

Interactive comment on “Spatial and temporal variability of snow depth and SWE in a small mountain catchment” by T. Grünewald et al.

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Final Author Comments

First we want to thank our three Reviewers, R. Dadic, J. Parajka and D. Bavera, the editor and the author of the short comment, M. Pelto, for their efforts reviewing our manuscript and for their constructive comments and corrections. We have addressed most of the comments in the reviewed version of our paper. The reviewer comments are marked with RC. We added a short reply to each of the important comments, marked with AC and also give the text passages which we changed (marked with "").

RC C7-C9: J. Parajka Major comments

1. C8 1. RC: It is a novel approach, so it is, in my opinion, important to give more in-

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formation about the methodology. In order to assist in future campaigns in different regions, it will be very useful to provide more details about the requirements, recommendations and experience for e.g. positioning of the instrument, timing of the campaigns, positional accuracy, potential sources of errors and corrections and other factors needed to take into account.

AC: We would like to emphasize, that it is not our aim to publish a methodology paper on terrestrial laser scanning. This has been done before and is well cited in our paper. Nevertheless we agree with Dr. Parajka that some more information on the method might be of benefit for future applications. The added information the instrument placing, tachymeter surveys and ALS-comparison is given below:

“The LPM was mounted on a mobile tripod, which was placed on a small hill located in the south of the investigation area (Fig. 1). This position was chosen as it provides good angles of incidence for the scanner for most areas of interest and a good overall coverage of the catchment. Additionally, the place is easily accessible from the nearby storage place of the scanning equipment. In order to minimize possible errors which might be caused by slight settling or tilting of the tripod, the area was sub-divided into five single scans. Each of the scans was restricted to a maximum duration of one hour and the position of the instrument was re-determined after each scan. Detailed information how to measure snow depth with TLS can be found in Prokop (2008). We subjected all laser data to a variety of quality checks. In particular, we compared the laser measurements with those obtained from a tachymeter, as described in Prokop et al. (2008). With a tachymeter (Leica TCRP1201 total station) more than 500 single distance measurements were obtained on each of the four measurement campaigns. The tachymeter provided a very high accuracy for distance measurements at mid-range distances (up to 300 m) and therefore provided an adequate method for the quality assessment (Prokop et al. 2008). The quality control revealed that snow depths showed mean deviations in z-direction of less than 4 cm and standard deviations of less than 5 cm for distances of up to 250 m. This is a higher level of accuracy than that published

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by Prokop et al. (2008) and more than sufficient for our purposes. Furthermore, an airborne laser scan is available for the end of the accumulation season using an independent helicopter based technology (Vallet et al., 2005; Skaloud et al., 2006). With this data, it was possible to investigate whether there were systematic errors in the terrestrial scans. The analysis showed that the ALS data had a mean deviation of less than 5 cm compared to a simultaneously performed tachymeter survey. The standard deviation was 6 cm. The spatial distribution of the deviation in z-direction between the ALS and the TLS survey for 26 April is shown in Fig. 2. Comparing TLS and ALS gave a mean deviation of 10 cm and a standard deviation of 13 cm. The difference between TLS and ALS increased with the distance measured by the TLS as is clearly visible in Fig. 2 (blue colours). This is likely due to a decrease in accuracy of the TLS over long distances as the footprint of the laser beam increases. Furthermore, small deviations from the accurate position of the scanner's origin have a larger influence at larger distances. Especially in the areas with big differences between ALS and TLS, angles of incidence were worse for the TLS, which again affects the accuracy of the scan. The positive deviations (yellow and red areas in the east of the investigation area) were probably caused by a marginal movement or settling of the tripod, which caused a slight disposal of the TLS in comparison to the ALS for that area. Nevertheless the accuracy of the TLS is still more than sufficient for the analysis presented here.”

