Zaragoza, 12-08-2014

Dear editor, dear reviewer,

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 have been taken into account, what have resulted extremely useful for us. We have followed the majority of them, and we think that they helped to prepare a better manuscript, easier to be read 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.

Looking forward to hear your kind reply.

Jesús Revuelto and co-authors.

Review

Reviewer (R):
1 General comments
Revuelto et al. addressed most of my mentioned concerns. The presentation of wind measurements and the revised statistical analysis improved the manuscript. I understand the focus on the influence of topography on snow depth distribution and I think the focus on this topic alone is in general worth a publication. Some of my mentioned concerns are not well addressed, e.g. the quantification of persistence. The inter- and intra-annual persistence is a main conclusion in this manuscript although the chosen formulations are rather vague.
My major concern is the presentation. There are many hints that the manuscript is in a rather sloppy (type setting errors, missing white spaces, English grammar). I am not a native English speaker as well, but it seems that especially the new parts of the manuscript, and the discussion and the conclusion sections need editing.

Authors' response (A):
We thank the kind comments of the reviewer. We have done a new effort to be more concise in our argumentations, providing more quantitative information, and also improving the presentation of the manuscript by means of a carefully check of errors and a better edition of the grammar.

R:
2 Major comments

2.1 Quantification of persistence
In the abstract the authors mention their aim to assess if the contributions of topographic variables were variable over intra- and inter-annual time scales (line 30f). The chosen methods and results do not quantify the persistence, and thus all statements remain qualitatively and vague. For example in the discussion section line 559f ("...their contribution to the total explained variances are rather similar."). I can see that the TPI is consistently explaining snow depth (Table 3 to Table 5), but with a quite variable contribution (e.g. from 49% to 83% for the TPI in Table 5). What I do not understand is, if the presented differences between survey days hint to a variable or a persistent contribution? Are these values similar or different? Here the manuscript is not able to quantify persistence.
Similarly, the authors mention that the models were consistent in the conclusions section (line 581ff) and concluded that this suggests a consistent effect of topography. Indeed some variables are always present in the models (line 559), but is a standardized coefficient for the TPI between -0.4 and and -.78 similar or different, especially if the model's r2 is quite variable.
Since the time consistency of the influence of topographic variables is one main conclusion of this manuscript I expect a more detailed and quantitative investigation. This was already one of my major concerns in the first round. Here are some suggestions how this can be quantified:
• I understand that the authors do not want to present models that can be applied to other areas. They want to assess which topographic variable explains how much on a single day, and how does this contribution vary in time. This was mentioned as a reason for not training a model on one day and verifying the model on a different day, or training a model with all days

(global model). But to my point of view, these investigations show how the single day models differ in relation to a global model. It could answer the question, are the presented ranges of coefficient rather similar or different to each other.

• Another way to obtain a relation is using Monte Carlo methods. In the first version of this manuscript this was used for to obtain confidence intervals for correlation coefficients. However, confidence intervals for the presented regression coefficients would quantify persistence: This would give a relation between a confidence per day and the variance between days.

• How often is the TPI the first variable in the trees (both in already analysed single day models, and in random subsamples of a single day)?

• What are the average and the range of the TPI's critical value in the first node of the trees (both in already analysed single day models, and in random subsamples of a single day)?

…

Authors response (A):

In this new revised version, we have tried to highlight more this question in a quantitative and accurate manner. The role of the different topographic variables and their temporal similarities or differences is now explicitly mentioned in the abstract, results and discussion sections. The analysis considers how important topographic variables are depending on the specific snow distribution in different times along the analysed snow seasons with different statistical approaches. The majority of the figures and tables of the articles are comparing the accuracy of the different used models for the different surveys and how each variable contributes to explain snow distribution and if this contribution is constant or variable amongst the different surveys. We agree with the reviewer´s comment; we needed to provide more precise and/or quantitative information concerning how persistent is their contribution; or if their contribution was variable in the different calculated models. For this purpose, we have calculated the coefficients of variation (CV) and the mean values (from the 12 surveys) for each topographic variable and for each method used in this work (Pearson's r coefficients, MLR and BRT). This information is summarized in a new figure that shows CVs plotted versus the average value for the topographic variables. In this way, it is possible to check the relative importance of each variable (mean value) versus the variability in importance during the studied period (CV). In such a way, information about persistence is now provided. Line 427ff: "Figure 7 shows the mean contribution of each topographic variable versus the coefficient of variation from the twelve surveys for the different statistical approaches considered in this study (Pearson´s r coefficients, beta coefficients of the MLRs and the contribution to the explained variance for each BRT). Clearly, *TPI* is the most important variable to explain the snow distribution in the catchment, but it is also the variable that exhibits a lower variability between the different surveys (CV<0.2). Besides it has been introduced as predictor for MLRs and BRTs in all studied days. *Sx* is the next variable in importance to explain snow distribution, being introduced as predictor in the majority of the modelled days (11 and 10 out of 12 days for MLRs and BRTs respectively). It shows a low temporal variability when correlation´s coefficients are calculated (CV=0.24), but the variability in its contribution to MLRs and BRTs increases noticeably, with CV values of 0.35 and 0.59 respectively. The rest of the variables show a much lower mean contribution for explaining snow distribution and a high temporal variability in their explanatory role. Lower CV values are observed for MLRs, ranging the majority between 0.3 and 0.4, than for BRTs models, ranging the majority between 0.4 and 0.8."

