

Review of “Brief communication: Weak control of snow avalanche deposit volumes by paths morphological characteristics”

General Comments

The authors present a study that examines the influence of select morphological variables on avalanche deposit volumes in 77 avalanche paths located in the French Alps from 2003 to 2018. They apply a variety of statistical techniques to examine which variables have the most influence and compare the efficacy of each technique to determine the one with the best predictive power.

The manuscript is organized and presented well but could use a final grammar/wording revision to improve the readability in places. The sequence of statistical analysis from the ANOVA to the stepwise regression techniques appear appropriate for this dataset and sample size. The interpretation is mostly clear (see specific comments/questions below). I understand that the purpose of this study is to examine morphological variables exclusively. I think to emphasize this the authors should explicitly state this as the objective in the Introduction. 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. It seems this work falls into the Brief Communication manuscript type by reporting on “novel aspects of experimental methods and techniques which are relevant for scientific investigations within the journal scope”. Therefore, I recommend publication after the specific comments below are addressed.

Specific Comments

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

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

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

Line 41: Geometric size or destructive size?

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)?

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

Line 60: Avalanche occurrence rates?

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.

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

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?

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

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

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

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

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.

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.

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.

Line 161: change “slopes” to “slope”

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

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?

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.

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.

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