Dear editor and reviewer 2,

Here is our response to your very helpful and constructive comment in blue. We responded point by point for every major and specific comments.

The paper "Snow mechanical properties variability at the slope scale, implication for snow mechanical modelling" present both experimental measurements and modelling results of the horizontal variability of mechanical properties and stability indicators at the slope scale, that is to say from 1 to 100m typically. The scientific question is of high importance as this variability can be of paramount importance for the avalanche hazard for two main reasons. Variability can lead to weaker areas compared to the mean properties of the slope. In case a skier (or other trigger) meet this area, it could trigger an avalanche that would not have been released elsewhere (the knock-down effect). Moreover, the variability of mechanical properties also influence the propagation of cracks in weak layer and could promote or arrest long propagations. The originality of this paper is to combine measurements and, from these measurements, a method to estimate the values anywhere in the slope by the inference of statistical relationships between some chosen predictors (terrain information, absolute position, snow depth, incoming solar radiations) and the mechanical and stability metrics. Both scientific question and used methods seems relevant and at the cutting edge of the avalanche hazard research field and adapted for the readership of EGU journals. However, the paper would benefit from additional efforts before publication as all elements are not provided to the reader to estimate the impact of such research and to reproduce the results. In particular, sections methods and discussion may be easily improved. I detail below my main concerns as well as some minor comments I identified while reading the paper.

Main comments

The main limitation to estimate the impact of this research is that the model transferability is not addressed in the paper. Being able to estimate the horizontal variability of mechanical properties in a slope is of very high interest for the community. However, the impact of the method depend on the minimal set of knowledge to be able to apply in a different situation. It would be interesting to discuss these requirements of the method in the discussion for better reuse of the results.

This comment was not specifically address but we responded to every others comments that we think is responding to this comment. We added a lot of information on the method, especially about the covariates (request by reviewer 1), and also a significant amount of information was added in the discussion related to the methods and the covariates selection, differences between forested/alpines areas and persistent/non-persistent.

In relation with the first point, a 10-fold cross-validation is used to estimate the error. However, in the paper, you point out that mechanical properties are correlated in space (and measure a correlation length). Hence, a random draw of an evaluation group does not seem sufficient to be able to have an independent evaluation set. It would be necessary to ensure that points from evaluation and training sets are at least spatially separated by a correlation length (or more). This may introduce complexity in the method but ensure a stronger evaluation. In any case, a discussion of the impacts of chosen evaluation method would be welcome.

It is a very good comment and close observations could bias the error estimation. We added two sentences in the discussion to elaborate on the impact of the chosen evaluation: "The cross-validation procedure was made by randomly selecting 10 subset, but the random selection could take into account a minimum distance between obseration (i.e our correlation length) to ensure complete independent subsets before computing the RMSE and MAE. However, our 10-fold cross-validation still provides a reliable estimation of the performance of our model but future work should take this into account."

When studying correlation lengths of the values of mechanical properties and stability metrics, you used the R function to perform a fit. It would be interesting to know the model used (function that is used for the fit) and provide the fitted parameters to quantitatively compare the results.

-We added the sentences "Four different types of covariance models (Gaussian, Exponential, Spherical, Matern) were fitted to the experimental variogram using iterative reweighted least squares estimation with function fit.variogram from the gstat package in Rstudio".

It would also strengthen the results. On Figure 3 and 4, it would be possible to plot a vertical line for correlation length. It would also be interesting to provide a reproducible table of fitted values (at least the correlation length) that is extensively used.

- We added in the Figure 3-4 a vertical line corresponding to the correlation length of the fitted variogram. We also added the model of the variogram that was fitted.

On Figure 3 and 4 it may also be possible to provide a small insert on each graph to represent the log-log variogram and provide data for the fractal dimension.

Unfortunately, it is not possible to add a small insert into Figure 3-4 for clarity issue. We added a figure with the log-log variogram in the appendix (Figure A1).

The results are convincing for the mechanical properties but I wonder what could be the use of critical crack length and skier crack length with less interesting results. Could you comment it in the discussion? Moreover, it is possible to imagine two ways of inferring stability indices: it is possible to infer from mechanical variables or to use a statistical model to predict directly these final variables. Could you comment the choice you made ? There is good reasons to choose one or the other, and it could also be interesting to compare both methods.

It is a very interesting comment but the goal of the study was to use GAM models to estimated snow properties and stability metrics with microtopography. We think from the results we showed that the spatial prediction of the snow properties is reliable to analytically computed the stability metrics directly from the estimated surface of snow properties. We also showed that the spatial predictions of stability metrics are less reliable and reinforce the point to change the method to spatially predict stability metrics.

We suggested in the discussion that future work should try ton infer directly from spatial fields of mechanical variables:"Future work could use spatial estimation of the snow mechanical properties and then compute directly the stability metrics from the spatial field without the GAM spatial modeling".

The GAM model is evaluated on the basis of maps and scoring of Fig. 6 and 7. However, the choice of covariates and what do we learn from the frequency in GAM may be of interest for further use of such techniques. In particular, authors chose a set of covariates and perform different tests (e.g. with different moving windows for TPI and VMR). I would be interested in recommendations from the authors on choice of covariates for further use of similar methods.

A complete paragraph in the discussion was added to discussion (also requested by reviewer 1) about covariates and the different scale used in this study.

