

Reply to Anonymous Referee #2

Many thanks for your detailed comments and suggestions. By these opinions, we know it is very difficult to find a spatial pattern of snow distribution using limited investigated data. So we focused our study on how meteorological factors influence the snow distribution and examining how to consider these influences in modeling inhomogeneous snow spatial distribution. 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. The study attempts at finding general spatial patterns of the accumulation and ablation of SWE, which can be used for modeling purposes. More specifically the questions are: What snow distribution and depletion patterns exist at the grid scale in a shallow snowpack region? How do meteorological factors influence these patterns during different snow seasons?

I do think that these questions are highly relevant and well worth a study, but I am afraid the present paper does not give a basis on which future studies can be built. The main reason is the limitation in data, both spatially and temporally, that, in my view, prevents the answering of the two questions above. Also, the presentation of methods and results are unclear and imprecise and I find it hard to draw any conclusions. That said, I do believe the collected data contains information that can be put to use and reported, as you have collected a time series of the spatial frequency distribution of snow parameters (SWE, SD, snowmelt). Such data are rare and therefore valuable.

Reply:

The direction of the revised manuscript has been changed to analyzing the meteorological controls on snow distribution evolution. Benefiting from the referee, we thought a more concentrated and detailed analysis in examining the existing modeling method in grid scale is more valuable, instead of finding a new 'patterns'. The main reason is that we understand the lack of observation data. It is not enough for developing a fit snow patterns over the shallow snowpack in different regions.

2. The terminology is very imprecise which leaves the reader quite confused and struggling to understand what is analyzed.

Reply:

In the revised manuscript, we have given more detailed illustration in analyzing.

3. The figures need to be labeled a), b), etc, for easier reference when there are

more panels in a figure.

Reply:

Thanks! Done.

Detailed Comments.

4. p.4172, l.5. The “contrast” is not very clear

Reply:

We have focused the question on understanding meteorological controls on snow distribution changes. It has been modified correspondingly.

5. p.4172, l.11. Reformulate this point 1), it is not very clear

Reply:

In the previous manuscript, we give the results qualitatively. It not clear so readers maybe confused in it. As we have modified, this part has been deleted from the abstract.

6. p.4172, l.22. You must define the grid scale, grids come in many sizes.

Reply:

Yes! In our study, the scale is 100m*100m. We have added some description of contrasting between different scales from some literatures.

7. p.4173, l.3. Liston (2004, J. Clim.17) has a nice discussion on scale and processes regarding the spatial variability of SWE.

Reply:

Thanks! Liston (2004) discussed different influence on the spatial variability of SWE including the scale choosing, micro-topography and so on. It is helpful to direct us in exploring the rule of describing inhomogeneous snow processes at small grid scale. In the revised manuscript, we have discussed it.

8. p.4174, l.1. I do think the spatial variability of precipitation need to be incorporated as a source of the spatial variability of SWE.

Reply:

Yes! It is very important. Different random fields of precipitation were simulated and input into modeling snow distribution. The results were used for analyzing the role of precipitation in forming spatial variability of snow distribution.

9. p.4174, l.12. This statement needs to be referenced.

Reply:

Thanks! Done.

10. p.4174, l.18. Why is this likely?

Reply:

In the RESULTS part, we give the reason. It maybe unreasonable to give this statement. So we moved this statement to DISCUSSION part for a better

structure.

11. p.4177, l. 10. You are forcing a spatially smooth precipitation field by restricting the ratio to be between 0.8-1.2. How does this bear on the analysis of pattern from the modeled fields?

Reply:

Good suggestion! In the revised manuscript, the precipitation has been regarded as random field and different scenarios were simulated for sensitive analyses.

12. p.4178, l.8. The heading of this subsection is Spatial Accumulation Factor, whereas in the text it is called Periodical Accumulation Factor. I have three comments to this: i) I do not quite understand why it is called "Periodical", ii) I think Spatial Accumulation Factor is a better name, and iii) one should not call the same thing by two names.

Reply:

Yes! 'Periodical' is for contrasting the similarity of accumulated snow distribution at different periods. It may be confused for many readers. Only Spatial Accumulation Factor was used in the revised manuscript.

13. p.4178, l.14: : temporal similarity between

Reply:

Thanks! Done.

14. p.4179, l.15. Are these PDFs calculated for each day, i.e. is a temporal notation of the parameters missing?

Reply:

There is a mistake. The counting of SWE distribution is not depended on the PDF.

15. p.4180, l.1. The First sentence in section 3.1 needs reformulation. I also think the comments on Fig. 2 are imprecise, i.e. the SDV a similar trend as SCF. It doesn't look that way.

Reply:

This sentence has been revised as "The SDV also exhibited a decreasing trend."

16. p.4180, l.8-14. The commenting on Fig. 3 is also imprecise. I do not see "slow accumulation" in SWE and "nearly unchanged" SDV and CV. Also, CV both increases and decreases in the melting period.

