The Cryosphere Discuss., 7, C2677–C2684, 2013
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The Cryosphere Discussions

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7, C2677–C2684, 2013

Interactive Comment

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.

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We would like to thank the reviewer for her very constructive comments, which echo those of the first reviewer and emphasize the methodological contribution of our work, while making clear that the data that we have available are not sufficient to allow more general conclusions.

GENERAL COMMENTS:

RC: The paper of Veitinger et al. addresses the topic of snow cover distribution focusing in particular on its effect on summer terrain. To this aim the authors introduce the concept of the roughness of the snow covered winter surfaces and analyze it, at

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different scales, with respect to the roughness of the summer terrain. As the authors, I also think this variable is very powerful in giving insight in the spatial distribution of the snow cover, being able to distinguish even from uneven areas and therefore also to better recognize potential avalanche release areas. My main concern is related to the generalization of the results starting from the three study cases, where the data are poor for statistics: 7 TLS in ST and 3 ALS in CB1 and CB2. Therefore, I would not stress much the consequences of the findings but more describe the methodology and highlight its potentiality, which I found very high. In conclusion, I think the manuscript is suitable to be published in The Cryosphere after the authors will have considered the following specific points. Of course, I am available for further discussion in The Cryosphere discussion process.

AC: We agree with the reviewer that a generalization of the results requires more data from other field sites of different altitude, exposition, snow climate, etc. Therefore we substantially edited the manuscript, providing a more detailed description of the methods to shift the focus from the results more to the methodological aspect, as suggested by the reviewer. This required also some restructuring of the manuscript. We now strictly separate the methods section form the results and discussion section. Due to the preliminary state of the results, we will present the results subsequently followed by the discussion rather than splitting them into different sections. Furthermore, we enlarged the dataset of the ST site by one laser scan from another winter season (2012/13) which now spans three winter seasons.

The new structure will be as follows:

1. Introduction

2. Methods

2.1. Field sites and data acquisition

2.2. Surface roughness calculation

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

SPECIFIC COMMENTS

RC: Pag. 4637, line 16: are the three basins potential avalanche release areas? I guess so; better to say it explicitly, otherwise it is not clear why you choose these areas for your study.

AC: All three basins are areas where avalanches can potentially release, as slope angle is mostly above 28 degrees. However every basin in itself does not necessarily represent one single potential release area. As this might be true for CB2 (avalanches often release over the whole basin), the CB1 area mostly produces small avalanches. The basins were selected as a function of their ground roughness ranging from very smooth (CB2) over moderately structured (ST) to CB1 (very rough and irregular).

RC: Pag. 4638, line 5: ": : :as the z value of the upper left, upper central: : :". A specific figure, associated to Fig. 2 could be helpful. As description of the method in the text is so detailed, I would add such a figure. Otherwise, you could also delete all this part of the text and just refer to the literature. I personally prefer the first solution.

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AC: As suggested by the reviewer, we added a figure to address the geometry of the roughness calculation.

RC: Pag. 4639, eq. (7) and (8): the equations should be: x = xy * cos(beta) e y = xy * sin(beta). Check it.

AC: We agree, the formulas were changed accordingly to the reviewers' suggestion. However results of the roughness calculations are not affected by this change.

RC: Pag. 4640, line 17-20: I guess this roughness is of the summer terrain; better to say it. But actually, the most relevant comment on this paragraph is that especially lines 19-20 show already the results of the application of the method you describe in Sec 2. Moreover, what do you mean with larger scales? Is it here referred only to the size of the three basins? Or is it referred, as later in the manuscript, to the scale of the analysis (5-25 m)? This paragraph needs to be clarified.

AC: In the restructuring process of the article we decided to introduce the concept of roughness later in the methods section (3.2) and therefore remove this paragraph in this section of the article. However we will provide a short qualitative description of the terrain morphology in the three basins. We will show the results of the applied roughness calculations later in the results section (3.2. Terrain roughness).

RC: Pag. 4641, line 4-6: The sentence is not clear. It is not clear how you determine the precision of each single scan. By the difference of two consecutive scans of the same snow surface? Does this mean that in each campaign of TLS you scan twice the same snow surface? I am not an expert of the laser scan technique, but I know that at the Seehore test site in Italy a TLS campaign implies a single scan of the area, as doing it twice would be too expensive and time consuming.

AC: To assess the scan quality we performed reproducibility tests. We always performed a scan in coarse resolution in the beginning of the measurement campaign in addition to the normal laser scan acquisition. This allowed us in the post processing

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to detect misalignments between the two indicating possible errors due to an unstable scanner setup (stability of tripod, wind influence, etc.). Only scans with a mean deviation of less than 10cm of the coarse scan were considered (this was meant with precision). For more clarity we will add this information in the paper. Furthermore, we will only indicate an accuracy measure in agreement with the VdlS field site. Accuracy was determined in a neighboring basin with the same device with a mean deviation of 4cm and standard deviation of 5cm at a distance of 250m. As our measuring distance ranges between 200m and 600m we estimate our vertical accuracy better than 20cm.

RC: Pag. 4641, line 15: Even if an increase is visible, due to the low number of data, I would put the sentence in a less statement way: “it might thus be a potential good indicator for the increasing: : :”.

