The Cryosphere Discuss., 7, C2666–C2676, 2013
www.the-cryosphere-discuss.net/7/C2666/2013/

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.

J. Veitinger et al.

veitinger@slf.ch

Received and published: 9 December 2013

Firstly, we would like to thank the reviewer for the very comprehensive set of constructive comments which he makes. In sum, they have allowed us to improve the paper considerably by restructuring it in a more traditional sense, clarifying where our emphasis lies and thus making clearer our contribution. In the following text we respond in detail to each of the reviewer's comments.

1 General comments

RC: 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

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

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.

AC: We agree with the reviewer that the results from two field sites are not enough to ultimately conclude about the general behavior of terrain smoothing. To do so, many more multitemporal datasets of field sites with different altitude, exposition, snow climate would be necessary. However, as our intention in this study is to highlight how the increasing number of available LIDAR data can be used to asses terrain smoothing, we have put a stronger focus on the methodological aspect and present the results as preliminary as suggested by the reviewer. Nonetheless, we enlarged the dataset of the ST site by one laser scan from another winter season (2012/13) which now spans over three winter seasons. The datasets were selected in a representative manner in the sense that the dataset of the ST field site (8 scans over three winters) serves as an example for the exploitation of mulitemporal acquisitions over a larger timespan (here three winter seasons) to assess terrain smoothing in a basin. The comparison with the VdlS field site (where only three datasets are available) is intended to illustrate the differences of terrain smoothing processes with respect to changing terrain morphology.

RC: The organization of the manuscript should also be improved. The current structure

TCD
7, C2666–C2676, 2013

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.

TCD

7, C2666–C2676, 2013

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.

AC: We substantially restructured the manuscript, now strictly separating the methods section form the results and discussion section. We further provide a more detailed description of the methods to shift the focus from the results more to the methodological aspect. Finally, we present the results, making clear that these are preliminary and discuss these and the methodology in general.

The new structure will be as follows:

1. Introduction

2. Methods

2.1. Field sites and data acquisition

2.2. Surface roughness calculation

TCD

7, C2666–C2676, 2013

2.3. Terrain smoothing assessment

2.4. Persistence of snow depth and snow surface roughness

3. Results and Discussion

3.1. Snow depth distribution

3.2. Terrain roughness

3.3. Terrain smoothing on basin scale

3.4. Terrain smoothing on local scale

3.5. Inter-annual and intra-annual persistence of snow depth

3.6. Inter-annual and intra-annual persistence of surface roughness

4. Conclusions

The major changes are briefly described in the following:

2.1. Only the data acquisition methods (ALS, TLS) and the parameters used to describe snow depth distribution are introduced. The snow conditions at the times of the acquisition are presented in the results section 3.1.

2.2. Here the vector ruggedness measure is introduced (section 2 in old manuscript)

2.3. In this section the method to quantify terrain smoothing is introduced in detail. It covers the introduction of the technical measures (e.g. F, before section 5.1.) as well as the method used to assess terrain smoothing on basin and local scale. It is presented in more detail to emphasize the methodological focus of the paper.

2.4. A short description of how to assess persistence is inserted.

3.1. This section describes the snow conditions at the time of the LIDAR acquisitions.

3.2. This section shows the results of the roughness calculations of the summer terrain

TCD

7, C2666–C2676, 2013

in the 3 basins. A graphic showing the roughness distributions in the three basins is added.

3.3. and 3.4. These sections replace the old sections 5.1., 5.2 and 6. These parts are now reorganized in only two sections.

3.5. and 3.6. These sections correspond to the old sections 6.1. and 6.2.

4. Elements of discussion (Page 4651 lines 10 -16) were integrated in chapter 3.3.

Further all tables and graphics were adapted with respect to the additional laser scan. Where necessary, graphics were moved accordingly to the new structure of the manuscript.

2 Technical comments:

RC: 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.

AC: The paragraph was changed to: “Whereas the latter is more related to the macro-topography (meter scale), the microtopographical (milimeter to centimeter) influence of snow surface roughness on albedo has also been highlighted. . ...

RC: 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).

AC: We added a figure showing the pixel geometry of the roughness calculation. We also mention that grid cell values represent elevation.

RC: 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 (other-

TCD
7, C2666–C2676, 2013

wise, 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.

