The Cryosphere Discuss., 7, C2299–C2305, 2013
www.the-cryosphere-discuss.net/7/C2299/2013/

The Cryosphere
Discussions
Open Access

Interactive comment on "Influence of snow depth distribution on surface roughness in alpine terrain: a multi-scale approach" *by* J. Veitinger et al.

S. Morin (Referee)

morin.samuel@gmail.com

Received and published: 9 December 2013

The manuscript by Veitinger et al. belongs to the currently large number of scientific contributions (publications, conference presentations etc.) dedicated to spatial patterns of snow height distributions measured by means of airborne and terrestrial laser scanning. This technique has emerged a few years ago and is the tool of choice to address this issue in a variety of environments. I am myself not an expert of this rapidly expanding field of research making it difficult to judge the intrinsic novelty of this contribution. Besides several scientific and editorial issues summarized below, I nevertheless think that the content of the article is sufficiently solid for publication in *The*

Cryosphere – although alternative reviews and comments may have a sharper view on this manuscript.

1 General comments

The main weakness of this article is the relatively small number of snow depth distributions individual datasets (7, spanning two snow seasons, at the ST site near Davos, and 3, spanning three seasons, at the VLDS site - split into CB1 and CB2 basins), completed by one summer high resolution DEM for each site. While the results obtained by the authors appear reasonable, this relatively small number of individual dataset questions the generality of the findings reported. Given the large number of similar data sets that are produced and published, I would encourage the authors to apply their methodology to other such datasets which would reinforce the strength of the study. If not, the paper should probably place more emphasis on its methodological nature and show the results obtained on the limited dataset as preliminary examples of the developed methodology. In its current form, the manuscript insists primarily on the scientific results and implications which are based on a limited sample size thus with a questionable robustness which may be challenged by upcoming publications on the topic.

The organization of the manuscript should also be improved. The current structure is summarized below :
1 Introduction
2 Surface roughness (i.e., a description of the method used to quantify surface roughness based on DEM data)
3 Field sites (a brief description of the two field sites and some of their (summer) topographical features
4 Data section, split in 2 for the two field sites, describing in the same section the

number of data collected in the field for each of them, introducing some of the variables used to describe snowpack height variability (equation (11)), and providing a brief overview of the snow conditions at the time of the observations. In the case of the VdlS, this section also described how the data was resampled for further analysis.
5 Terrain smoothing on basin scale. This section is split in subsections without a general introduction of what is seeked in this general section:
5.1 Terrain smoothing assessment : this section introduces additional methodological descriptions (introduction of the factor F) and provides results from the data analysis itself.
5.1 Terrain smoothing as a function of snow depth : this section compares the results from the previous subsection together with snow depth-related variables. This section also contains conclusive statements in reference to pre-existing literature.
6 Local assessment of snow depth and surface roughness structure. This section starts from an analysis of pixel-scale relationships between roughness and snow height and discusses it in light of previously published results.
6.1 Inter-annual and intra-annual persistence of snow depth : this section, in some ways disconnected from the beginning of section 6, addresses the persistence of snow height features on a intra- or inter-annual basis.
6.2 Inter-annual and intra-annual persistence of surface roughness : this analysis could be the twin of what is carried out for snow height in the previous subsection, although the method employed is different (arbitrarily chosen reference date for roughness vs. coefficient of correlation between dates for snow height).
7 Conclusions : this section not only summarizes and concludes from the previous sections, but it also contains some elements of discussion (e.g., Page 4651 lines 10 ? 16).
I think the article would benefit from a significant reorganization to make it easier to understand. A standard article structure (Introduction / Material and methods / Results / Discussion / Conclusion) is the way to go which will avoid mixing up description of methods/metrics/variables (methods) with technical considerations (methods) and

snow conditions (results) found for example in Section 4. In addition, Tables do not seem to be currently numbered in their order of appearance in the text, which a reorganization of the paper structure may help to address. Significant work is needed by the authors, but in the long term this will certainly clarify the flow of the manuscript and thus its perception by the readers.

