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Interactive comment on "Topographic control of snowpack distribution in a small catchment in the central Spanish Pyrenees: intra- and inter-annual persistence" *by* J. Revuelto et al.

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Interactive comment on "Topographic control of snowpack distribution in a small catchment in the central Spanish Pyrenees: intra- and inter-annual persistence" by J. Revuelto et al.

Dear editor, dear reviewers, We are pleased to submit a revised version of the manuscript. First of all we want to thank for your effort in improving the work. Recommendations about the methodology, and about the form to transmit the main ideas of the paper have resulted extremely useful. We have followed by far the majority of them, and we think that they helped to prepare a better manuscript, easier to be read

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and more robust from a methodological point of view. Below, we provide a point by point answer to all the comments raised from the review, and the changes that we have introduced in this revised version. Despite it is required a separate answer to both reviews, we provide the same document to both reviewers to facilitate them a fast assessment of all changes introduced in the manuscript.

Looking forward to hear your kind reply,

Jesús Revuelto and co-authors.

Referee 1.

Major coment:

Referee (R hereinafter): I do not see good reasons for randomly sub-sampling the data for the correlation- analysis (P1945 L12ff). Sub-sampling the data set is meaningful for the regression trees (training data and validation data) but - in my opinion - not really required for correlation- and regression analysis. The authors cite Hair et al. for reasoning this procedure. Checking this reference I think that the decision to sub-sample the data is based on this or a similar statement of Hair et al.: "... large sample sizes of 1000 observations or more make the statistical significance tests overly sensitive, often indicating that almost any relationship is statistically significant" (Hair et al. 2006, p195) However, Hair et al continue: "With such large samples the researchers must ensure that the criterion of practical significance is met along with statistical significance" - which means that large sample sizes are adequate as long as the model is physically meaningful- which is the likely case in the presented study. Merging all data in one regression analysis instead of presenting average values of multiple random sub-sampling would be more straightforward and better understandable. I therefore suggest to redo the analysis with the complete data set. It can then be checked if the new results deviate from the presented ones. I expect that the results will not differ significantly and then the "simpler" approach should be presented (it can still be stated in the text that an additional analysis with random samples has been performed).

Manuscript authors (A hereinafter): As suggested by both manuscript reviewers, statistical analyses have been redone using the whole dataset for obtaining Pearson correlation coefficients and also in multiple linear regression (MLR). Meanwhile bootstrapping technique has been used to determine whether the relations depicted by Pearson's coefficients can be considered statistically significant. Thus, when more than the half of the 1000 random replications (with 100 cases) provided p-values below 0.05, the correlations where considered statistically significant. As it can be seen little differences in the revised manuscript are observed regarding our previous approach. Thus, the main conclusions of the manuscript have not been affected with these changes.

R: Moreover the model assumptions for linear regression (at least normality and constant variance of residuals) should be tested and briefly stated. And I also suggest to test if meaningful additional factor combinations (e.g. multiplication of parameters) change the models.

A: Normality test and variance in the residuals were checked, in any case we have seen the need to transform any variable. Meanwile, residuals have shown constant variance and no spatial autocorrelation (using Moran's I test) was detected in the residuals (according to method presented in López-Moreno et al., 2010). As the main purpose of the paper is to see if the role of the topographic variables and their inter- and intra-annual changes and not to achieve a maximum predictive capacity, we have preferred use isolated variables and not combination of them.

Minor Coments

R: p1938 L4: state that all 12 campaigns were in winter A: The campaigns were done in winter and spring (between February and June) as it is now clearly stated in the revised manuscript, period that is referred in the revised version as. "snow season".

R: L9: best instead of better A: It has been changed.

R: L12: the reader might not know about the TPI - a short description should be added

A: A brief description of TPI has been included in the abstract: "index that compares the relative elevation of each cell in a digital elevation model to the mean elevation of a specified neighborhood around that cell with a specific shape and searching distance"

R: L16: remove "most" A: It has been removed.

R: L22 remove "distributed"? A: It has been removed.

R: P1939 L23 most studies abbreviate Lidar with "Light detection and ranging " A: This suggestion has been included.

R: P1940 L8: Grünewald et al 2013 and Lehning et al 2011 could be added as reference; these studies explicitly examine the effect of topography on the snow distribution at different study sites. A: These references have been included and discussed, thank you for suggesting these interesting works.

R: L12 "such as" instead of "including" A: This change has been included.

R: P1942 L7: where was the "historical data set" measured at the meteorological station indicated in Fig 1 or somewhere else? A: The historical data set, used for the percentiles calculations for temperature and SD comparison, was measured at the meteorological station at Figure 1. It has been clarified in the revised text.

R: L23: In this context I would remove Schirmer and Lehning (it is a companion paper of Schirmer et al. 2011 that used the same data set) and add Egli et al. 21012 and Mott et al. 2013 instead. A: These suggestions have been included.

R: L26: here you should cite Grünewald et al. 2010, Prokop et al. 2008 and Schaffauser et al 2008 A: These references have been included.

R: P1943 L3 what is meant by "TLS instability" A: It meant little variations of TLS position due to tripod and tribrach little displacements, originated by the inherent vibration of the device while it is working and submilimetric ground variations. It has been clarified in the text: "To reduce the influences of TLS instability (originated by small

displacements of the tripod because TLS vibrations while it is operating)"

R: L5 please name the most important points of the protocol (very briefly.) A: The most important points of the protocol have been briefly included: "This protocol has these main points: data collection; which includes experimental setup design and information acquisition by the scanning procedure; and data processing, where data is filtered, quality checked and the SD maps generated. Mainly, the methodology was based on differences between DEMs obtained with snow coverage in the study area and a DEM taken at 18 July 2012, when the catchment had no snow."

