Authors comments (AC)

We thank our anonymous referee for his valuable comments and constructive suggestions.
The manuscript has been modified since the first publication for TCD, consisting mainly of the insertion of a new section (Applicability of TLS for HS measurements in steep and rough terrain) in the discussion. We further reformulated all paragraphs, commented by the referee, to make the paper more clear and precise.

Anonymous Referee #1 (AR1)

Major comments

1) It is unclear how some of this work is different from Grünewald et al. (2010) or Schirmer et al. (2011 in review).

AC: In contrast to Schirmer et al. (2011) and Grünewald et al. (2010) we studied the snow cover distribution of an entire winter in a rock face, which is located next to their study sites. Our main aim was to demonstrate the applicability of TLS to measure snow depth in steep, complex and inaccessible topography and to present first results about the spatial and temporal variability of snow depth in a rock face. Grünewald et al. (2010) mainly focus on the variability of snow water equivalent (SWE) during the ablation season in an alpine catchment with smoother terrain. Schirmer et al. (2011) analyzed the snow distribution of the accumulations season for two consecutive years and applied the model of Winstral et al. (2002), to demonstrate how such a simple model approach is able to reproduce the snow depth distribution in three areas next to the rock face.

2) In the measurement methods section, the specifics of the Terrestrial Laser Scanner (TLS) are outlined, but the accuracy and thus actual error estimation are not integrated, so this needs to be at least discussed if the authors do not present results.

AC: The accuracy and error estimations are described in section 2.5 (Data quality analysis), not in section 2.3 (Measurements methods). Direct error estimations, in the sense of direct comparison with more established methods (e.g. tachymetry) could not be preformed because of the inaccessibility of the rock face. Note that for tachymetry-measurements (Leica TCRPI1201) without a prism the distances between possible measurement-positions and the rock face are too large (> 250 m; Leica, 2006). Nevertheless, to get an idea about the performance of the TLS measurements in our study, we preformed several repeatability and reproducibility tests (described in section 2.5). In addition we compared the TLS measurements with the ALS measurements.

3) What about the other topographic/terrain variables that other use. These should be discussed more or included in the analysis.

AC: We agree that in other studies, next to slope, curvature and surface roughness, often the topographic parameters elevation and slope aspect were used to describe the snow depth distribution (e.g. Golding, 1974; Elder, 1998). But, as mentioned in the manuscript (P1391), the studied rock face covers only a very small range of slope aspect and elevation. We therefore only analyzed the correlation with slope, surface roughness and curvature. More complex parameters such as the simple model approach of Winstral et al. (2002) were not included in this study as our overall aim was to observe and analyze the snow distribution in a rock face and not to find a model to describe it. But the use of parameterizations of wind exposure, or using multivariate regressions would be worth trying in the future.

Golding, D. (1974): The correlation of snowpack with topography and snowmelt runoff on Marmot Creek Basin, Alberta. Atmosphere, 12, 31-38.
Elder, K., Rosenthal, W. und Davis, R. (1998): Estimating the spatial distribution of snow water equivalent in a montane watershed. Hydrological Processes, 12, 1793-1808.
Winstral, A., Elder, K., Davis, R., 2002. Spatial snow modeling of wind-redistributed snow using terrain-based parameters. Journal of Hydrometeorology 5, 524–538.

specific questions:

1) p 1388: why was the duration of one scan restricted to one hour?

AC: The duration was restricted to one hour to reduce possible errors, which might be caused by slight settling or tilting of the tripod. And for each scan the position of the instrument was re-determined. Like this we could minimize the errors caused by small changes in position.

2) p 1389: more information and likely references are required to explain how the DEM was created. Explain "triangulated using Delauny triangulation within ArcGIS."

AC: We used the standard Delaunay triangulation (Delaunay, 2004) within ArcGIS to obtain a digital surface model.

Delaunay, Boris N.: Sur la sphère vide. In: Bulletin of Academy of Sciences of the USSR 7 (1934), 6, 793-800.

3) p 1389: Are there enough points within each 1-m pixel for each scan (DEM and snow surface) to provide a good difference to estimate snow depth?
AC: The average point density of the point clouds of all scans was 6.12 points per square meter. The point density strongly varies within the point cloud of one scan. Towards the margin of the rock face and in unfavorable topographical conditions for laser scanning (e.g. close to measuring shadows, ~9.2 % of the entire area) the density was smaller than one point per square meter. On the other hand, nearly half of the entire area had a point density of more than 5 points per square meter. To investigate the optimal cell size for the DSM grids, the volume of snow depth change of three accumulation periods calculated with different cell sizes (between 0.5 m and 10 m) was compared to the reference volume (calculated based on the TIN). The results were similar for all cell sizes. The difference in volume was always smaller than 2 % of the TIN volume, most probably because of to the small noise component of the original point clouds. Further, Deems und Painter (2006) suggest a grid size similar to the original point density, to minimize smoothing.