Reply:

We have modified the sentence as 'As the field investigation results indicated in February and March 2012, there are two different periods: the earlier stage with slow changed and the later stage with fast ablation. During the earlier stage, the SCF values changed slightly before the beginning of snowmelt, while the SWE increased slowly. Meanwhile, the CV and SDV changed a little. During the later days (14 March to 21 March), the SCF and SWE decreased drastically with the

occurrence of the snowmelt processes. Meanwhile, the CV increased primarily and the SDV decreased violently.'

17. p.4180, l.15-20. Discuss changes in the PDF using statistical terms. By referring to the "peak value", you mean the "peak frequency", and your point regards the "skewness" of the distribution.

Reply:

We have modified the sentence as 'During March, the skewness of the distribution decreased and the peak frequency increased with the snowmelt process.'

18. p.4180, l.20-25. I do not understand the first point, isn't SWE also reduced if SCF is one? Regarding the third point, I do think CV and SDV varies during the "winter" (not precise terminology), and CV both increases and decreases during melt, and SDV.

Reply:

We want to state that there are no certain relationship between SWE and SCF if SCF is one. So there are two states as we illustrated. 'During winter' has been changed to 'before melt occurrence'. The other sentence has been changed as 'When snowmelt occurred, the CV and the SDV changed more largely.

19. p.4181, l.11. Strange sentence. When is SWE approximately 0? Is there an error?

Reply:

Yes, there is some misunderstanding from us. We have deleted the statement. It is not necessary for the discussion and conclusion.

20. p.4181, l.20. Again terminology, use accumulation and ablation (melt) periods.

Reply:

Thanks! Done.

21. p.4182, l.4. Same again, be consistent in the terminology.

Reply:

Thanks! Done.

22. p.4182, l.8. I do not understand: "we determined if the spatial rules of a prior snow distribution exist in the pre-melt period in our study region" what rules?

Reply:

There is a statement error. It has been modified as 'firstly, we analyze if the spatial similarity of a prior snow distribution exist in the pre-melt period in our study region'

23. p.4182, l.11 Can it really sublimate at any time?

Reply:

Sublimation almost occurs in most of time. We have modified the statement more precisely.

24. p.4182, l.11. Here it is really critical that the terminology is consistently used.

You use stages, seasons and periods, and later on “winter”.

Reply:

Yes! We have revised to an uniform terminology through the new manuscript.

25. p.4182, l.18-19. Are the distributions similar or different? You are comparing spatial distributions and spatial frequency distributions.

Reply:

The statement is not precisely. We have deleted the comparison between spatial frequency distributions for a more clear description.

26. p.4182, l.24. How is the significance measured? And where is the figure with the distribution for the 21 of February?

Reply:

There is an neglect in the previous manuscript. In our revised version, this part is excessiveness for constructing the conclusion. So we deleted this statement.

27. p.4183, l.3. What rule is this?

Reply:

This is not a precisely statement. “similarity” may be more fit here. We have modified it.

28. p.4183, l.25. What is a snow period?

Reply:

It means a whole snow season from autumn to later spring. We have revised the terminology for more precise.

29. p.4184, l.1. I am not sure how to read the PAF in Fig 7. How is it correlated to SWE? There is a high scatter for high PAF.

Reply:

“excellent agreement” in our previous manuscript is not precisely. Since our revised version is not focused on the comparison of similarity, more analyses on differences were given.

30. p.4184, l.11. Is Figure 7 for a snow melt or an accumulation period. I thought it was for an accumulation period (“snow period”), but now I am not so sure.

Reply:

It is for snowmelt periods.

31. p.4185, l.1. Similar?, how?

Reply:

In melt season, the trend is similar. In the revised Figure, it is more clear. More description also inserted here.

32. p.4185, l.7 “loose range”?

Reply:

“a more large range”. We have modified it.

33. p.4185, l.14-15. This is a point which have been discussed by other authors which you have already made references to.

Reply:

Yes! We have inserted some references here.

34. p.4185, l.16 and on. I do not find these sensitivity analyses by the SNOWPACK model complete. How high are the variations in forcing compared to, say, daily observed values? Are the variations (30, 60, 90 W) realistic or relevant?

Reply:

SNOWPACK model was used to supply a similar real scenario. On the basis of the constructed model parameters, different meteorological factors were considered into analyzing the influence of it on snow distribution change. The different variations (30W and so on) maybe not enough, we have increased more sensitive scenarios.

35. Also, the other meteorological forcings, such as variability in precipitation, wind etc is needed in order to assume what is important for spatial patterns of SWE on small scale slopes.

Reply:

Yes! These factors are very important. Different random fields of precipitation and wind speed were used in the new modeling.

36. I am really not very informed by the figures 9-11. Is there a better way to illustrate the sensitivity?

Reply:

An improved image has been made to replace the old one. Fit curves with different color were used in the new one.