R: L13: Is 25 April still accumulation season? Table 1 indicates clearly reduced snow cover in comparison to the earlier survey. How did you distinguish between accumulation and ablation season? A: The 25th April shows less snowpack than the previous survey, but corresponded to a period of alternation of melting events with snowfalls. During May and June melting conditions dominated clearly. To avoid confusion we have modified this phrase as: "In each year three surveys were undertaken from February to April (2012: 22 February, 2 April, 17 April; 2013: 17 February, 3 April, 25 April), and three were undertaken from May to June when dominated intense melting conditions (2012: 2, 14 and 24 May; 2013: 6, 12 and 20 June)."

R: L18: I think this is wrongly formulated: the resolution of the original DEM was 1m but I guess that the resolution of the raw data (point density) was higher (and varying in different areas). A: This point has been clarified in the revised version as follows: "From the two scan stations located in the study area (Fig. 1), 86% of the total area of the catchment was surveyed using TLS. DEMs of 1m grid size were initially obtained from point clouds of varying density in different areas, but always with a minimum of 1point/m2 (Revuelto et al., 2014)."

R: L22-26: unclear: how where the two DEMs combined - did you use the IGN only for the data gaps or where both DEMs averaged where data were available for both of them? How good do the DEMs match? - Our experience showed that DEMs obtained

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by different methods can vary strongly. A: The IGN DEM was combined with the TLS one, averaging all values within each cell of the empty raster (it has been specified in the manuscript). DEMs matched well, with a constant bias of 12.3m that was corrected. It is necessary to bear in mind that the filled areas were not used for the analyses, but just to be able to run the scripts for obtaining TPI and Sx for the whole area. In this way, I think that potential small biases have not affected the obtained results.

R: P1944 L8: Curvature in which direction - horizontal, vertical or a combination? A: Curvature was calculated in the direction of maximum slope of the terrain, taking into account the neighbours cells. It has been clarified in the text.

R: L10 watts per hour square meter could be abbreviated; it should furthermore be mentioned that potential clear sky radiation might differ strongly from the real radiation (e.g. effect of clouds) A: These units have been abbreviated, and also it is mentioned the possible differences between clear sky radiation and real radiation.

R: L15-16: "max slope line of the terrain" > this is actually the aspect right? Writing "deviation from the aspect to..." would be clearer. Isn't easting the deviation of the aspect from East? Providing formulas for northing and easting and also for TPI would be helpful.

Here, Northing is defined as the cosine (sine for Easting) of the angle between North direction and terrain orientation or aspect. This way, for a better understanding, the text has been modified accordingly. For TPI it is provided the reference in which this index is defined and following the suggestion of the other revision the algorithm of the TPI is presented in the text. Moreover, as done for Sx parameter, provided references may allow to the readers finding all necessary information.

R: P1945 L1: are all those wind directions present in the study area? Only such directions should be used A: All these directions are considered in order to compare the best correlated Sx direction for each survey day and the average wind directions of the 15 day study period. This allows considering if the correspondence between real

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wind direction and Sx best correlated direction is consistent; as finally is concluded in this work. We have also included wind roses of the previous days to scans to clarify this question (Figure 4 of final version manuscript attached as supplementary material).

R: L18 How where they (Sx and TPI) selected? I guess that analysis for all variations were performed but only the two are presented? A: These searching distances were selected after considering all Pearson correlation coefficients for all distances and directions. In order to simplify the presentation of the results, the best correlated distances are presented. Except for the 02/04/2012 and the 24/05/2012; 25m searching distance for TPI and 200m searching distance for Sx, were the distances with better results for all surveys. Note that the 02/04/2012, and the 24/05/2012 surveys days had the lower SD and SCA average values (Table 1 of final manuscript version), and for these days, very small differences were observed between 15m and 25 m distances for TPI and 100m and 200m for Sx. As examples in figure 1 provided in this discussion, are presented box plots of correlations correlations obtained for the TPI searching distances for the 02/04/2012 and the 03/04/2013 (a day with high SD accumulation). We consider that with the brief explanation provided in the text may serve to be understood by the readers and will trust manuscript authors, otherwise the manuscript would be considerably increased.

R: P1946 L3-12: In my opinion the section can be shortened (e.g. Eq 1 is not required). Where factor combination included in the regression models? And where the variables scaled? - Scaling could help to identify the importance of each variable for the model. A: Following your recommendation we have removed Eq 1 from the text and shortened a little the text. We also specify that we compare beta-coefficients, which are in standardized units (scaled) and, hence, they can be directly compared independently of their magnitude. The main objective of the manuscript is to assess the role of the different variables instead of finding the best predictive model. Thus, any factor combination was included because it is preferred to consider the isolated effect of each topographic variable to facilitate their comparability

R: P1947 L5 (also Fig 7): Were multiple or only one tree grown for each survey? Please specify. Different sub-samples might result in very different trees and one should consider to apply "random forest" instead of a single tree. How many splits were allowed when growing the trees (how were they pruned)? A: Sample sizes used for binary regression trees of 15000 cases with 5000 cases used for validation, represent the entire data set for each survey day (around 56 ha, covered in a 85% by the TLS, at 5m grid size). In early analysis before writing the manuscript, five trees (with different cases combinations for growing and testing the trees, but always respecting 15000 and 5000 cases) were grown for each survey, with negligible differences. This way, in the early manuscript version, it was decide to only include one binary regression tree for simplicity. As stated in the text, 15 terminal nodes were allowed (a similar number of other snow studies; (Winstral et al., 2002)) with a minimum of 1% explained variance for each split, and at least 500 cases on each terminal node. It was done aiming to find medium sized trees. As the main purpose is to see the total explained variance of each trees, the final tree size is not critical as after the first splits the explained variance is rather low and main conclusions are not affected.