2. C8 2. RC: In my opinion, the presented results are somewhat unbalanced with respect to the title, which indicates the focus on both, snow depth and snow water equivalent variability. I would suggest to expand the results section and to evaluate in more detail also the spatial and temporal variability of snow depth. I think that snow depth is a primary variable observed by TLS, so it will be very interesting to see e.g. its changes in time and space, the dynamics of snow cover depletion (snow cover area changes), the spatial correlation of snow depth, etc. The snow water equivalent assessment is very important for many reasons, however it is not an observed variable here. Simply, I would suggest to put more stress on snow depth than linearly scaled

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SWE.

AC: We agree that the title indicates a focus on both, snow depth and SWE and it is also right that snow depth (HS) is the primary observed variable. We therefore now moved the main focus to snow depth instead of SWE and changed the title to “Spatial and temporal variability of snow depth and ablation rates in a small mountain catchment”. All concerned figures (Fig. 3, 5, 6) have been changed to HS. Nevertheless we still want to leave some information on SWE in the paper as we think that our paper as well addresses snow hydrology, with SWE being a very important variable for this approach. We are therefore showing one additional SWE map (Fig. 4) to allow for comparison with HS. Regarding “melt rates” we think that it is important to keep the main focus on SWE-melt rates. As ablation is especially of interest for hydrological questions, we believe that snow density and therefore the SWE-melt rates must not be neglected and that the main focus should remain on SWE-melt rates instead of changes of HS. Nevertheless, to enable comparison, we now show an additional figure, showing daily change of HS for one period (Fig. 8).

Minor comments

C8: 1. RC: P7: more details about snow density measurements (location, variability) will be useful.

AC: Additional information on the density measurements has been included to Fig. 1 (location) and Table 1 (standard deviations) with references in the text.

2. RC: P9: the incoming solar radiation calculated by Alpine3D represents the potential or actual value (how are the clouds estimated)?

AC: Alpine3D calculated the actual value using input from a meteorological station located in the area. Clouds are represented by the model indirectly because the model calculates radiation based on a comparison between potential radiation and the measurement. This will be a problem for a discontinuous cloud cover. We already stated

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this in P10 L25f.

“Furthermore, there may be differences between the modelled meteorological variables and the true distribution, e.g. incoming radiation neglects frequent convective clouds over mountain tops.”

3. RC: Results section: The evaluation of mean snow depth only for areas covered by snow is somewhat misleading. It is stated that the fourth campaign represents the end of the melt season, and the mean snow depth presented is larger than 1m. Please consider to revise the focus and to present also the averages over the whole region. The changes e.g. in snow cover area would be also a very useful information. The same may apply for the SWE analysis. From hydrologic point of view, it is much more important to know the catchment mean of SWE rather than to estimate the mean SWE of snow covered area.

AC: We agree that averages of snow depth and SWE respectively should refer to the complete area investigated and we therefore changed this and rewrote and shortened the complete paragraph 3.1. We have also included information on the snow covered area and additional information on the total amount of SWE stored in the area.

“3.1 Time development of snow depth distribution A map of the spatial snow depth distribution in the investigation area for the end of the accumulation season (26 April) is shown in Fig. 3a. The corresponding calculated SWE map is shown in Fig. 4. At that point in time, the entire catchment was snow covered, with exception of small snow-free patches in the steep rock faces in the south and north-west of the area. Average snow depth was 2.0 m (SWE 696 mm) and maximum snow depth was 9.0 m (SWE 3120 mm). Those large snow depths were mainly located at two cornice-like drifts which had formed on the steep north-easterly slopes of the Wannengrat summit due to drifting and blowing snow. Areas with above average snow depth could also be found in the steep south facing slopes (SE). The flatter areas of the Chilcher Berg clearly showed average or below average snow depths. Nevertheless, linear features

