

Reviewer 1 comments and authors response

Paper title: Brief communication: Weak control of snow avalanche deposit volumes by paths morphological characteristics

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Paper number: tc-2021-103

We thank Reviewer 1 for his/her useful questions and comments on our manuscript. Please find below detailed feedback to individual comments and questions.

Major comments:

1. Reviewer 1 highlights that « *to emphasize that the purpose of this study is to examine morphological variables exclusively the authors should explicitly state this as the objective in the Introduction* »

We thanks R1 for this helpful suggestion. We will follow the recommandation and we will add the following sentence in the introduction to clarify the main objective of our study: « Here, the objective is to exclusively examine the relationship between avalanche path morphology and snow avalanche deposit volumes. »

2. Reviewer 1 highlights that « *My biggest concern is the limited scope of inference that using just morphological variables to predict deposit volumes provides. However, I think the authors clearly state this limitation in the Abstract and Discussion and this work provides a solid quantitative measurement of the influence of morphological variables on deposit volume.* »

We agree with the fact that the general opinion supposes that morphological variables of the path have a limited impact on deposit volumes. We conducted this analysis to confirm and quantify this *a priori*. Consequently, the scope is indeed, only limited to the control of avalanche deposit volume by avalanche path morphology. In the revised paper we will address this concern by making the goal of our study even clearer as it was. Specifically we will modify several points in our discussion to further explain that the avalanche path geomorphology is indeed a rather minor, yet significant factor, for predicting depositional volumes and suggest potential explanations (e.g. climatic contribution) for the remaining variability of deposit volumes.

Specific Comments

- *Line 14: The last sentence is a bit confusing in the way it is currently written. What do you mean by “weakness”?*

We want to express that overall, the power of morphological variables to predict snow avalanche deposit volumes remains somewhat limited. « weakness » will be changed to « limited » in the revised manuscript.

- *Line 28: Include “morphological” before “factors” to emphasize the use of morphological variables exclusively.*

Changes will be made according to the suggestion.

- *Line 29-30: Should read “snow avalanche deposits”. Remove “volumes”.*

Changes will be made according to the suggestion.

- *Line 41: Geometric size or destructive size?*

This refers to the geometric size, modification will be done.

- *Line 42: How was the depth of debris deposits calculated, specifically? Width and length seem relatively straightforward to estimate, particularly based on images after the event, but can you elaborate on how observers estimate depth from the designated vantage point(s)?*

The EPA operators are very familiar with the studied paths, including their snowpack-free morphology and systematically use the same predefined observation point, so as to maximize the accuracy of the estimation, especially the depth of the deposit. However, of course their visual estimate has some uncertainties. Two sentences will be added in the revised manuscript to clarify this point: « The EPA operators are very familiar with the studied paths, including their snowpack-free morphology and systematically use the same predefined observation point, so as to maximize the accuracy of the estimation, especially the depth of the deposit. The depth of the deposit remains however difficult to estimate as for safety reasons this is not based on direct measurements on the deposit.»

- *Line 59: Do you mean “including snow avalanche records for which we did not calculate volumes”?*

Exactly, changes will be made in the revised manuscript.

- *Line 60: Avalanche occurrence rates?*

Yes, modification will be made.

- *Line 63-69: The reporting of snow depth in this paragraph is a bit confusing to me. Is the mean annual snowpack at the end of February only 90 cm at 2740m? Then, on average, the snowpack height increases another 80 cm from March through the end of May to reach 170 cm? Line 63 states that the snowpack depth at this elevation regularly exceeds 200 cm, though. Please clarify.*

We did not specify that we are using mean values for the period 2003-2017. For example, the spring snowpack is on average 170 cm but regularly exceeds 200 cm. We will modify the text to clarify

these issues : « The data from two weather stations handled by Météo-France and located at elevations of 1715 m a.s.l. and 2740 m a.s.l. in Bessans for the period 2003-2017, respectively (Fig. 1), was analyzed in order to determine climate conditions having locally prevailed over the study period. This showed that the depth of the local snowpack regularly exceeds 50 cm at 1715 m a.s.l. and 200 cm at 2740 m a.s.l.. The winter (Nov-Feb.) season is characterized by a cold mean air temperature (-4°C at 1715 m a.s.l., -5.5°C at 2740 m a.s.l.), with heavy precipitation that nearly only fall in the form of snow but the mean depth of the snowpack remains relatively thin (90 cm at 2740 m a.s.l.). By contrast, the spring season is characterized by higher mean air temperatures (3.5°C at 1715 m a.s.l., -2°C at 2740 m a.s.l.) and the occurrence of significant daily warm spells (daily mean air temperature up to 25°C at 1715 m a.s.l), which favors the occurrence of rain on snow events and wet snow avalanches. The mean daily fresh snowfall is half as much as during the winter season, but, the mean snowpack remains thick (170 cm). »

- *Line 79: What is the exact accuracy of the DEM? 1m?*

Yes, we will remove meter to specify 1 meter.