R: P1947 L 14 I suggest to include a section that briefly describes the general characteristics of the snow cover and how it changed during the seasons. Most of the information is already available in Table 1 and Fig 3 but should be included in the text. A: Following the reviewer's recommendation, we have described the general characteristics of the snow cover at the end 3.1 section.

R: L16- P1948L6: The section is hard to follow, and appears incomplete and inconsistent: e.g. the later surveys also show high correlations in the N sector and the mentioned increase of SX is not obvious for all of the surveys. It should furthermore be described in more detail what the best directions where and why they differ between the surveys. A: This section has been modified, because wind information has been included in the manuscript and also the information provided before in Figures 4 and 5 is now in Tables 2 and 3 (see supplementary material provided in discussion were

the final version manuscript can be found), so differences are better observed and this section is better understood. Now the text is written as follows: "Table 2 shows the correlation between SD and Sx for the eight wind directions at a distance of 200 m (identified as the best correlated searching distance in previous analysis). Despite differences in magnitude, the correlations for surveys carried out at the beginning of the season (22 February 2012 and 17 February 2013) in each year showed that SD was clearly affected by N and NW wind directions. This was particularly evident in 2013, as the correlation values were higher for both days. The contribution of N and NW wind directions is clearly evident for the surveys on 17 February 2013 (Figure 4, were wind roses with average wind speeds and direction, for the 15 day period before each survey are presented), when greater SD was recorded in the leeward slopes from a northerly direction (Fig. 3, upper areas of the maps). In the two years of the study a correlation with W and SW wind directions was observed to increase progressively during the snow season (Fig. 4 and Table 2 correlations). In 2013 this phenomenon was less marked because of the greater SD accumulation at the beginning of the snow season accompanied with NW direction winds, which resulted in only moderate changes in the Sx for the most strongly correlated wind directions. It was also observed that in both study years once the snow had started to melt (the last three surveys in each season) the snow distribution did not change in relation to Sx directions. When the best correlated Sx directions for each survey are compared with wind roses (Fig. 4) a good agreement is observed. These directions for survey days are: 315° for 22 Feb. 2012, 270° for 02 and 17 April 2012, and 225° for the three surveys in May 2012; in 2013, 315° was the best correlated direction for 17 Feb. and 270° for the other five surveys of the snow season"

R: P 1947 L18 please list the dates you are meaning with "beginning of the season". One could mark the sub-panels of Fig 3 to 5 with a,b,c.... and mention the panels in the text. A: Dates referred as "beginning of the season" have been listed in the revised manuscript. Figure 3 has been converted into a Table (table 2 of revised manuscript).

R: P1949 L18 what about the accumulation season? A: We have added the following sentence: "However, during the accumulation period in 2013 statistically significant positive correlations were observed with Northing and Radiation, which are connected to the strong snow redistribution by winds from N-NW directions."

R: P1949 L21 and Fig 6: Wilmotts D an R2 are similar in terms of their information content so what is the additional value in showing both of them? I would only show R2 as the more common one. A: In general they both are rather similar, but are not exactly the same. In some cases, two contrasted variables (i.e. observation and simulated values) may show high correlation coefficients, but they can fail assessing the magnitude of the variables. Observing equation 2, Willmott's D account the observed and predicted values differences, so it is a good indicator (similar to Nash-Sutcliffe) that we recommend to apply when statistical models are tested. This way, we are interested on contextualizing the obtained results with R2 (as stated, the most common one) and Willmott's D.

R: P1951 L9: If I understood correctly, the wind-direction of SX differs between the surveys. I think that the concrete direction of SX should be stated in the text, table 3 and Fog 7: A: Yes, the wind direction of Sx differs between surveys. These directions are included in Figure 6 (Figure 7 in previous manuscript version, so it is not provided in the supplementary files of this discussion) and main results about wind directions are now stated, commented and discussed.

R: P1952 L1: in its current form the manuscript is not really showing that the variability of the distribution of snow was high - it was shown that the terrain control on SD was variable. A: We think that Table 1 and Figure 3 (table and figure maintained as in the previous manuscript version) show that SD distribution is highly variable in space and time in the study site. Following your recommendation a brief consideration about this has been included.

R. p 1952 L9-24: the results should be related to other studies in more detail: what

exactly was analyzed and found these studies? E.g. one could mention the parameters and performance of the most important studies. A: The revised version mention the wide variety of results that can be found in this regard: "There have been many studies of the spatial distribution of SD in mountain areas (Anderton et al., 2004; Erickson et al., 2005; López-Moreno et al., 2010; Mccreight et al., 2012). Besides some researches have also focus their attention in long-term inter-annual snow distribution analyses (Jepsen et al., 2012; Sturm and Wagner, 2010, Winstral and Marks, 2014) but there are very few datasets that have enabled investigation of the intra- and inter-annual occurrence of topographic control on the snowpack distribution, being important to investigate both time scales. The results of previous research have highlighted the difficulties in fully explaining the distribution of snow in complex mountainous terrain. In addition, the results have differed among studies, and suggest that different variables govern the distribution of snowpack among areas as consequence of their differing characteristics and geographical settings, including surface area and altitudinal gradients, the importance of wind redistribution, the presence or absence of vegetation, and the topographic complexity (Grünewald et al., 2013)". We think that probably is enlarging too much the manuscript by doing an exhaustive description of the cited works.

R: P 1953 L2 - I think it should be "wider convex" not "wider concave" A: We think that the text is right. Curvature correlation with SD is constant, during the study period, while TPI correlations decrease (at least this is clear for 2012 snow season, and this tendency is also observed in 2013). Curvature only considers terrain characteristics of surrounding cells, so it is a considered "deep" concavity. In the other hand, TPI considers terrain characteristics in a 25 m distance (the best correlated searching distance), so it is clear that it attends "wider" concavities (negative TPI values, which represent concavities, have higher SD values as it is expected from the negative correlation coefficients). This way as TPI correlation decreases at the end of the snow season, it is clear that snow is melting faster in wider concave areas, and remains longer in deep concavities, because higher snow amounts were accumulated before in deep concavities.