Moreover as suggested by the reviewer, it has been provided information of how often TPI is the first variables in BRTs, and also the average and the range of the TPI critical value in the

first node of each tree. Hence it has been included in line 403ff: "Besides TPI was in all cases the first split variable (which accounted from a 23 to a 30 % of the explained variance), with a critical value that ranged from -0.671 m to -0.473 m and an average value of -0.54 m." for the 2012 year. For the year 2013 in line 413ff: "For this year, also *TPI* was the first split variable in nearly all BRT, with critical values ranging from -0.47 m to -0.21 m and an average value of -0.28 m; except for the 13 February 2013, in which *Sx* was the first split variable."
Concerning the specific sentences referred in this section by the reviewer, they have been changed as follow:
Line 559 (now line 577): "Despite model results differ between survey days and years, the most important variable, *TPI*, is always present in the models and their contribution to the total explained variances show very low CV values."
Line 581 (now line 604): "The results from BRTs and MLRs models were consistent in terms of variables importance ranking, and the explanatory capacities of the main variables were similar for all surveys. Except *TPI,* that showed very low coefficient of variations for the two approaches, the variability of the contribution of each topographic variable for the different surveys was noticeably lower for MLRs than for BRTs."

2.1 Presentation quality

R:
2.1.1 English grammar
Especially the new added paragraphs and the discussion and conclusion sections need editing. There are many long sentences which are hard to follow (e.g. line 536-540). Quite often the grammar is not correct, e.g. in line 519ff : "… the correlation …prevented us of potential problems of multicollinearity."

A:
English grammar has been checked for eliminating errors. In such a way, the cited examples now are stated as follows:
Line 536 to 540 (now line 555ff):" The scores were comparable, and in some cases better, to values reported in previous researches using similar methods. As an example, Molotch et al., (2005) reported r^2 values between 0.31 and 0.39 using BRT; and Winstral et al., (2002), who considered different number of terminal nodes of BRT, obtained an optimal tree size of 16 nodes data set with an r^2 value close to 0.4."
Line 519ff (now line 538ff): "Moreover, the high and opposite correlation between *Northing* and *Radiation* obtained in PCA results (not shown in the manuscript), was showing a potential problem of multicollinearity."

R:
2.1.2 Imprecise argumentation
In the following are only some examples presented, this is not a complete list:
The first sentence of the conclusion is not precise (line 572f), since only the TPI of one search distance was studied. All other search distances and also the curvature were excluded before the analysis.

A:
Accordingly, we have changed this sentence as follows (line 593ff): “The *TPI* at a 25 m searching distance was the best topographic variable, and the most persistent in time, for explaining SD distribution in the Izas experimental catchment.”
Nevertheless, in line 246 it was stated that a first analysis was done to consider the best correlated search distance: “The spatial scales of *Sx* and *TPI* for which SD showed a higher correlation; 200 m and 25 m respectively, were selected for further analysis (not presented in the manuscript).”

R:
Areas of snow free zones and areas maximum snow depth were not investigated, but mentioned in the discussion (line 562f) as a hint for time consistency.

A:
The discussion does not mention any analyses considering snow free areas (that might lead to spurious results in the statistical analyses as explained in methods section). We only frame some results when they are obtained at a time with an extensive snow cover, or when they are obtained at a time when snowpack is patchy. This information is obtained from table 1 and Figure 3.