The discussion is quite short and do not discuss the relative interest of the information provided by measurements and by the GAM modelling. The study gather a large amount of data and a brief summary of the main guidelines of the studies would help the reader at the beginning of the discussion (the start seem quite steep for me).

We added three sentences at the beginning of the discussion to briefly remind the reader of the main guidelines of the study : "This study gathers a unique dataset describing the spatial variation of snow mechanical properties and stability metrics at four different study sites. The comparison of the variogram and fractal dimension demonstrate that the slab properties (depth and density) vary at a smaller scale compared to the weak layer properties and stability metrics (smoother pattern). Spatial GAM modeling was used to spatially predict with good accuracy the snow mechanical properties using microtopography and with less precision, the stability metrics."

Additional minor comments are detailed below:

page 1, line 10-12: "snow mechanical properties [...] were measured". SMP does not provide a direct measurement of density or elastic modulus. Maybe it would be better to use the word "estimated" rather than "measured". Same remark for page 3, line 67.

We changed the word measured for estimated, also suggested by RC1.

page 1, line 16: I am unsure whether the mention to log-log variogram is useful in the abstract, especially as it is not shown in the article.

We changed for fractal dimension.

page 1, line 19: VRM is not defined in the text before page 24. It would be better to define at first occurrence and at least in the methods section. Moreover, an very short reminder of what are VRM and TPI variables would be welcome.

This sentence is not longer in the abstract, also suggested by RC1.

page 2, line 42: lacking space before parenthesis.

Fixed

page 7, line 183: The method used to identify the weak layer and the influence of this expert identification on the results may be enhanced. We have very few information on how this have been done, how this choice can impact the results and what consequences do this manual interaction have on the transferability of the method.

We added two sentences to describe the procedure to identify the slab and the weak layer.

page 7, equation 1: replace "+-" by "-".

Fixed

Table 1: How is justified this choice of variables ? In particular, the use of variable xy limits the transferability. It would be interesting to understand what lead you to this presented set of variable.

The choices of these covariates is based on multiples studies who link the microtopography to snow depth. The second paragraph on the section 2.5.1 covariates processing.

We added a sentence to explain the choice of the spatial coordinates and a reference: “The fitting of a smooth function, explained below, to spatial coordinates will take into account the residual spatial autocorrelation (Nussbaum et al., 2017).”

Page 13, line 326: Isn't there also low correlation length for slab density?

Yes and the following sentence stated that slab density has a small correlation length.

Page 13, line 331: Please define in the methods clearly the method to compute the correlation length and show it on the plot as Figure 3 does not allow to clearly know the correlation length for slab density at JBC22-SH.

A vertical line was added in Figure 3.

The method was defined the method section: We added this sentence : “Four different types of covariance models (Gaussian, Exponential, Spherical, Matern) were fitted to the experimental variogram using iterative reweighted least squares estimation with function fit.variogram from the gstat package in Rstudio (R core,2013) .”

Figure 5: Could you provide the number of elements in each boxplot ? It may be interesting to provide a similar figure for correlation length.

The number was provided in the figure title, which is the four surveys.

Page 17, line 360: The percentage of deviance is not defined or presented in the methods section.

We added the sentence in the method section: "The percentage of deviance explained (sum of squared errors) is computed to demonstrate the amount of total variance accounted by the model, this metric is more suited for non-linear model compared to R^2, which is still shown in the results for comparison."

Figures 7: The computation of lsk and ac with the analytical equation suppose that the snowpack is sufficiently homogeneous on the horizontal axis. From what I see on the figure, the computed values are here relatively low compared to the scale the model is applied. However, such a check may be important to mention for further use of this method.

We added the sentence after the sentence stating a 0.5m resolution in method section: "A smaller resolution will not be in line with the assumption of homogeneous snowpack for the computation of the skier crack and the critical crack length."

page 23 line 431: the usefulness of dataset EP20DF and EP19FC are not fully clear for me. Results are not shown, so we do not have an idea of the performance the method could have on such different areas.

We changed that and presented the datset EP20DF and EP19FC in the method section and in the first section of the results.

Page 17, line 375-376: Could you identify the outliers and the two weak spots (I clearly see on the north side but I am unsure of the second one you identified).

We added to the sentence "two major weak spots on the north side (right) and north-west (upper-middle)".

Page 17, line 383-384 and page 19 line 401: How do you explain that snow depth is not an interesting predictor? Maybe the dataset is too homogeneous ?

It is difficult to understand why the snow depth was not as used as others covariates. It is the most surprising results we had.

We added two sentence in the manuscript to comment this results: "Snow depth was only a used to predict the slab depth and slab density but was never used, in all four surveys, to predict the shear strength of the weak layer. A possible explanation on this result could be that weak layer spatial variation is not related to snow accumulation process, but it might also only be to our dataset being too homogeneous."

Page 22: A lot of use of 'Our result' or 'this result'. It is not always perfectly clear to what you intent to refer. In the same idea, line 426 and 429 you refer to Fig.9, maybe precise the variable you are interested in and/or add a) and b) to the two subfigures to point more precisely the data you want.

We changed the use of our results and this results to be more precise depending on each cases in page 22.

Figure 9 : You introduce a new dataset and new results in the discussion which is quite unusual. This may be moved in methods and results section.

The Figure was moved in the result section (now Figure 3) and the new dataset is present in the methods sections.