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R: L26 "altitudinal differences" instead of "altitudinal gradients" A: This change has been included.

R: P1954 L3-8: I do not understand the link between elevation as a model parameter in this study and the findings of Anderton et al. and Lopez Moreno et al. Following the manuscript the parameters (including Elevation) where included to the models if they had a significant effect- this section sounds like, "because the others found that elevation was important we included it" A: This is not the intention of this sentence and the sentence has been reformulated accordingly. Elevation was not included because others found that it was important. This paragraph starts "Although Elevation has been found to largely explain the snow distribution in areas having marked altitudinal differences (Elder et al., 1998; Erxleben et al., 2002; Molotch and Bales, 2005), in our study no strong association was found between SD and Elevation, with significant correlations occurring only during the snowmelt period. This is because of the low elevation range of the study area (300 m). During the accumulation period the entire catchment is generally above the freezing height. However, during spring the 0ïĆřC isotherm shifts to higher elevations, which may lead to different melting rates within the basin.". This way it is clear that not high correlations are obtained at Izas study site. Afterwards, it is compared with results in the same study site "Despite the relatively weak correlation between Elevation and SD, this variable was introduced as a predictor in the MLRs and BRTs for most of the days analyzed. Similarly, López-Moreno et al. (2010) reported that elevation was of increasing importance as the grid size increased. Anderton et al. (2004) also informed about the importance of elevation to explain snowpack distribution in the same study area".

R: L7-9: this is contrasting the statement in P1953 L 27 that says that the effect of elevation was low. please reformulate A: We agree with referee's comment, and this sentence has been reformulated into: "The results of the present study suggest the increase in importance of Elevation at the end of the snow season, and particularly when it is considered in combination with other topographic variables in MLR and BRT

models."

R: L10: slope-effects can also be reasoned by solar radiation (impact angle of sun), this should be mentioned A: This affirmation has been mentioned in relation with slope.

R: L 28 "with respect to both..:" A: This has been included in the final manuscript version.

R: P1955 L5-13: This is a little confusing: so this means that Northing and radiation could be used instead of Sx? A: We agree that the sentence was not clear, and as stated before it could confuse manuscript readers. This way it has been changed in final version manuscript, including the new results derived from referee's major comments. Please, see referee 2 response, where it is fully explained, and also contextualized with principal component analysis for avoiding multicollinearity effects and PCA analysis done for considering them.

R: L14/15 which previous studies? please reference and describe A: Two examples of previous works (Winstral et al., 2002 and Molotch et al., 2005) have been discussed: "The MLRs and BRTs provided reasonably high accuracy scores when observed and predicted SD data were compared. The scores were comparable, and in some cases better, to values reported in previous researches using similar methods. Molotch et al., (2005) reported r2 values between 0.31 and 0.39 with BRT; and Winstral et al., (2002), considering different number of terminal nodes of BRT with similar topographic variables, obtained an optimal tree size of 16 nodes (which is quite similar to the tree size selected in this study, in spite of differences in the study area, the nature of the dataset, etc) with an r2 value close to 0.4. Moreover results presented here were obtained from a separate dataset, and data used to create the models are not considered for testing, thanks to the large available data set. "

R: 16-17 another reason could be that the study areas differ in terms of complexity of terrain and accumulation patterns A: Yes, we agree with this opinion and it has been mentioned in the revised text: "One reason for the improvement may be the use of the

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TPI as a SD predictor, as this variable has not been considered in previous studies. Nevertheless, it should be noted that the study sites considered in other studies, could differ in terms on complexity of terrain, and also in SD accumulation amounts."

R: P1956 L3-4: I do not fully agree with this statement. I should be formulated more carefully 1) the models differ between the surveys and years - only some parameters are similar 2) you did not analyse the spatial consistency of the snow cover but the spatial consistency of the terrain control on the snow cover A: We agree with this comment and this sentence has been restructured according to this formulation: "In spite of model results differ between survey days and years, some variables are always present in the models and their contribution to the total explained variance are rather similar. Moreover for 2012 and 2013 a consistent inter-annual distribution of the snow pack in the catchment is observed; the areas of maximum SD and the location of snow free zones were consistent between both years of the study, and more importantly there is an strong consistency of the effect of topography on SD is clear".

Referee #2

2. Major comments

2.1 Missing non-topographic information: wind, solar radiation and melt R: The study focuses on topographic influence only. Since the distribution of snow depth is obviously an interaction of topography and wind characteristics it would be a great benefit if wind information is included in the analysis. Some topographic variables are hard to interpret without the knowledge of the wind conditions beforehand. The analysis related to Figure 4 would greatly benefit if each direction correlates best is consistent with measured wind directions. If wind was measured at the weather station mentioned in Figure 1, a closer analysis relating wind with topographic characteristics is necessary. If not, modelled results of synoptic wind conditions could fill the important missing information. The TPI interprets the topography in all directions equally, while the Sx parameter takes sheltering effects relative to wind direction into account. Thus, the TPI

