Reply to Anonymous Referee #1

*The manuscript has been major revised benefiting from the anonymous referees. Many thanks for your suggestions. In the revised version, we focused on the meteorological controls on snow distribution evolution and analyzed how these factors influence the modeling, so that the paper direction is more clear and the collected data could support our conclusion more efficiently. The new title of the manuscript is named '**Meteorological controls on snow distributions at grid scale in a shallow snowpack region**'. The other major revisions include an updated modeling of snow distribution, and some parts were modified or deleted for a more clear description of our working. In our revised manuscript, the statement about similarity comparisons between different 'patterns' have been compressed into a few paragraphs. We answered the comments as following,*

1. **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.

Reply:

From the comments, we known it is difficulty to find a fixed distribution pattern for a shallow snowpack region from our limited data. However, analyzing the influence of meteorological factors and considering it in modeling spatial distribution processes of snow are more valuable. So we changed the direction of the manuscript. More discussion and conclusion are about the influence of meteorological factors on snow distribution change and how to consider this influence in the inhomogeneous modeling in sub-grid scale.

2. **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.**

Reply:

We have investigated the heights of shrubs over the 100 sub-grids in the field. Different land covers play important roles on the snow distribution evolution, we have emphasized this point in the revised manuscript. The influence of relief on snow distribution change was discussed.

3. **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.**

Reply:

Yes! The snow cover is shallow so the vegetation can disturb the snow cover consistent. It is very important in modeling the snow distribution processes in a shallow snowpack. We have add this factor in describing the snow distribution.

4. **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?**

Reply:

Surely the field investigation disturbed the snow cover when we walk along a fixed route. However, the measured results is really not influenced. We measured snow properties on ***both sides*** of a fixed route. Because of an enough distance from the sampling point, the measured snowpack ***is not disturbed*** for our walking. In other words, if we investigate the snowpack in very close intervals such as 10cm (of course it is impossible), the measured results must be influenced by the human trampling. In real case, the statistic results is from the sampling points in intervals of 10m, so the human trampling almost not influenced the measurements if we keep avoiding the sampling points.

We have illustrated it in our revised manuscript.

5. **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?**

Reply:

Yes, there are 121×4 samplings of SWE each day (in each sampling points, we measured snow properties at four different directions). We used an instrument named SNOWFORK to measure the average snow density, and obtain the snow depth by rule. Then we determined the SWE by the average density and snow depth. The directly measured value of SNOWFORK is the permittivity of snow, then snow density and moisture were determined by the permittivity. Snowpack almost was not disturbed because of the fork of SNOWFORK is very thin. For a better illustration for reader, we have described these points in detail in our revised manuscript.

6. **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 highly 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.

Reply:

We discussed the "patterns" over different seasons in the old manuscript. As results, obvious patterns exists in the melt season, however these patterns is not exists in the accumulation periods. Snow patterns is difficult to be concluded over the whole period. So it may be not easy to give a clear description of the 'patterns'. To overcome this, we move our concentration to a better description of modeling snow distribution evolution, and not try to find a fixed 'patterns' over the whole snow season. We thought a better modeling method including meteorological factors is more valuable for snow modeling in shallow snowpack region. On the other hand, we known giving an direct results is very important. So in our revised manuscript, the real snow distribution evolution was illustrated by a series of images. We expect a better description of snow distribution at grid scale for readers.

7. **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.**

Reply:

In our previous manuscript, we use a metric named PAF to describe the period snow distribution change trend. We known the introduction to this metric is too simple, so a more detailed illustration was given in the revised version.

8. **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)**

Reply:

We have illustrated the accuracy of the SWE measurements in the revised manuscript. Please find the answers to the doubt in the above 5th reply.

9. **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).**

Reply:

Yes! It is necessary to analyze more different cases especially with different precipitation amounts. We have noticed Liston(2004) developed an excellent subgrid snow modeling method at the large scale. In our revised manuscript, we have discussed these opinions for a better illustrating. And, we added more scenarios including different snowfall increases in the sensitive analysis.

10. **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.**

Reply:

It is excellent suggestion. In our revised manuscript, we have adjusted the structure of the manuscript, present more origin data and supplement more analysis.

11. **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.**

Reply:

We known it is difficulty to find a fixed snow patterns over the whole snow season. In this shallow snowpack region, the relationship between SWE and SCF have a similarity in melt season, however this similarity is not exist in the pre-melt season. More further, a local 'pattern' is not very useful for different modeling purposes. So in our new manuscript, we examine the influence of different meteorological factors to the snow distribution change, and try to distinguish the roles of meteorological factors in changing snow distribution.

12. **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)?

Reply:

The snow mass change was modeled by the SNOWPACK model. This point has described in detail in the revised manuscript.

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

Reply:

More modeling results have been supplemented to demonstrate it.

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

Reply:

An investigated results were given there in the new revision.

15. **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?**

Reply:

Yes, SNOWPACK model (Lehning, M. et al.) use an extinction coefficient to model the solar radiation into snowpack.

References:

Liston, G.E. (2004). Representing subgrid snow cover heterogeneities in regional and global models. *Journal of Climate*, *17*, 1381-1397.

Lehning, M., Bartelt, P., Brown, B., & Fierz, C. (2002). A physical SNOWPACK model for the Swiss avalanche warning. Part III: meteorological forcing, thin layer formation and evaluation. *Cold Regions Science and Technology*, *35*, 169–184.