R:
The last sentence of the conclusion is not very precise: “Several interesting temporal evolutions ….were found…” (line 588 ff). Which evolutions are meant here? I can recall the temporal evolution of the SX parameter, not more. They can be mentioned here.

A:
We have rewritten and shortened the conclusion section to be more specific and to send a clearer message to readers. Thus the referred sentence has been removed in final manuscript version and it has been included in line 600 ff: “Nevertheless, *Sx* has shown a similar evolution pattern for the best correlated direction in the two analyzed snow seasons.”

R:
The authors have stated in the conclusions (line 584f) that “…terrain characteristics have shown a major role on snow distribution, as TPI explanatory capacity”. While this sentence is another example of not good English grammar, I also wonder about the expression “major role”, since only terrain characteristics were studied.

A:
In line with previous comments, conclusions have been reformulated to gain clarity.

R:
I mentioned Elsner and Schmertmann (1994) in my first review, which analysed cross-validation problems in time series. They do not analyse spatial autocorrelations as stated here (line 234ff).

A:
We really thank this comment for improving manuscript references. In such a way, this reference has been removed and Koenig, 1999 has been added (line 241).

R:
Line 303f: “Upper areas of the map”.

A:
Now it is stated as “northerly areas of the maps”

R:
Line 322f: I think the TPI showed the highest correlation for all days, not “nearly all”. See also Table 3. Similarly, I would say it is “significant on all days”, and not on “some” (Line 339).

A:
As correctly suggested by the reviewer, these points have been changed. Now these sentences are: (line 326)“The *TPI* at 25 m showed the highest correlation with SD for the 12 sampled days.”; and: (line 342) “In 2013 the correlation was greater, and was statistically significant for all days.”

R:
Line 412f: When was snow not mobilized and how can the authors determine?

A:
Snow was considered as not mobilized when no change in the main Sx wind directions was observed. In such a way this line is now written as: (line 419) “When snow was not mobilized for long periods by wind (no changes on the best correlated wind direction of *Sx* are observed), the SD distribution was more dependent on variables related to terrain curvature (*TPI* and *Curvature*).”

R:
Line 431-439: This was already stated in the first review: Please cite the few studies which were mentioned in these lines. The authors mentioned “few”, but cite only one.

A:
The argumentation of this paragraph was not appropriately specified, and as the reviewer shows, references were not appropriately given for a good understanding. This paragraph has been changed as follows (line 450 ff): “Many studies have analyzed the spatial distribution of SD in mountain areas considering both, intra- and inter- annual variability of the topographic control on the snowpack distribution (Anderton et al., 2004; Erickson et al., 2005; López-Moreno et al., 2010; McCreight et al., 2012). Other researches have also focused their attention in long-term inter-annual snow distribution analyses (Jepsen et al., 2012; Sturm and Wagner, 2010; Winstral and Marks, 2014). The results of these previous works have highlighted the difficulties in fully explaining the distribution of snow in complex mountainous terrain. In addition, results differ among studies, and suggest that different

variables govern the distribution of snowpack among areas as consequence of their different characteristics and geographical settings. These differences include surface extension, the altitudinal gradients, the importance of wind redistribution, the presence or absence of vegetation and the topographic complexity as concluded by Grünewald et al., (2013) in a study where seven study sites across the world were considered.”

R:
Line 451: Figure 3 is still not large enough that I can see snow in deep concavities. Maybe zoom into one example area.

A:
We would like to present this figure in the actual format for preserving temporal evolution of SD across the whole catchment. Figure quality in the uploaded version is really high, and allows zoom in without problems (also it has been uploaded as supplementary material). In any case, we are absolutely flexible to add any change in the figure if it is still considered by the reviewer or the editor.

2.1.3. Typesetting

R:
Although there will be a typesetting correction after this second review, this manuscript has at this stage many typesetting errors. Sometimes there is a white space between numbers and units, quite often not (e.g. line 460). In line 239 is a white space after the “<” but not before.
The averaged Sx parameter (which I called very sloppy “Sx dash” in my last review, mainly because I copied and pasted this text into a browser window), should be consistently called in the manuscript with an averaging bar over the Sx, and not just “Sx dash (Sx further on)” (line 214), since this could lead to confusion. Thus, it will be consistent with the original paper of Winstral et al. (2002).

A:
As suggested by the reviewer, typesetting has been deeply reviewed for improving manuscript quality. We thank the reviewer´s comments for helping in this issue.