is not able to explain snow drifts in leeward and less snow in windward slopes. Including wind information in this manuscript seems necessary to explain why the TPI is so important. A comparison of wind speeds to other studies could explain if the found importance of the TPI is site specific or just because other studies have never used it. My interpretation of the results could be: The conclusion that the Sx parameter (see also comments below to the usage of this parameter) explains less than the TPI hints to a rather minor role of snow redistribution in this study. I would like to see a discussion on the wind/terrain interaction in this manuscript. A strict focus on the topography seems not sufficient in this context. A: We have taken into account these comments and the revised manuscript includes new information about wind direction and solar radiation. We fully agree that including wind roses (Figure 4 of supplementary material, which is the reviewed manuscript version) of the previous days to each snow survey permits a better explanation of many of the results discussed in this paper. We think that this additional information is very consistent with the results obtained from the Sx parameters, and also permits to understand the changing role of potential incoming solar radiation and northing as the snow season advance. Regarding the reached results about the influence of Sx and TPI on snow distribution, we have added the following discussion: "Sx parameter takes into account sheltering effects with topographic origin in relation to wind directions. As it has been observed in this study, higher SD amounts are observed in leeward slopes, which for this study site are in E-SE slopes, being perceived this effect in the SD distribution maps. TPI is not able to explain snow drifts, because this index considers the topographic characteristics in all directions. Nevertheless, terrain characteristics at the study site in relation to SD distribution have shown a higher importance of TPI when compared to Sx. The most likely explanation of this result is that the basin has a rather reduced size, shows the same general aspect (South east facing) and topography is relatively gentle. Under such conditions, during wind blowing events snow is accumulated in all the wide concavities of the basin (represented by TPI) independently of its specific location. Nonetheless, wind redistribution will be affected by a combination of local topography in relation to the main wind directions;

what makes necessary to consider the Sx parameter, and this effect lasts in time until the melting season is advanced."

R: Similarly, radiation was only modelled from terrain information assuming clear sky conditions. Measurements of solar radiation are needed to account for different melt rates. Winstral and Marks (2014) used modelled melt rates. Such a model attempt would lead this manuscript to a more quantitative interpretation of different influences found in the topographic variables (abstract lines 19ff). If the authors want to quantify the topographic control, I suggest to study this in relation to including or excluding non-topographic data and how the statistical models improve or deteriorate. If these measurements are not available, this study has a weakness in experiment design. Numerical weather models may close this gap. A: We have added to the manuscript the time series of net radiation in the revised manuscript in order to show the temporal variability in incoming energy to the basin. However, we think that the use of the variable "potential radiation" is correct and useful for the main purpose of this article, because it explain well the relative spatial variability of the incoming radiation (independently of the magnitude) during specific periods of time. In the revised version of the manuscript we have wanted to highlight that the main purpose of the article is to assess if the effect of different topographic variables on snow distribution (following a methodology widely used for this purpose so far) is constant in time at the intra- and inter-annual scales. The idea to apply methods shown in Winstral and Marks (2014) trying to quantify the influences of this control from non-topographic data is far of this scope, and we do not have the same data set to do this properly. In the paper we present extremely high density sets of snow depth measurements (from the TLS) for a rather nice temporal resolution (six surveys per year); and we want to focus the value of this work on this dataset. However, we do not have currently information of SWE variability across the basin, so the direct estimation of melt rates is not a reliable task. Thus, we have wanted to work with the very good observations that we have, instead of introducing simulated values that contain an uncertainty and they are not strictly necessary for the main purpose of this research.

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2.2 Sx parameter R: In relevant publications (e.g. Winstral et al. 2002, Winstral and Marks, 2002, Winstral et al. 2013) the Sx parameter was only used as an intermediate variable. Finally, only an averaged 'Sxdash' parameter was considered in their analysis. This one is averaged over a certain upwind window of 60 deg (Winstral et al., 2002). Later they used 30 deg (Winstral and Marks, 2002, Winstral et al. 2013). So I think the authors are wrong (p 1944, line 25ff: "Rather than considering the contribution from all directions at a specific location, adding all the Sx values for all directions for each cell (Winstral et al., 2002), ...". With this wrong argument the authors appear to use Sx instead of Sxdash to account for wind directions. I think Winstral's arguments to average over a certain upwind window to create a more robust parameter seems valid, given the large fluctuation of wind speed and directions in space and time. The sheltering effect will certainly be influenced by more then only one upwind grid point. Furthermore, the dominant wind directions may not perfectly be constant in such a catchment, which may increase the catchment-wide correlation of Sxdash with snow depth compared to Sx. Comparisons with other studies (Schirmer et al., 2011, Winstral et al., 2002), which have used Sxdash instead of Sx, are only possible if the same parameter is considered. I strongly suggest to include Sxdash for similar wind direction in this analysis. A: We really thanks this observation that results from a mistake in our explanations in the original submitted manuscript. Effectively, we did not specify that the averaged parameter was being considered, so this point has been clarified in the revised version. Accordingly, we have removed the sentence in which mistakenly was said "Rather than considering the contribution from all directions at a specific location, adding all the Sx values for all directions for each cell (Winstral et al., 2002)," And we clarify that the Sxdash variable has been used.

2.3 Statistical analysis 2.3.1 Reducing the dataset R: I understand the purpose of reducing the dataset for statistical tests. The number of observations is so large that statistical tests have too much power and are showing significant results for irrelevant differences. Thus, the authors reduced their data set with a Monte Carlo method, e.g. for testing if the population correlation coefficient p is significantly different from 0 given