AC: We changed the sentence to: “Thus we believe it is a potentially good indicator for the increasing redistribution. . .”

RC: Pag. 4642, line 2: here you speak of accuracy while at pag. 4641, line 4 of precision. Could you not write here also the precision for the ALS at Vallée de la Sionne, instead of the accuracy? Anyway (see comment at pag. 4641, line 4-6) precision and accuracy must be better clarified.

AC: As mentioned before we will only use the term accuracy.

RC: Pag. 4642, line 25: you write here 3 to 25 m, but later in fig. 6 it seems that the first x value is 1 m. If the scale in the manuscript corresponds to the size of the moving window (Pag. 4642, line 26), I would expect the first value on the x axis in fig. 6 to be 3. Am I right?

AC: We agree with the reviewer. We changed the scaling of the x-axis which erroneously started at 2.

RC: Pag. 4644, line 1: what do you mean with initial roughness? Is it the roughness of the summer terrain at 1 m resolution? If so, I would add a reference to Fig. 3 (b) and

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(d).

AC: We mean roughness of the summer terrain and replaced “initial roughness” by “terrain roughness”. We also add a reference to Fig. 3.

RC: Pag. 4645, line 10: why do not simply say: “A better fit is given by a power function, of the form..”

AC: We prefer to state: “Therefore we believe a power function of the form . . .. is better suited to describe terrain smoothing.” for the following reason: We do not want to stress too much the quality of the fit (theoretically we could find functions which would fit our points even better but would not make sense outside of the interval of the points) but to provide a function which reasonably describes terrain smoothing. Terrain smoothing is constrained by (0,0) (without snow F = 0) and F = 1 with infinitive snow depth. Further the fit should reflect the exponential increase of HS with F. We will mention this in the manuscript.

RC: Pag. 4645, line 19: check the order of appearance of the Tables and the numbering.

AC: We will check that all tables are correctly called out in the new version of the paper.

RC: Pag. 4645, line 19: why only for basin ST? I guess it is related to the low (only 3) number of data for CB1 and CB2. Therefore, as this problem occur throughout the whole work, I would, at the beginning of the section, clearly state that some analysis can be done only for ST as more data are available.

AC: The reviewer is right, this analysis can only be done for ST, as more data is available. We will add a short comment addressing this issue.

RC: Pag. 4648, lines 18-25: For CB1 and CB2, with only three data, I would not do the analysis (see previous comment). Moreover, 25 January 2009 is not really at the end of the accumulation season, as later more snowfalls occurred (fig. 4. (d)). Therefore your explanation is questionable.

AC: The reviewer is right that three datasets are limited for an analysis; however we decided to mention it in the paper for the following reasons:

-It is true that the dataset of 2009 was not acquired at the end of the accumulation season. Still we already have a strong persistence with the dataset of 2006. This underlines the fact that the characteristic distribution pattern might also be reached earlier in the accumulation season. This might be even more favorable for potential applications exploiting the persistence of the snow depth distribution.

-The comparison with 2011 indicates the same behavior than observed in ST, that snowfalls occurring at the beginning of the accumulation season can deviate strongly from the snow distribution at peak accumulation (especially in rough terrain). Therefore we believe that this analysis gives valuable insight although not being overall robust due to the low number of datasets. We will put a comment on the robustness of the data.

RC: Pag. 4649, line 18: on which basis you select a reference winter surface roughness? Did you chose the one where some features are correctly represented by your experience? But, do not the most representative features come exactly from the analysis you are going to present, don't they? Why do not test all surfaces versus all surfaces, without making this choice? Also for this I would not do the analysis for CB1 and CB2, or, if so, stress again the poorness of the database.

AC: We selected a dataset which was acquired at the end of the accumulation season, where we believe that the characteristic surface pattern is most likely approximated. However, we will show the comparison of all surfaces to provide a more complete picture. Concerning the VdlS analysis, we believe that the analysis still reveals some interesting insight analogous to the comments concerning snow depth distribution. Again, we will stress the relatively limited size of our dataset.

RC: Pag. 4650, line 2: for a snow covered surface, the correct expression is Digital Surface Model (DSM) and not Digital Terrain Model (DTM). Check throughout the whole manuscript.

AC: We will change this according to the reviewers' suggestion.

RC: Pag. 4651, line 27: if you want you could add a reference to "Maggioni M., Bovet, E., Dreier, L., Buehler, Y., Godone, D., Bartelt, P., Freppaz M., Chiaia, B.2, Segor V., 2013. Influence of summer and winter surface topography on numerical avalanche simulations. ISSW 2013, Grenoble, 7-11 October 2013", where this topic is addressed and also winter and summer roughness considered.

AC: We added the reference.

RC: Table 1 and 2: The units are missing.

AC: We will add the units

RC: Table 3. Put the complete dates. (if you decide to keep this table in the manuscript) Table 5. Put the complete dates.

AC: We will add the complete dates.

RC: Fig. 4. Larger fonts would be better.

AC: We will enlarge the fonts.

RC: Fig. 8. Thicker line for the fitting

AC: We will thicken the line of the fit.

RC: The figures of the snow depth distributions in the basins CB1 and CB2 are missing in the appendix.

AC: We will add the snow depth distributions in the appendix.

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