4) p 1389: Using the digital photography to estimate SCA is good since it overcomes the problem with snow vs. DEM differences for shallow snow depths. However, what threshold was used to determine snow vs. bare in the RGB images? What was the colour of the underlying rock face? This could include the colour difference. See Fassnacht et al. (2009 Water Resources Research) who discussed the threshold issue.
AC: We manually checked which threshold was useful to decide whether a pixel was snow covered or not. Using a fix threshold was not possible as the natural illumination varied a lot. During the accumulation season, we could always use as threshold a blue value higher than 140 (3*8 bit RGB). During the ablation period a red value between 160 and 230 as threshold was chosen for the most of the cases. The colour of the underlying rock face was (light) grey.

5) p 1390, line 3: Is ablation based on snow depth useful? Perhaps remove this statement.
AC: With the term ablation, all processes leading to a negative snow depth change were meant here, possibly also including settling. To determine the snow depth change we only included cells, which were snow covered at the end and beginning of the observation period. This allowed us to exclude cells, which get snow free during the observation period, e.g. at the beginning of the period.

6) p 1390: what is meant by "The pathologic case is excluded in which many pixels may show a small ablation rate for a given time period between TLS measurements, caused by the fact that the pixel was already close to complete ablation at the start of the period?" The word "pathologic" is unclear.
AC: We have rephrased the sentence to now read: "This rare case is excluded in which many pixels may show a small ablation rate for a given time period between TLS measurements, caused by the fact that the pixel was already close to complete melt at the start of the period."

7) p 1391: The Data Quality Analysis section is very good. I am confused by the statement on line 8 that "the main differences occurred in the steep, rough parts of the rock face." The rough part I understand, but the error should be reduced in steep sections as the horizontal angle increased and thus those areas are more perpendicular to the TLS thus giving less error. The rough sections obviously provide more problems.
AC: In the rock face Chüpfenflue, the rough parts are also the steep parts, but we agree that this statement is not correct for the TLS data analysis. For TLS the look angel angle is more or less perpendicular in the steep, nearly vertical, parts in center of the rock face. However, this is not true for ALS, where favorable look-angles mainly occur in flat terrain. We changed this paragraph in the manuscript.

8) p 1392, l1-2: what is meant by exposition in the statement "it was not possible to study the influence of the frequently used parameters exposition and elevation on the snow distribution in the rock face?"
AC: We replaced the term exposition to slope aspect in the entire document.

9) p 1392: The daily measurements of snow depth at WJF should be included in Figure 2, rather than the sampling dates as those data are measured. Then the actual date of peak snow depth accumulation can be determined.
AC: We now included the daily measurements of HS at WFJ in Fig. 2 (see Fig. 1 below).

10) p 1392-1393: compare the slope histograms for the two sites. Show as a plot.
AC: We added a new figure showing the slope histogram of the two sites Albertibach and Chüpfenflue (Fig. 2 below).

11) section 3.2: it would be useful to illustrate more of the snow depth distribution patterns either as images as Figure 4 is difficult to read or using spatial statistics metrics.

AC: We address this point with the updated Fig. 6. of the manuscript (Fig. 3 below), which shows the histogram (calculated for classes with a step width of 5° slope) of the mean snow depth (HS, left axis) and newly in addition its coefficient of variation and on the right axis the relative frequency of the snow covered cells (SCA) is given. Further, we added a new Figure (Fig. 4 below), which shows how often a cell had a HS within the range of HS of 5% of the cells with the highest HS of this observation day (calculated based on a cumulative histogram of HS per observation day for the winter 2008/09).

12) section 3.3: how is roughness computed? This needs to be explained in more detail earlier - section 2.6?
AC: To describe the surface roughness we used the vector-based parameter VRM (Vector Roughness Measurement) described by Sappington (2007).
Sappington, M., Longshore, K. and Thompson, D.: Quantifying landscape ruggedness for animal habitant analysis: a case study using bighorn sheep in the Mojave Desert, J. Wildlife Manage., 71 (5), 1419-1426, 2007.

13) p 1395: what about the other variables typically used to correlate to the distribution of snow depth. These are mentioned earlier, but not used.
AC: See comment 3 of the major comments of AR1.

14) p 1396: How is this present paper different than Schirmer et al. (2011 in review for WRR)?
AC: See comment 1 of the major comments of AR1.

15) p 1397: there are other suitable references for the advection of heat from bare areas to snow covered areas. See Neumann and Marsh (1998 Hydrological Processes).
AC: We included reference Neumann and Marsh (1998) in the manuscript.