the sample. However, I do not see a reason why the value of the sample correlation coefficient r should not be calculated for the whole dataset. To my opinion one is interested in the best estimation of the population correlation coefficient, for which in this case the largest number of observations should be best. I can see a benefit in a confidence interval around r in which p will be located. However, the mean, median and range of r values derived by subsamples does not make sense to me (Figure 4 and 5). The ranges is always quite similar in these Figures, which hint to me that there is no additional information (besides from that: for so many observations and for continuous data mean and median should be close to be the same and do not need to be mentioned both). I could not find any arguments in the cited book for this procedure (Hair et al. 1998). If the authors do, please cite these arguments with page numbers in case of citing a book. I have similar arguments for not reducing the dataset to build the linear regression function and the regression trees. For their stepwise linear regression a statistical test was used by the authors. As mentioned above, such a test would have too much power and select too many variables to be included, which leads to a overfitting regression function. The authors worked around this problem and reduced the dataset as well (lines 16f. p. 1946). I could not find this procedure in the cited book (Hair et al,1998). Similar to the correlation coefficient, I think the regression function is best when all data is given to the model. The overfitting problem can be solved if the argument for selecting a variable is not a statistical tests, but for example the adjusted rsquare or the AIC, which take the number of freedom into account. Another suggestion would be to separate feature selection (determined with subsets) and the determination of the final regression function (with all data). More problematic is the overfitting problem with regression trees, since it is not just a linear function which will be trained to fit the data. But instead of reducing the dataset, I suggest to apply a method proposed by Breiman et al. (1995, p.59 - 87, p. 241ff) to determine the model parameter "tree size". This is done with crossvalidation. In contrary, the stopping arguments used in this study are arbitrarily predetermined by the authors (lines 24ff, p. 1946) and therefore prone to overfitting. I cannot find such a procedure to determine the optimal tree

size in Breiman et al., (1995). A: As suggested by both reviewers, the correlation coefficient -r has been calculated for the whole dataset, instead using the reported Monte Carlo procedure. As suggested, we used the Monte Carlo approach to identify the statistical significance of the relations between snow depth and the different topographic variables, but the r-values have been estimated using the whole dataset. The use of this approach has provided very little differences in the results so it has not affected the conclusions from our analyses. As, now we have a single value of correlation for each topographic variable we have converted the original figures 4 and 5 in the new Tables 2 and 3 (see supplementary material of the pdf file Tables 2 and 3). We think that in this way we facilitate the reading of these results. Meanwhile, we have followed the reviewer's recommendation for applying the linear regressions and tree models, in which the whole dataset was used. For the multiple linear regression we used a reduced dataset (1000 cases) to identify which variables contributed to the model in a statistically significant manner. For this purpose, we used a stepwise procedure. Once the model was built, we used the entire dataset to obtain the definitive model. Again, very little differences have been found in the obtained beta-coefficients and the total explained variance of the model. In this way, the main results and discussion presented in the first submission has not been affected. Regarding the binary regression trees, it is important to note that the purpose of the analysis is not creating models for predicting snow distribution in unsampled areas, but simply to see how variance is reduced as the dataset is splitted according to different thresholds in the topographic variables. When predicting snow values is the main goal, overfitted trees is a serious problem to deal with this methodology (as stated by López-Moreno and Nogués-Bravo, 2006; López-Moreno et al., 2010), however in the frame of this research is not a critical issue as the first nodes explain by large the most of the variance and the last ones has a minimum impact in the results. For this reason, a cross-validation procedure is not strictly necessary. The procedure we used in this work looked for obtaining medium sized trees (with a maximum size of 15 nodes); which are quite similar to those obtained in other studies (Winstral et al., 2002) and of similar size for the different days. The criteria to

cease the growth of the trees was to indicate a minimum number of cases associated to each new node (500 cases) and a threshold of 1% of improved explained. Obviously, these criteria have a strong subjective component (as most of the thresholds we take in statistical analyses, including in a cross-validation analysis) but we think that they are adequate for the purpose of this research and they cannot derive spurious conclusions in the manuscript.

R: The presented tree in Figure 7b may be already at a stage of potentially overfitting to the data: TPI is in general negatively correlated to snow depth. For the subset which reaches the node TPI < -0.8 (bottom left of the tree) it is the opposite: Large values of TPI are an indication for larger snow depth. Similarly at the node TPI < 0.33 in the middle of the tree. While for me that is a sign of overfitting, the authors may have a physical argument why the general correlation may be reversed under the conditions determined by the nodes above. A: In the manuscript the trees are shown as simple examples of the technique and for this reason the peculiarities of the different splits are not explained in depth. Reviewer is true that this change in the sign of the TPI is contradictory, but the explanation is very simple. In general, the concave areas (TPI<0) accumulate deeper snowpack However, the mentioned positive TPI value falls in a branch that makes reference to the upper portions of the basin, and in this case is identifying the cornices that are slightly convex but, logically, accumulates a deep snowpack.

2.3.2 Interpretation of relative importance: Multicollinearity R: The relative contribution of explained variance by independent variables in linear regression is influenced by correlation between them: the relative contribution of two correlated variables should have be less compared to two uncorrelated. Also r square of a model with correlated independent variables will be smaller. This is well described in the cited book (Hair et al., 1998). The authors stated in line 12, p. 1946 that they account to this problem with stepwise regression. I think after stepwise regression multicollinearity cannot be excluded and should be studied and presented to interpret the relative importance of

variables (Table 2). A principal component analysis leading to uncorrelated input variables for the statistical methods would be helpful as well to assess how much variance can be explained by simple statistical models. The same arguments are valid for regression trees. An important input variable may not be selected by the tree algorithm since it is masked by one or more highly correlated other variables. An analysis of multicollinearity will help to interpret Table 3. A: We fully agree with this observation, and we have worked this point to present more clear information. As suggested by reviewer, PCA analysis has been applied to the topographic independent variables considered in this study (except Sx, because its direction changes for the different survey days and authors consider that should be always included). With this analysis when more than one variable exhibited very similar correlations with the PCs, we only retained for the multiple regression analyses the one with highest correlation, avoiding in this way problems of multicolliniarity. We have preferred to proceed in this way, instead of working directly with the PCs in order to obtain results easier to be interpreted. Following this criteria (see Table I of this discussion), TPI and Curvature were identified as potential for multicollinearity thus curvature was removed from the multiple regression analyses and binary regression trees. The other relevant association between independent variables is observed between Radiation and Northing that are oppositely correlated, the obtained Pearson's r coefficient are very similar but with opposite sign for all survey days. In such a way in MLR and BRT models created after the manuscript revision, neither Curvature neither Radiation have been used, because their contribution is respectively explained by TPI and Northing (note that these are better correlated with components 2 and 1 respectively). This result is not presented in the final version manuscript because simplicity is preferred. Nevertheless the main result is commented in 3.3 Statistical Analysis section as follows: "Prior to run the models a principal component analysis (PCA) was applied to the topographic variables for detecting correlations between independent variables that could originate multicolinearity in MLR and BRT. This analysis (not shown) grouped the topographic variables in three components, from which it is observed that TPI and Curvature are highly correlated, and also Northing

and Radiation (but in this case inversely) presented almost identical correlations with the three identified components. As TPI and Northing showed higher correlations with their respective components and also show in general higher Pearson's r coefficients with SD (see result section), the variables Curvature and Radiation were discarded as predictors in MLR and BRT analyses.".