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with above average snow depth are also clearly visible in that area. From the DEM and a topographic map we could identify those features as ditches, which were packed with snow probably due to snow transport processes (Lehning et al., 2008). The following scan, 17 days later, showed predominantly unchanged spatial patterns (Fig. 3b): The cornice- like drifts in the NE remained the dominant feature until the end of the ablation season while snow depth in the SE suffered stronger decrease than average (Fig. 3c, d; see also Fig. 6). A significant number of snow-free patches emerged especially on the knolls and ridges where snow depth was lowest in the beginning of the ablation season. Complete melt out propagated quickly from those first snow-free areas. This may be because of lower snow depths at the edges of the snow patches or because of lateral advective transport of heat from the warmer snow-free surfaces onto the colder snow cover (Essery and Pomeroy, 2004), which will be discussed in detail below. Histograms of snow depth at the time of each scan show that the distribution and the variability changed in the course of the ablation season (Fig. 5a). The mean snow depth and the spatial variability, represented by the standard deviation (σ), both continuously decreased but σ remained on a high level (Fig. 5b). The snow covered area and the total volume of SWE stored in the area strongly decreased. While 98% of the investigation area was covered with snow on 26 April, this area steadily decreased to 25% on 10 June (13 May = 82%, 2 June = 38 %). The snow volume decreased from $3.3 \cdot 10^5 \text{ m}^3$ on 26 April to $1.0 \cdot 10^5 \text{ m}^3$ on 10 June (13 May = $2.6 \cdot 10^5 \text{ m}^3$, 2 June = $1.1 \cdot 10^5 \text{ m}^3$). There were clear differences in snow depth distribution between the two sub-areas SE and NE (Fig. 6a, b). The cornice-like drifts (discussed above), which featured the areas of maximum snow depth, were located in NE. Mean snow depth with 3.1 m (SWE = 1059 mm) were therefore larger in the NE than in the SE with 2.7 m (SWE = 919 mm) at the end of the accumulation season. A higher variability was also observed in the NE ($\sigma = 1.7 \text{ m} / 569 \text{ mm}$) than in the SE ($\sigma = 1.0 \text{ m} / 355 \text{ mm}$). This finding is also obvious from Fig. 3, where the more homogenous snow depth values in the SE are clearly visible. The context of higher variability and higher mean snow depth in the NE remained during the entire ablation season.”

4. RC: Results: Please consider to revise the term melt rate. It is somewhat confusing. It refers to the SWE change over two weeks, so there may be also other factors (as it is already stated in the text), which may affect this change (e.g. new snow/rain, wind?).

AC: “melt rate” has been replaced by “ablation rate”

5. RC: Fig2: For comparison, it would be very interesting to add here the map of snow depth observed by TLS.

AC: See major comments (2).

RC C14-C17 D. Bavera General Comments

1. RC: TLC measures the snow depth by difference with the no-snow conditions. For this reason it should be better to clarify what is measured and monitored (snow depth) and what is afterward computed by correlation (SWE). Sometimes the terminology could appear misleading (e.g. P2, line 6 “monitored”). It is worth to provide the analysis maps of space/time variability primarily for snow depth, as it has been done for the SWE. I would suggest giving more importance to snow depth analysis also in the conclusions.

AC: See response to RC C7-C9 (Major Comments 2)

2. RC: The description of the snow density estimation should be more detailed both in terms of measurements (number, location, etc.) and analysis of data (relationship between snow depth and SWE, included parameters, assumptions, etc.). Moreover could be also included the snowpack density variability during the melting season.

AC: See response to RC C7-C9 (Minor Comments 1)

3. RC: The reliability of the snow depth measurements is compared with other indirect measures (ALS, Tachymeter). It could be interesting to include a comparison with a fieldwork using snow stake to measure the snow depth.

AC: We did no measurements using snow stakes in the campaigns. Comparison of

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TLS with snow stakes has been done and published by Prokop et al. 2008.