16) section 4 Discussion: this is limited. What about the error of the TLS. It has been discussed in other papers, but it could be different here, as the slopes are larger. See the work by Hopkinson et al. (2004) and others for more specifics on error. It is often given as the tangent of the slope when using ALS, so it would be the tangent of (90 degrees minus slope) for TLS.
As mentioned in section 2.5, the estimation of the accuracy of obtained TLS data was not possible because of the inaccessibility of the rock face. A comparison with tachymetry measurements could not be preformed because the distances between rock face and TLS is larger than the operating distance of the tachymetry without prism. We therefore cannot analyze the errors as in the work of Hopkinson et al. (2004) or Prokop et al. (2008) were they used reference-measurements to estimate the errors of the LIDAR-data. We therefore preformed repeatability and reproducibility tests and presented the data in section 2.5. Further, we now added a new section 4.1 in the discussion of the manuscript. In the new section the application of TLS to measure HS in a rock face is discussed.

17) p 1399: the authors state the limitations of the particular study. With more analysis, as listed in this review (e.g., error), the paper could have greater applicability. If not, then the focus of this paper is too narrow to be published in TC.
AC: See author comment to the specific comment 16 of AR1.

18) Table 2: snow depth change compared to what or when?
AC: Table 2 lists the mean snow depth change during the mentioned period of the rock face Chüpfenflue, of the Albertibach catchment and of the point measurements at WFJ as well as the variability (coefficient of variation) of the snow depth changes in Chüpfenflue and Albertibach.

19) Tables 2-4: How do you know you are at the same point in space to compute changes and differences in the average?
Changes in snow depth were calculated based on a fixed grid with a cell size of one meter. To calculate the differences we always used the center of the cells.

20) Table 2-6: be consistent when listing the periods - either use dates or refer to Table 2.
AC: To make it more consistent we additionally added the periods in Table 2.

21) Figure 3: Where is Wannengrat that is stated in the legend?
AC: Wannengrat refers to the same area as Albertibach. To be consistent, we changed Wannengrat to Albertibach in Fig. 3 of the manuscript.

22) Figure 4: the underlying maps in these 4 figures make them quite busy and difficult to read. Perhaps use a hillshaded DEM.

AC: We tried to use a hillshade as background, but our DHM (hillshade) only covers the rock face. We think that with this illustration the topography of the rock face is better visible than with the hillshade only.

23) Figure 6: add the standard deviation of Coefficient of Variation

AC: We added the Coefficient of Variation (CV) of the snow depth in Fig 6 (see Fig. 3 below).

24) Figure 8: this figure is confusing.

AC: We rephrased the caption of Fig. 8 (Fig. 5 below) to make it clearer.

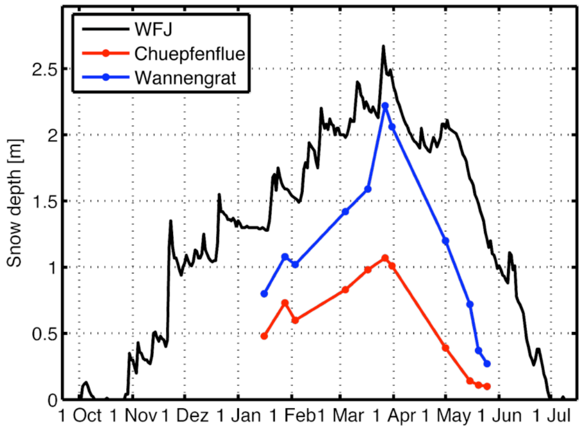

Fig. 1: Course of the snow depth measured daily at the Versuchsfeld Weissfluhjoch (WFJ) and the mean snow depth (HS_{tot}, calculated for the entire base area) of the rock face Chüpfenflue and the Albertibach catchment from winter 2008/09.

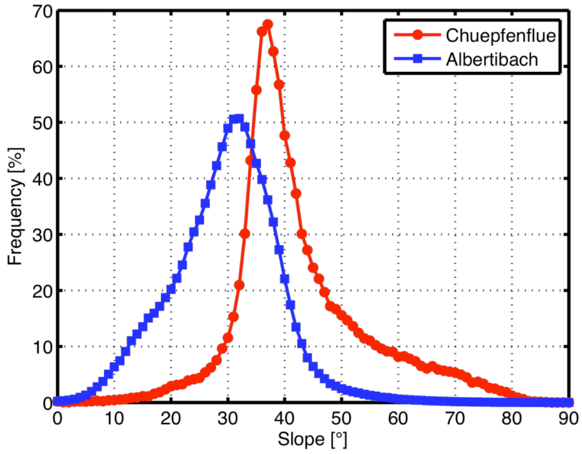

Fig. 2: Histogram with the relative frequency (calculated for classes with a step width of 1° slope) of the slope.