2.4 Quantification of persistence R: Above I mentioned that the persistence of topographic variables lacks a quantitative analysis. Statistical models which are trained for the whole dataset (all days and all years) can be compared to single day models. Single day models can be validated not only against data of the same day, but against all other days to quantify persistence. Models from other studies can be compared quantitatively, can be applied to this dataset. This would give more value compared to the conclusion in line 14 p. 1955 ("The scores were slightly better than reported in previous research using similar methods.."), without reporting scores nor citing studies. A: In this study 12 survey days have been analyzed, with very different SD accumulations amounts. With the obtained results in MLR and BRT, some variables have been found to exert a strong control on SD distribution in mountain terrain, but their contribution varies amongst different days. The idea of training a model for the whole dataset (all days and all years), as suggested by reviewer #2, also was considered before the first manuscript version. Nevertheless it is out of the scope of this work to develop a predictive model for the study area, and it is preferred to analyze the exact contribution of each topographic variable for the specific SD distribution for all available days. In such a way, models are trained and validated for surveys specific dates, and this way authors are able to consider how important topographic variables are for the observed snowpack distribution in a day. This allows observing topographic variables importance or contribution changes during the year on explaining SD distribution, which is the main objective of the work. The idea to build models with all the available data (from different days) to test if it is possible to develop common models with some scaling factors for the specific snow conditions of each day is really interesting and indeed we had in mind to progress in our research in this way. In fact, we are still conducting our survey and

we would like to test this approach having at least 4-5 years of data to ensure that this technique works properly for years of very contrasted climatic and snow conditions. In the revised manuscript we report the scores obtained in previous research and they are compared to the scores obtained in this study as follows: "The MLRs and BRTs provided reasonably high accuracy scores when observed and predicted SD data were compared. The scores were comparable, and in some cases better, to values reported in previous researches using similar methods. Molotch et al., (2005) reported r2 values between 0.31 and 0.39 with BRT; and Winstral et al., (2002), considering different number of terminal nodes of BRT with similar topographic variables, obtained an optimal tree size of 16 nodes (which is quite similar to the tree size selected in this study, in spite of differences in the study area, the nature of the dataset, etc) with an r2 value close to 0.4."

2.5 Integrating studies from long-term snow observations R: Recently, a long-term data set on snow depth distribution was published using snow probes (Winstral and Marks, 2014). They refer to other long-term data sets (Sturm and Wagner, 2010; Jepsen et al., 2012). It would be a great benefit of this manuscript if the findings of these spatially limited observations are compared to this ALS dataset. Can their hypotheses be confirmed by this data set which covers a better spatial resolution, not only a different region? A: Authors consider that at the point in which this work has been presented, where two opposite climatic conditions winters have been analyzed, and in which the intra-annual persistence is also analyzed; is not of major importance to deeply consider long-term snow observations (indeed we do not have data for this purpose). The suggested references analyzed the variability between several years, and the expected snow distribution with different approaches. This could be easily related with the analyses that we aim to conduct in this area for the next years (see answer to previous point). Nevertheless, this works have been cited in the reviewed version manuscript: "There have been many studies analyzing the spatial distribution of SD in mountain areas (Anderton et al., 2004; Erickson et al., 2005; López-Moreno et al., 2010; Mccreight et al., 2012). Besides some researches have also focussed their attention in long-term

inter-annual snow distribution analyses (Jepsen et al., 2012; Sturm and Wagner, 2010, Winstral and Marks, 2014) but there are very few datasets that have enabled investigation of the intra- and inter-annual variability of the topographic control on the snowpack distribution, being important to investigate both time scales."

2.6 Figure captions R: Figure captions sizes. A: Figure captions have been improved and the most problematic figures concerning captions sizes have been removed and the information provided by them is given in tables.

3. Minor comments

R: Please mention page number in case of citing books (e.g. Hair and Zar). A: According to changes introduced in the revised text, these book citations have been removed.

R: Please provide a formula for the TPI. A: The formula to calculate TPI has been provided in the revised manuscript

R: Lines 18ff, p. 1943: Please provide the extent of the larger DEM, which is used for solar radiation, etc. A: The extent of the larger DEM used for calculating solar radiation and Sx is 1200 m from the exterior limit DEM obtained with the TLS. This information has been included in the revised text

R: Lines 6f, p. 1944: Please mention the window size which is used for curvature and slope calculations. A: These window sizes (10m) have been included in the text