4. RC: I would use a different expression for “melt” and “melt rate” (e.g. P2, line 12; P16, line 6). They are including, in this manuscript, also other processes (sublimation, new fresh snow, etc.) as already stated by the Authors. I suggest using something similar to “snow mass variation” or “SWE variation”. Then also snowdrift phenomena and avalanches could determine space/time variability of the local snow depth within the basin.

AC: see response to RC C7-C9 (Minor Comments 3)

5. RC: Please include more details on the experimental setting and instruments, experiences, suggestions

AC: See response to RC C7-C9 (Major Comments 1)

6. RC: It is worth to test on more basins, in future works, to give more robust conclusion

AC: It's completely right that testing on more basins would be of benefits. But currently no such dataset exists. Future work might enable such studies.

Specific Comments (selected)

RC: P5, line 12: How the ending date of the accumulation season has been estimated?

AC: “The end of the accumulation season was estimated from snow depths measurement at the permanent weather stations in the catchment.”

RC: P6: the removal of the melting out cells could be misleading. Regarding the SWE I think that it would be more relevant, for practical application, the total amount of SWE on the basin than its mean value on the snow covered area (the same comment for P11).

AC: see response to RC C7-C9 (Minor Comments 3)

RC: P7, line 12 and 22: units of snow depth (HS) should be always the same in the

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manuscript (m or cm?). In any case I suggest to use the same units for snow depth and SWE (mm)

AC: We changed all snow depth to m; We do not think that SWE and HS should have the same unit as the commonly used unit HS is m or cm while it is mm for SWE

RC: Fig. 9: Comment: it seems that for low slopes it is unlikely to have high “melt rate”. This could be related to local avalanches which transfer snow mass from slopes (giving high “melt rate”) to flat area and also to a greater accumulation of snow in flat area. Snow mass movements could be included in the discussion.

AC: “Notice that the high correlation with slope, meaning that flat areas are characterised by smaller ablation rates, can not be explained by relocation of snow from steep to flat areas due to avalanches, as no such hazard affected the investigation during the ablation season.”

RC: References: I suggest including in the reference list three more papers, considering their relevance for the topic of this paper. . .

AC: Bavera and De Michele (2009) and Mizumaki and Perika (2008) have been cited and added to the references.

RC C35-C39 R. Dadic

General Comments

1. RC: The authors have done a lot of very interesting work, which is only briefly mentioned. It would be nice to see a more detailed discussion on the snow density measurements and the corresponding correlation analysis or on the TLS validation against the tachymeter and ALS.

AC: See response to RC C7-C9 (Minor Comments 1 and Major Comment 1)

2. RC: There should be more references to figures in the text, so it is more clear where the statements in the text come from.

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AC: We added more references to figures.

3. RC: I do not understand what the aim of the analysis of the average SWE in time is, when only cells that have snow are used (P10, L28). The analysis is not teaching us anything about physical processes of snow (nor are the results relevant for what the authors are presenting), but seems more like a statistical misrepresentation of the real processes. Also, the spatial variability would not have been constant through time (P12, L26) if always the same cells were used. The authors should consider removing this part of the paper, as well as Figure 4b.

AC: We changed the statistics to the scanned area as reference. The section has been rewritten according to the changes. (See also RC C7-C9 (Minor Comments 3))

Specific Comments (selected)

1. RC: The title should either emphasize SWE or snow depth, because these two are closely correlated in this paper, and the data properly covers only the snow depth, while SWE is linearly dependent on density.

AC: See response to RC C7-C9 (Major Comments 2) 2. RC: P5, L24: It is not clear from the text, why you compare TLS and tachymeter measurements up to 250 m only. I assume that this is the reach of the tachymeter, but it should be stated in the text.