AC: To attribute the right quadrant to the calculated aspect value, the atan2 function (arctangent function with two input arguments) is used to retrieve information about the signs of the input arguments and thus to return the right quadrant. This is implemented in the method but not explicitly mentioned in the paper. We will explicitly mention it in the manuscript.

RC: 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 "$\sum$"

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.

AC: We agree with the reviewer that the "selected neighborhood" at this position is not adequate as indeed every pixel (not only in a certain neighborhood) is decomposed into its x, y and z components. Thus we suppressed it and changed the phrase to: "Normal unit vectors of every grid cell of a digital elevation model (DEM) are decomposed into x , y and z components . Further, the reviewer is right assuming that the neighborhood comes into play when calculating the resultant vector in equation 9. Here the size of a moving window can be set by the user, defining how many surrounding cell values should be considered for the roughness calculation. Thus the neighborhood is defined

TCD
7, C2666–C2676, 2013

as size of the moving window. We will provide these definitions and reformulate the description to make this section more clear.

RC: Page 4640, line 17 : I understand this paragraph refer to the summer (snow free) situation. This may be explicitly made clear.

AC: This paragraph was rewritten in the restructuring process, with a qualitative description of the terrain morphology in the three basins. We explicitly address the summer situation.

RC: 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.

AC: We added "...in the sampled basins".

RC: 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 ?

AC: Identical in this case refers to variables extracted from the snow depth distribution datasets. We reformulated the phrase to: "....almost identical snow depth distribution parameters such as mean and standard deviation."

RC: Page 4645, line 6 : "by a scaling factor corresponding to the value of its standard deviation". why not simply state that HS$\sim$ = σ(HS) x HS- ? Note that this means that the unit of HS$\sim$ is m2.

AC: We followed the suggestion from the reviewer.

RC: 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 R2 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.

AC: We share the concerns of the reviewer using R-square in non-linear fitting and

about the statistical significance of the relationship due to the low number of data. Indeed our intention to provide the R-square values are for comparison purposes only. To illustrate the better agreement with the fit when integrating the standard deviation of snow depth (even this relationship is not significant in a statistical sense). To make this more clear we did not mention the values anymore in the text of the manuscript but rephrased it as suggested by the reviewer to: “By visual inspection we observe a much better agreement with the fit when the variability of snow depth is integrated”.

RC: Page 4648, line 14 : R2 values provided here were calculated using thousands of data points, and are thus statistically very different from the R2 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.

AC: We will provide the significance levels of the statistical relationships.

3 Typos or other suggestions

RC: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".

AC: All typos were corrected following the suggestions of the reviewer.

RC: Page 4651, line 21, "have currently been developed" needs some reformulation (are currently developed ? have been developed ?)

AC: This was changed to “. . .are currently developed”.

RC: Page 4654, line 29 : typo in the names of the authors.

AC: Corrected.

RC: Tables 1 and 2: units should be given for each variable mentioned in the tables.

TCD
7, C2666–C2676, 2013

AC: We will add the units.

RC: 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).

AC: We will insert the complete dates.

RC: Table 5 : missing units.

AC: We will add the units.

RC: 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.

AC: We will change the tables accordingly to the suggestions of the reviewer. Further we will provide the comparison between all snow surfaces (similar than for snow depth distribution).

RC: Figure 1 : Caption "the the" ! "the"

AC: Corrected.

RC: 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.

AC: We will introduce an additional graphic explaining the pixel geometry of the aspect and slope calculation (letters a-h). The reviewer is right that the sum of vector components is done for each pixel in a given neighborhood around the pixel under consideration. Fig. 2b illustrates this exemplarily for one pixel using a 3x3 neighborhood. We believe that with the better definition of the neighborhood earlier in the manuscript (see comment regarding neighborhood size) this becomes clearer.

TCD
7, C2666–C2676, 2013

RC: 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.

AC: We will increase the size of the labels and mention that it treats the summer DEMs. Further, the graphic with the roughness results will be presented in section 3.2 in the new manuscript (graphic is split).

RC: Figure 4 : All text elements are definitively too small and should be significantly increased.

AC: All text elements will be increased, the data from season 2012/13 for ST will be added.

RC: Figure 8 : the power fits are not visible (the line thickness could be increased).

AC: Line thickness will be increased

RC: 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 ?

AC: We will mention that pixel –scale estimates are used. Terrain roughness refers to the snow free situation. It is calculated at a scale of 15m. We will add this information in the caption.

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