

Interactive comment on “Statistical modelling of the snow depth distribution on the catchment scale” by T. Grünewald et al.

Anonymous Referee #2

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The paper analyses a very good snow data set from airborne laser scanning in different mountain environments in Europe and North America. Using multiple linear regression, authors study which topographic variables control the snow distribution, and if this control is transposable in time and space. I think that questions that authors are trying to solve are very interesting for many researchers, and the paper is clear and well written. Following, I suggest a number of questions that authors may consider or discuss before the acceptance of the manuscript.

1. In the page 3250, it is said that used aggregation follows the concept of HRU. This is true for the first approach (the one as performed by Lehning et al. 2011), but not for the second approach. From my understanding, the later is simply a degradation of the original raster, but it is not based on common characteristics between the averaged

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cells. I think it should be clarified in the text, and I would suggest to remove the mention to HRUs to avoid potential confusion. In the same way, all presented results are based on the second approach, thus I would recommend not mentioning the first approach (the one based on subjective impression).

2. The justification of the need of degrading the original resolution is well justified and suits well for the general objective of the paper. However, it seems to me that 400m of cell size is an excessive smoothing as much of the variance of the dataset is removed and the sample size is excessively reduced. Authors consider $N > 20$ (p3251, line 6) as the minimum size to fit robust regression models. However, I think that this number is excessively low, when more than 5 independent variables are used. I think results would be more robust using a higher spatial resolution (25-100 meters) and removing some outliers derived from questions related to cornices, etc. Even according to figure 5, I would use a cell size of 200 meters, as r^2 does not change too much and sample size would have been multiplied by four. I do not want authors will redo all analyses, but this question should be discussed in more depth and maybe some example on how the contributing variables changes as the cell size is increased. Could the used coarse resolution be affecting the finding of the inter-annual consistency?. Probably, it will also decrease as the used spatial resolution is decreased.

3. The way to proceed with the multiple regression analysis should be a bit better explained. Which procedure of introducing new variables was used (stepwise, other?). Moreover, I understand that authors have not used an independent dataset for validating the models. This could lead to an overestimation of the model's accuracy. This should be mentioned in the text.

4. When the calibrated model for a specific survey is used to predict the snow distribution authors used the term cross-validation based on a leave one out cross validation. I would avoid the use of the term cross-validation as it could result confusing and you are not exactly leaving one out the cases.

5. It is absolutely normal that models developed for one study site will not be adequate for other regions. Checking this point would be more interesting comparing neighboring valleys or areas of very similar climatic characteristics. In this case, the study areas are very different and it is normal

that the role of temperature, solar radiation or wind redistribution that are related with the different topographic variables will be very different among sites. I think that section 3.2.2 could be removed from the article.

I hope my comments will result useful for the authors.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 3237, 2013.

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