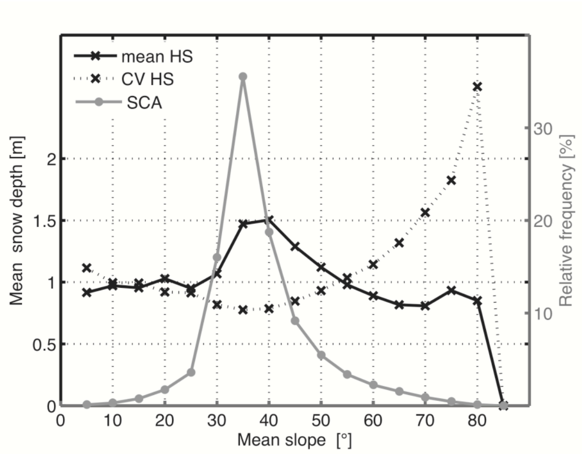

Fig. 3: (Fig. 6 in the old manuscript.) Left axis: Histogram (calculated for classes with a step width of 5° slope) of the mean snow depth (HS, left axis) and its coefficient of variation. Right axis: Relative frequency of the snow covered cells (SCA) in the rock face Chüpfenflue on 27 March 2009.

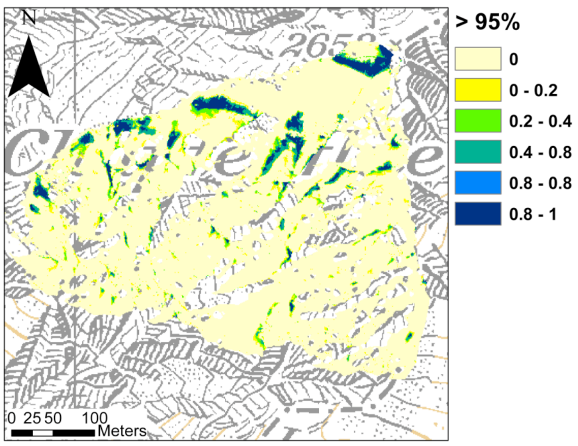

Fig. 4: Frequency of cells with highest HS per observation day over the winter 2008/09. The frequency shows how often a cell had a HS within the range of HS of 5% of the cells with the highest HS of this observation day (calculated based on a cumulative histogram of HS per observation day). A value of one means that a cell had on all observation days a HS within the range of HS of the 5% of cells with the highest HS of this observation day.

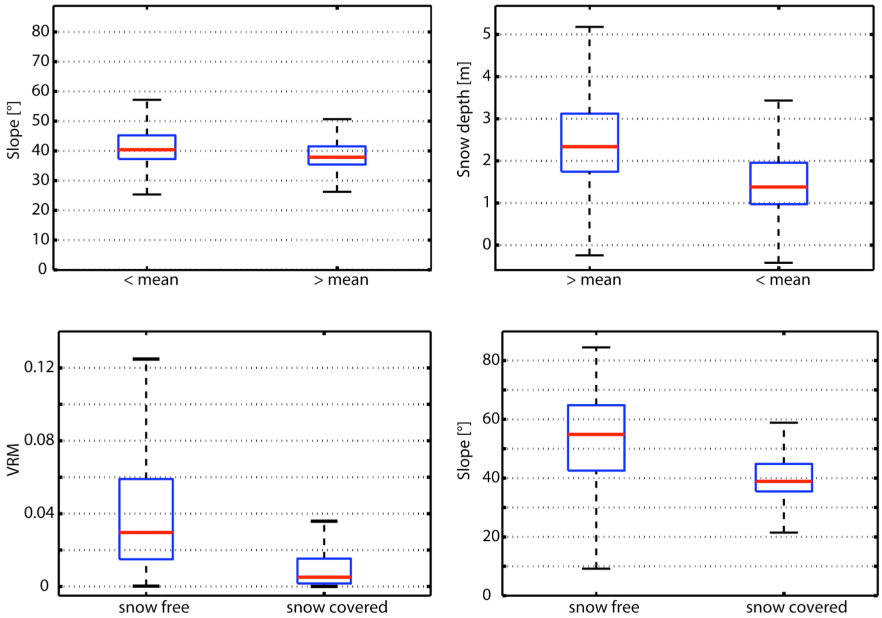

Fig. 5 *Top*: Box plots to compare slope angle (left plot) and snow depth at the end of the accumulation season (HSmax, right plot) of cells with a snow depth change (dHS) higher (>mean) or lower (<mean) than the mean. dHS is given for period 7, ranging from 31 March to 15 April 2009.

Bottom: Box plots to compare surface roughness (VRM, left plot) and slope angle (right plot) of snow free or snow covered cells in the rock face Chüpfenflue. The snow cover is estimated based on the orthophoto from 27 March 2009 (end of the accumulation season).