Firstly, we'd like to thank the reviewer for the very detailed review and the comprehensive set of constructive comments which he makes. In the following text, we respond in detail to each of the reviewer's comments in the review manuscript as well as to the comments in the supplement.

1 Review comments

RC: Are the scientific methods and assumptions valid and outlined clearly? No, unfortunately an important part of the description of the method is only understandable for readers with in-depth background of the refe4renced works by Sappington (2007) and Wood (1996). The authors miss to explain in an understandable way for a common reader the difference between window, neighborhood window, kernel window, scale, multi-scale, and possible grid-size. However this is important for the reader to understand the limitation of the method finally.
As I understand, the authors use a grid with a resolution of 0.5 m and you look at a window of 3x3 grid cell. This mean you consider an area of 1.5 times 1.5 mˆ2 to evaluated? However in many case available grid data may have a much lower resolution.
The author indicate only briefly (in a figure caption) which "scale" they finally use.

AC: We fully agree with the reviewer that this part is not clearly written and understandable. The reviewer is right in his assumption that we use a resolution (grid size) of 0.5m and a window size of 3x3 pixels to compute roughness. For better understanding, we will avoid the terms scale, multi-scale and kernel window as they are not relevant to understand the method. As a result, we will delete lines 3 – 8 on page 5. However, we will add a sentence at the end of the section explaining that a resolution of 0.5 m is used throughout the entire analysis. Concerning the limitations of the grid size c.f. next comment.

RC: Does the author reach substantial conclusions? It would be nice if the author would discuss the limitation of their approach a little more. To which extent does the grid size influence their results, etc.

AC: We will add a paragraph in the discussion, detailing how grid size influences the generality of our results. Previous studies often used very coarse resolution elevation data which did not allow to draw meaningful conclusions about the effect of surface roughness. By using high resolution elevation models, we are sure to capture surface roughness down to a level of single rocks and boulders which are known to affect release area size. Therefore, we believe that the link between surface roughness and release area size, as shown in our paper, is dependent on highly detailed elevation models and may not be visible in coarser resolution DTMs.

* Is the description of the data used, the methods used, the experiments and calculations made, and the results obtained sufficiently complete and accurate to allow their reproduction by fellow scientists (traceability of results)? No, see comments above on scale vs resolution and window size.

AC: c.f. comments above.

Does the abstract provide a concise, complete and unambiguous summary of the work done and the results obtained? More or less, yes. Not sure what the authors mean with "The assessment of potential release area size is nowadays mainly based on terrain analysis; however, it is assumed that with increasing snow accumulation and the attenuation of terrain irregularities larger release areas may form." Practitioners don't use properly only terrain analysis to identify release area estimates.

AC: We agree with the reviewer that practitioners don't only use terrain for release area estimation. However, terrain analysis, such as slope maps, is an important part in the process of release area estimation.
Further, terrain analysis is normally based on DTMs acquired under snow-free conditions. However, a snow-covered winter terrain can significantly vary from its underlying, snow-free terrain. This may lead to different, and/or potentially larger release areas. Therefore we will clarify and change this part to:
„One of the major tasks in assessing the potential size of avalanche releases areas is terrain analysis which is usually based on DTMs of a snow-free summer terrain. However, a snow-covered winter terrain can significantly vary from its underlying, snow-free terrain. This may lead to different, and/or potentially larger release areas."

RC: Are mathematical formulae, symbols, abbreviations and units correctly defined and used? If the formulae, symbols or abbreviations are numerous, are there tables or appendixes listing them? No some mixing in use of symbols , e.g. dHS vs. HN in tables and figures. I Propose to use symbols according to Fierz, C., Armstrong, R.L., Durand, Y., Etchevers, P., Greene, E., McClung, D.M., Nishimura, K., Satyawali, P.K. and Sokratov, S.A. 2009. The International Classification for Seasonal Snow on the Ground. IHP-VII Technical Documents in Hydrology N83, IACS Contribution N1, UNESCO-IHP, Paris.

Ac: We will use HN instead of dHS throughout the entire manuscript (c.f. comments in next section)

2 Comments in supplement

In this section we will address the comments in the supplement which have not yet been answered in the previous section.

RC: p.3, l.3: There have been trials prior to that (season 1998/99)

AC: The reviewer is correct. There have been trials before 1998/99. The reference to the year 1998/99 has been erased. The sentence now reads: "Avalanches are artificially released by explosives".

RC: Table 2 and Figure 11: Here you use HN <=> dHS in tables. Actually HN is not defined

AC: We will use HN instead of dHS throughout the entire manuscript.

RC: Figure 6. This is an important scale which needs to be explained early on. Why 1.5 m (3times 0.5m ?) . What would change if different scales are used? This scale may have a large influence on the analysis and should not only be briefly named in a figure caption.

AC: We will add a statement in the method section about the resolution of the data used in the analysis. We will further avoid the term scale and only use the term resolution. Further, we will address the dependency of the results from resolution in the discussion section (c.f. comments above).

RC: p14, l.6: check sentence, not quite consistent, not clear what you mean here. did you or did you not compare release area size?

AC: Yes, we compared release area size to snow depth measured at the weather station. We will make this clear in the manuscript.

RC: p.14, l.9: Does not figure 10 suggest a nonlinear correctional? Would e.g. Spearman rank correlation a better measure?

AC: It is true that potential release area size does not necessarily increase linearly with snow depth. This is dependent from e.g. terrain characteristics. One could therefore also use a rank correlation factor such as Spearman. We addressed this issue by using logarithmic transformations of release area width in the regression. This also reduces the impact of large, non linear differences on the correlation and accounts for the non-linearity in the data.

RC: Figure 13: is this transition real for #200 or just an artifact due to missing data? May be indicate lack of data?

AC: The reviewer is right, measurements only exist for data points; the line in between indicates a lack of data. We will mention this in the manuscript.

RC: p.18, l.18: I don't get the connection here between smoothing and near ground surfaces? again were is the connection to smooth surfaces?

AC: We showed in our study that generally, more snow leads to a decrease of surface roughness and potentially larger release areas. However, we also observed large release areas where snow depth at the bed surface is low (= near ground) and surface roughness consequently still high, such as #726.

These avalanches, however, were characterized by large slab thickness, meaning that surface roughness at bed surface was covered by a thick layer of snow on top of it, levelling out the irregularities and forming a smooth surface at the top of the snowpack. In other words, the overlaying slab was thick enough to form a continuous layer and, ultimately, produce a wide release area.

This suggests that surface roughness at the top of the snowpack, rather than that of the bed surface is relevant for potential release area size. In this way, terrain smoothing is also relevant for slab avalanches that release on deeper layers in the snowpack where surface roughness may be still present, such as deeps slabs.

We will add this statement in the manuscript to better explain the link between terrain smoothing and roughness of the bed surface.

RC: p. 18, l.22: What is an important size?

AC: Release area widths up to 700m were observed for deep slabs. We will precise this in the manuscript.