- *Line 82: It seems that the shape of the starting zone (or thalweg of the starting zone) represented by convexity and/or concavity might be a useful variable as well. Did you examine this?*

No, but we totally agree with the referee, this is a potential area of improvement that we are taking into consideration. We are currently developing a GIS tool to clearly define the starting zones and consequently improve our study. However, we decided to not include this point because this approach is still in progress. Moreover we conclude in our work that no matter which descriptors are used, the control of the deposits volume by path morphology remains weak and for us our investigations are sufficient to state this with full confidence. To clarify this point in the discussion, we will add these sentences: « Additional morphological descriptors, such as convexity or concavity of the starting zone, could slightly improve the predictive power of the models. However, we suspect that no matter which descriptors are used, the control of the deposits volume by path morphology remains weak. »

- *Line 116: Change “carryout out” to “completed”.*

Change will be done

- *Line 130/Table 1: This table should probably be moved to the Results section.*

Indeed, we will move the table 1 to the Results section.

- *Line 141: Is this statistically significant? If not, consider using ‘substantial’ to avoid confusion.*

No this is no statistically significant, modification will be made.

- *Line 147: stronger? Perhaps ‘more abundant’ is a better word choice.*

Indeed, correction will be made

- *Line 150-151: I assume that each avalanche deposit volume is treated/measured individually as opposed to measuring the cumulative debris volume in paths with >1 avalanche over the course of the winter. Please clarify either here or in the Methods section how the individual avalanche height component in each volume calculation was separated from the cumulative height in paths where subsequent avalanche debris “stacked” on top of older debris.*

An estimation was performed by observed for each event to avoid the possibility of an estimation based on a superposition of several deposits. However, in rare cases, the estimations may be biased by a complex deposit superposition. A sentence will be added in the method section: « The depth of the deposit remains however difficult to estimate as for safety reasons this is not based on direct measurements on the deposit. This is especially problematic in case of pre-existing successive deposits, but observers try to take such effects into account as much as possible when providing their visual estimates »

- *Line 153: See my previous comment on Table 1 and since you reference the values in Table 1 here, Table 1 should be moved to the Results section.*

We moved table 1 to the Results section

- *Line 158-159: text reads “...positive correlation of frequency with min slope ($r = -0.24$ $p < 0.05$). This is slightly confusing because the r value is negative indicating a negative correlation. I read this negative r value as such: as frequency decreases, minimum slope increases. It seems that what you mean is that higher frequencies are correlated with lower minimum slope angle. Is this correct? Please clarify for the reader. Also, perhaps I missed it, but I don’t see frequency related to the morphological variables in any table in the main manuscript or supplement. Please clarify and consider including frequency correlation values in the table as well.*

Indeed, the negative r value indicates a negative correlation, modification has been made to clarify the sentence. We will add the frequency correlation to the morphological variables in the Supplementary table 1.

- *Line 161: change “slopes” to “slope”*

Done

- *Line 201/Figure 2: Please define ‘ $Y=T$ ’ for panels (g)-(i) for the reader in the caption.*

Y is corresponding to the predictor (combination of topographic variables) and T is corresponding to the target (observed deposit volume). This information will be added to the caption of figure 2.

- *Line 220: I understand that wind on a massif scale isn’t available, but can you provide more insight into prevailing wind patterns in this region in the context of your results?*

No, we do not have access to prevailing wind data. However several adjustments will be made in the discussion section to clarify how wind effects are considered in our study: « This correlation shows that winter deposit volumes may be influenced by prevailing climatic conditions.

Specifically, we suspect that the significant influence of orientation reveals wind impacts. Thus a prevailing wind from the west during the winter season may cause large accumulations of snow on the east oriented hillside, later favoring important deposit volumes. Such hypothesis remains however speculative without direct wind measurements at high elevations. »

- *Line 227-228: It is interesting that the slope angles differ slightly between the frequency categories defined here. As I previously mentioned, it may be worth examining the convexity as a function of frequency and deposit volume.*

As said before, we agree with you and will add a sentence in the discussion section: « Additional morphological descriptors, such as convexity or concavity of the starting zone, could slightly improve the predictive power of the models. However, we suspect that no matter which descriptors are used, the control of the deposits volume by path morphology remains weak. »

- *Line 243-244: As currently written, this sentence is a bit confusing. Do you mean that variations in snowpack characteristics due to changes in climate variables are more influential in determining deposit volumes than morphological variables? Please clarify.*

Indeed we speculate that variations in the snowpack due to changes in climate variables are more decisive than morphological variables to determine deposit volumes. However, we also want to mention the possibility that the roughness of the ground may have a direct effect on the snowpack characteristics, later influencing the avalanche characteristics. That is why we present this question as « an insightful perspective for further work ».

- *Supplementary Figure 1: should “O” be “W” in x-axis labels?*

Change will be done