R: Lines 18f, p. 1945: The authors excluded all search distances except of one based solely on the univariate analyis. In combination with others, important information may be lost in a multivariate model. While this may be the only practical solution, I suggest to mention univariate results between search distances, that the reader can follow this decision. Where the r values close quite similar between search distances? How easy was this decision to include only one search distance? Why are the search distances (25 m, 200 m) so different between the two variables? A: We have modified the text according to this suggestion. These searching distances were selected after considering

all Pearson correlation coefficients for all distances in all survey dates. As the number of results is really large, only the best correlated distances are presented. Except for the 02/04/2012 and the 24/05/2012; 25m searching distance for TPI and 200m searching distance for Sx were the distances with better results for all surveys. Note that the 02/04/2012, and the 24/05/2012 surveys days had the lower SD and SCA average values (Table 1), and for these days, slight differences were observed between 15m and 25 m distances for TPI and 100m and 200m for Sx. These correlations were done with the Monte Carlo approach of the early manuscript version, but using the whole dataset (as suggested by reviewers) the results have been the same As examples in Figure 1 of this discussion are presented correlations obtained for the TPI searching distances for the 02/04/2012 and the 03/04/2013 (a day with high SD accumulation). We consider enough to include a short comment in the revised version making reference to the obtained results of the best correlated directions

R: Line 10, p. 1946: Please mention how the coefficient were standardized line A: Standardized coefficients were automatically provided by statistical models software (IBMSPSS The revised text includes an explanation about how the standardized coefficients are obtained

R: 1ff, p. 1953: How can the authors define that snow "remains longer", "melts faster", instead of accumulates more or less. I would guess it is rather the latter since the authors stated that the TPI correlation decreased during melt. A: The phrase has been modified accordingly to the reviewer's suggestion.

R: And, Figure 3 is too small to identify enhanced snow depth concavities. A: We realize that the figure is rather small, but we prefer to keep the 12 panels in a single figure to save space and to perceive in just one sight the nature of these maps and the differences in the magnitude (even when it is not possible some details of the SD distribution).

R: Line 10ff, p.1953: Please check: I found in Schirmer et al. (2011) dmax = 300

m, not 200 m. In Molotch et al. (2005) I could not find a search distance specified while skimming through, searched also for "200" and found nothing. A: This comment is correct; in Schirmer et al. (2011) dmax = 300 m, not 200 m, also in Molotch et al. (2005) it is not presented any searching distance so this sentence has been changed and it is appropriately clarified.

R: Line 15f, p. 1955: "independent" dataset for train and test. I am not convinced that a randomly selected dataset from the same day is indeed independent given the high spatial correlation of snow depth in the first tens of meter. This problem is documented for correlated time series which increases artificially the randomly cross-validated quality measures (Elsner and Schwertmann, 1994). A similar problem may occur here as well: Nearby grid points have at this day an higher explanatory power. As mentioned above I suggest to test models against other days which would guarantee independent test and train datasets, or ensure that grid point pairs closer than tens of meters are not appearing both in the testing and training dataset. A: In the revised manuscript we avoid using the term "independent dataset". By using the random sampling to select the significant variables, we partially avoid the problem of using data excessively close each other in the testing and training dataset. We think that temporal correlation could also affect the suggested technique of testing models against other days.

References:

Jepsen, S.M., Molotch, N.P., Williams, M.W., Rittger, K.E., and Sickman, J.O. (2012). Interannual variability of snowmelt in the Sierra Nevada and Rocky Mountains, United States: Examples from two alpine watersheds. Water Resour. Res. 48, W02529.

López-Moreno, J.I., and Nogués-Bravo, D. (2006). Interpolating local snow depth data: an evaluation of methods. Hydrological Processes 20, 2217–2232.

López-Moreno, J.I., Latron, J., and Lehmann, A. (2010). Effects of sample and grid size on the accuracy and stability of regression-based snow interpolation methods. Hydrological Processes 24, 1914–1928.

Sturm, M., and Wagner, A.M. (2010). Using repeated patterns in snow distribution modeling: An Arctic example. Water Resources Research 46.

Winstral, A., Elder, K., and Davis, R.E. (2002). Spatial Snow Modeling of Wind-Redistributed Snow Using Terrain-Based Parameters. Journal of Hydrometeorology 3, 524–538.
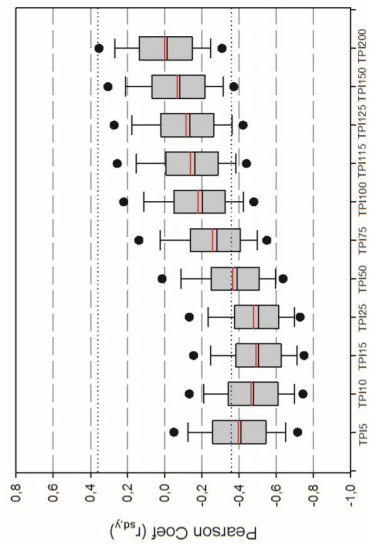
Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/8/C1160/2014/tcd-8-C1160-2014-supplement.pdf

Interactive comment on The Cryosphere Discuss., 8, 1937, 2014.

Full Screen / Esc

Printer-friendly Version
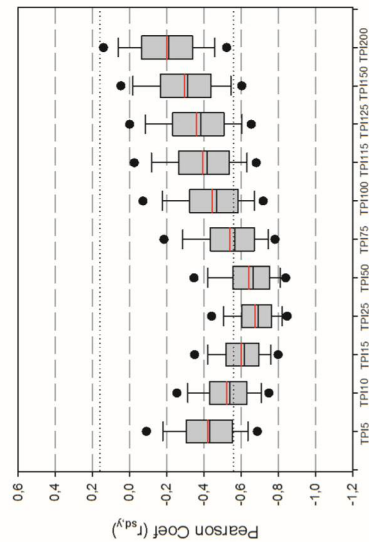
Interactive Discussion

Discussion Paper

Fig. 1.

	Component 1	Component 2	Component 3
Elev.	0,540	0,161	0,719
Slope	0,694	-0,062	0,521
Curv.	0,053	**0,886**	-0,125
TPI25	0,081	**0,894**	-0,004
Nort.	**-0,873**	0,066	0,475
East	0,249	-0,208	0,132
Rad.	**0,826**	-0,064	-0,426

Table I: Coefficients between topographic variables and principal
components from PCA.

Fig. 2.