AC: See RC C7-C9 (Major Comments 1)

3. RC: P6, L1: Is the deviation random or does it have a pattern?

AC: “The difference between TLS and ALS increased with the distance measured by the TLS as is clearly visible in Fig. 2 (blue colours). This is likely due to a decrease in accuracy of the TLS over long distances as the footprint of the laser beam increases. Furthermore, small deviations from the accurate position of the scanner’s origin have a larger influence at larger distances. Especially in the areas with big differences between ALS and TLS, angles of incidence were worse for the TLS, which again affects the accuracy of the scan. The positive deviations (yellow and red areas in the east of

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the investigation area) were probably caused by a marginal movement or settling of the tripod, which caused a slight disposal of the TLS in comparison to the ALS for that area.”

4. RC: P6, L4: The differences between ALS and TLS should be discussed more. Figure 2 shows that in places the difference is positive and in places the difference is negative (blue and red colors have the same absolute value), so it is not clear where the differences increase with distance. It would be helpful, if topography contours were overlaid on Figure 2.

AC: See RC C7-C9 (Major Comments 1)

5. RC: P7, L13–19: It would be very interesting to see the described density-analysis. You could show some of the figures from it and discuss it more in detail. Table 1 can be included in the text. It is not clear which ρ is used in which periods? E.g, do you use 345.2 or 388.6 from 26 April - 13 May or do you linearly interpolate between the dates?

AC: See response to RC C7-C9 (Minor Comments 1) Which density was applied to which HS should be more clear now:

RC: “As a next step, we produced maps with the total SWE applying Eq. 1 to each of the four snow depth maps. Maps of the average daily “ablation rate” were created by calculating daily ablation rates from the change between two consecutive SWE maps.”

6. RC: P9, L9: Are clouds considered in the ISWR model?

AC: See response to RC C7-C9 (Minor Comment 2)

7. RC: P9, L9: I do not understand why the SWE at the end of the season should have any influence on melt, if only cells that have snow at the end of the season are considered. Please explain.

AC: “SWE at the end of the accumulation season (SWE_{max}) derived from the TLS sur-

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vey. The ablation rate as calculated here contains also the contribution of settling and we therefore also investigated whether a correlation between maximum snow depth or snow water equivalent and ablation rate exists.”

8. RC: itemP10, P17: I do not understand what "cross-slope accumulation" refers to. Please explain.

AC: "cross-slope accumulation zones" replaced with "cornice-like drifts"

9. RC: P10, L28: The remark that mean SWE values remained constant is confusing because they are actually not, because you are averaging over different areas (as explained on P11, L7). You should consider removing this entire paragraph (P10, L28–P12, L1) as well as Figure 4b, because the statistics are not representative and they do not say anything about the actual snowcover.

AC: See General Comments (3)

10. RC: P13, L1–5: One reason for the shift of dominating processes is also that the sun is much higher later in the season, which leads to less difference in the potential incoming radiation for varying expositions.

AC: That is right and this is why shortwave radiation has a low influence on the spatial variability of the ablation.

11. RC: P13, L11: Add that snowfree patches will probably be larger in areas, which are more exposed to the sun (e.g. the northernmost slope in Fig 6c still has the lowest melt rate). Another reason for the patchiness of the snowcover is the irregularity of the underlying topography.

AC: The reviewer is right but in this part of the manuscript we are only discussing the effects of snow free patches on melting. It is not our aim to explain the development of the patches.

12. RC: P14, L21: Instead of having the slope angle correlated with the melt, it would

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be more useful to have the incoming solar radiation plotted. Please consider changing this plot.

AC: Fig. 11 now shows iswr instead of slope

SC C4-C6 M. Pelto

AC: As stated in our response to RC C7-C9 (1) this is not meant to be a methodology paper. We agree that a full energy balance model might be capable to explain more of the ablation. Nevertheless, as stated in the paper this is a first and easy approach to explain spatial variability of HS, SWE and ablation. More complex attempts are planned to be done in future studies of the data set. To our knowledge it is the first catchment-wide study. Prokop et al. (2008) or Schaffauser et al. (2008) (which we think M. Pelto is referring to) are investigating single slopes.

Interactive comment on The Cryosphere Discuss., 4, 1, 2010.

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