what appears to be good data set on a small plot in a very thin snow regime. Within those data were patterns of distribution related to something. . . .perhaps the underlying microtopography, perhaps the radiation regime, perhaps penetration of light into the snow. Exploring the patterns and asking what they tell us about thin snow on a grassy landscape is useful, but first and foremost, the authors need to present the data themselves so we the readers can assess the source of the discussion, then the authors need to place their data in the context of the larger world of snow (deeper snows, windier snows, snow on smoother or rougher substrates). They also need to tighten up the analysis and discussion sections, focusing on a series of questions.

Comments keyed to the text

Page 4182: The goal is to show patterns persisted. I am not sure that correlation of distribution curves (PDFs) is enough to demonstrate this. There are other metrics out

in the literature that do this better, like MDE.

Page 4182, Line 27: Usually sublimation is reduced at lower temperatures. What is the evidence for this statement (or better still, what was the change in the pattern)?

Page 4183, Line 5: Good. . . .begins to get at differences between shallow and deeper snow packs.

Page 4183, Line 24: This is logical, but it would be even better if it could be shown with data.

Page 4186: I found myself wondering about light penetration through thin snow to dark grass underneath, and how this could easily confound this whole train of analysis. Does SnowModel deal with the extinction coefficient?

Interactive comment on The Cryosphere Discuss., 6, 4171, 2012.