2.2 Minor Comments

R:
I suggested in the first round that topographic variables can be studied in relation to other variables. This would allow conclude as in line 584f that “…terrain characteristics have shown a major role on snow distribution…” With the added wind analysis the authors have increased the value of the manuscript, although I would see a great value to include modelled melt rates, short- and longwave radiation in the analysis. Some points can be considered in this context:

A:
This question was addressed and justified in the first round of reviews. We did not introduce such changes because we considered that it was out of the scope of the manuscript and because the characteristics of our data set did not allow doing it in a reliable manner.

R:
2.2.1 “Net radiation”
The authors have added in Figure 2 net solar radiation. It is unclear if this integrates longwave radiation or only shortwave radiation. The new information was never discussed in the text.

A:
This has been clarified in the text as follows: (line 140 ff) “Regarding net solar radiation data (short wave), no measurements were available before December 2011, However the annual evolution has been tracked on Figure 2 (bottom) showing a clear increase of incoming solar radiation while snow season advance, with high variability due to meteorological factors.”

R:
2.2.2 “Potential radiation”.
Similarly it is not clear to me if the used radiation introduced in line 193 integrates longwave radiation or only shortwave radiation.
If this is only shortwave, what I assume, I disagree with the explanation of the authors in their answer, 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.” During days with cloud cover it is not a matter of magnitude only, since diffuse and longwave radiation alter the spatial distribution of available melt energy.

A:
We have clarified this point with the following text (line199ff): “This algorithm calculates the potential incoming solar radiation (short wave) under clear sky conditions, which may strongly differ from the real radiation as a consequence of cloud cover. This measure provided the relative difference in the extraterrestrial incoming shortwave solar radiation among areas of the catchment for a given period under given topographical conditions (Fassnacht et al., 2013). In this way, *Radiation* can be considered as a good proxy of the spatial distribution of incoming solar energy within the study area.”

2.2.3 Line by line
R:
Line 136ff: What is the average number of observations set for a single day (to get an impression how small the 100 “SD cases” in a subsample is)?

A:
The average number of cases is 20,000, and it has been included as follows (line 242): “…Monte Carlo procedure, in which 1000 random samples of 100 SD cases were extracted from the entire data set (an average of 20,000 SD measurements for each day) and correlated with topographic variables for assessing significance.”

R:
Line 251ff: Not clear if the PCA was done for each day or for the global data set.

A:
It was done with the global data set. Thereby this line states now (line 257ff): “Prior to run the models a principal component analysis (PCA) was applied to the entire data set for detecting correlations between independent variables that could originate multicolinearity in MLR and BRT.”

R:
Line 301: Figure 4 is mentioned before Figure 3.

A:
Now Figure 3 is mentioned before, in Snow depth measurement section.

R:
Line 312-315: Only a replication of the table.

A:
In these lines are presented the main results of the referred table for highlighting main wind directions of Sx parameter. We want to maintain it, because are the only Sx wind directions considered in MLR and BRT.

R:
Line 375ff: “Good agreement between models”? Do the authors mean a range between .25 to .65 of the r2 is a good agreement?

A:
We have removed this sentence because it could result confusing. In the rest of the manuscript, the accuracy scores of the models are presented in a quantitative way (without specifications if models are good or not), and comparing the values with other studies.

R:
Line 444ff: The spatial scale of curvature and TPI is with 10 m (4 data points) and 25 m (25 data points) maybe too similar to come to this conclusion.

A:
Both topographic variables are considering curvature in different scales, being smaller the spatial scale of Curvature than the one of TPI at 25 m. In addition, several arguments are provided here for reasoning the obtained result as follows (line 466 ff): “*Curvature* (which refers to a smaller spatial scale of terrain curvature when compared with *TPI*) is also highly correlated with the SD distribution, but not as much as *TPI*. This reinforces the importance of considering terrain curvature at various scales for explaining the SD distribution in mountain environments. The correlation between snowpack and the *TPI* decreased during melting periods, whereas the correlation with *Curvature* remained constant. This suggests that snow accumulates more in small, deep concavities, but is shallower at the end of the season in wider concave areas that were identified by the 25 m *TPI* scale.”

R:
Line 190ff: As far as I know, ArcGIS takes for slope and curvature a 3 x 3 window. This would lead in the case of a 5 m resolution to a 15 x 15 m window.

A:
Yes, reviewer is right and we have corrected it accordingly.