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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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The paper "Topographic control of snowpack distribution in a small catchment in the central Spanish Pyrenees: intra- and inter-annual persistence" by Revuelto et al. presents an analysis of the correlation of topography and snow depth in the Izas experimental catchment for multiple surveys in of two years. In total, 12 snow depth data sets have been collected by terrestrial laser scanning. Snow depth is analyzed in a grid resolution of 5m and related to several standard topographic indexes (slope, elevation, aspect, radiation, Sx, curvature) and a topographical position index (TPI). Correlations of each of the parameters and snow depth of each survey were performed separately

and the parameters were combined in multiple linear regression models and regression trees. The methods are valid for the purpose and are representing standard approaches that have been applied by numerous previous studies. The main results are that the best variables and their predictive powers vary between the surveys. The TPI appears to be the best predictor for snow depth. About 50% of the snow depth variability could be explained by the regression models. Revuelto et al. present a conclusive case study on an important topic for snow hydrology and related fields that is well worth publication in TC. Similar case studies exist but are still rare. The structure of the paper is good and methods are mostly described adequately. Results and their interpretation are mostly coherent and supported by the data. However, I believe that parts of the paper could further be improved in a careful revision. Especially parts of the results and discussion section are little confusing (see detailed comments). Restructuring/ rewriting of these sections could improve the readability. Moreover little more effort should be performed to relate the presented results to other published studies. Finally, I think that the paper could profit from language edition.

Major comment:

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).

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.

Minor comments and technical corrections:

p1938 L4: state that all 12 campaigns were in winter

L9: best instead of better

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

L16: remove "most"

L22 remove "distributed"?

P1939 L23 most studies abbreviate Lidar with "Light detection and ranging "

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.

L12 "such as" instead of "including"

P1942L7: where was the "historical data set" measured at the meteorological station indicated in Fig 1 or somewhere else?

L23: In this context I would remove Schirmer and Lehning (it is a companion paper of

Schirmer et al. that uses the same data set) and add Egli et al. 2012 and Mott et al. 2013 instead.

L26: here you should cite Grünewald et al. 2010, Prokop et al. 2008 and Schaffauser et al 2008

P1943 L3 what is meant by "TLS instability"

L5 please name the most important points of the protocol (very briefly)

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?

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);

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 by different methods can vary strongly.

P1944 L8: Curvature in which direction - horizontal, vertical or a combination?

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)

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.

P1945 L1: are all those wind directions present in the study area? Only such directions

should be used.

L18 how where they (sx and TPI) selected? I guess that analysis for all variations were performed but only the two are presented?

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.

P1947 L5 (also Fig 7): Were multiple or only 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)?

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.

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.

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.

P1949 L18 what about the accumulation season?

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.

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

Fig 7;

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.

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.

P 1953 L2 - I think it should be "wider convex" not "wider concave"

L26 "altitudinal differences" instead of "altitudinal gradients"

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"

L7-9: this is contrasting the statement in P1953 L 27 that says that the effect of elevation was low. please reformulate

L10: slope-effects can also be reasoned by solar radiation (impact angle of sun), this should be mentioned

L 28 "with respect to both..:"

P1955 L5-13: This is a little confusing: so this means that Northing and radiation could be used instead of Sx?

L14/15 which previous studies? please reference and describe

16-17 another reason could be that the study areas differ in terms of complexity of terrain and accumulation patterns

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

Additional Literature mentioned in the review:

Lehning, M., Grünewald, T. and Schirmer, M., 2011. Mountain snow distribution governed by an altitudinal gradient and terrain roughness. Geophys. Res. Lett., 38.

Mott, R., Gromke, C., Grünewald, T. and Lehning, M., 2013. Relative importance of advective heat transport and boundary layer decoupling in the melt dynamics of a patchy snow cover. Advances in Water Resources, 55(0): 88-97.

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