2 Technical comments:

Page 4635, line 5: The influence of roughness on albedo concerns mainly sub-meter scales of surface roughness. Later in the article the importance of discussing roughness features with respect to a given scale is acknowledged, and I this should be the case here too.
Page 4638, equations (1) and (2). I think a figure would help understanding the geometric framework used. In addition, it should be explicitly mention that what is dealt with in these equations is the altitude of the pixels (currently not mentioned).
Page 4638, equation (4) : it seems to me that this equation will only cover half of the total range of azimuth values (between -π/2 and +π/2). Specific cases taking into account the respective signs of dz/dy and dz/dx should be treated separately (otherwise, it seems to me that slopes with true pitch values differing by a factor π would be given the same aspect). I wonder whether the article provides a summary of a (slightly) more complex method implemented in the data analysis software (in which case the issue can be addressed by editing the manuscript) or if this denotes a potentially more fundamental error in the analysis of data that is presented here.
Page 4638, line 16 : I don't understand what is referred to here as "selected neighborhood". I understand equations (1) to (4) are applied to groups of 9 pixels (one center pixel surrounded by 8 pixels). I thus understand that the x, y and z components of the slope orientation vectors are computed for each pixel. What is the role of the "neighborhood" here ? I understand the "Σ" terms in equation (9) refers to a sum over

different pixels considered. Is this where the "neighborhood" comes into play (through the number of considered pixels n) ? If yes, then I think the text should be reformulated to better reflect what is computed on a pixel basis and what is then computed on a neighborhood level basis.
Page 4640, line 17 : I understand this paragraph refer to the summer (snowfree) situation. This may be explicitly made clear.
Page 4643, line 27 : I would add "the sampled" between "in" and "basins" to acknowledge that this statement is not as general as it is written.
Page 4644, line 8 : What does "identical" refer to ? Does this refer to the closest (similar) index station, or to a variable extracted from the snow distribution dataset ?
Page 4645, line 6 : "by a scaling factor corresponding to the value of its standard deviation" : why not simply state that $\tilde{HS} = \sigma(HS) \times \overline{HS}$? Note that this means that the unit of $\tilde{HS}$ is m^2.
Page 4645, line 15 : I think that issues of significances should be handled with more care given the small number of samples upon which the regression is made. In addition, the use of a R^2 for non-linear fitting requires special care in interpreting the results and this should be addressed carefully. Maybe a visual comparison is the best that can be achieved in this situation, given the low number of data points.
Page 4648, line 14 : R^2 values provided here were calculated using thousands of data points, and are thus statistically very different from the R^2 estimated on page 4645. The number of datapoints used to compute the coefficient of determination should be provided. It would be even better to directly provide significance levels.

3 Typos or other suggestions (probably not exhaustive)

Page 4638, line 11 : "calculted" → "calculated"
Page 4641, line 18 : "vicinty" → "vicinity"
Page 4641, line 23 : I suggest to delete "within the snow surface".

Page 4645, line 13: "coeffcients" → coefficients.
Page 4645, line 19 : "charcteristic → characteristic"
Page 4648, line 20 : delete "," after "scans".
Page 4651, line 21, "have currently been developed" needs some reformulation (are currently developed ? have been developed ?)
Page 4654, line 29 : typo in the names of the authors.
Tables 1 and 2: units should be given for each variable mentioned in the tables.
Tables 3 and 7 : by consistency with Table 2, the complete date of the CB1-CB2 scans should be given (or edit Table 2 accordingly).
Table 5 : missing units.
Table 6 : rather than giving just scan number, I think for consistency with the rest of the table the dates should be given. Transposing the table should make this easier to achieve in terms of readability and space constraints.
Figure 1 : Caption "the the" → "the"
Figure 2 : the graphics are not exactly consistent with the text (letters *a* → *h* missing ; I don't understand what the part **b)** exactly represents - I had understood that the sum of vector components was done for each pixel in a given neighborhood which the figure does not represent at all.
Figure 3 : some labels are hardly legible (e.g. color bar on panel **b)**), their size should be increased. For complete clarity, the caption should mention this are snowfree DEMs.
Figure 4 : All text elements are definitively too small and should be significantly increased.
Figure 8 : the power fits are not visible (the line thickness could be increased).
Figure 9 : the caption could mention that these are pixel-scale estimates. I don't understand what the "terrain roughness" given in the caption refers to : snowfree ? what scale ?

Interactive comment on The Cryosphere Discuss., 7, 4633, 2013.