We thank both referees for the constructive comments and the suggestions. We accepted their observations and responded to all their comments. We are confident that the overall quality of the manuscript has improved. Detailed responses follow.

**Reply to Referee #1**

*The paper discusses soil erosion caused by avalanches in steep alpine terrain. This is an important topic because only few studies have addressed winter soil erosion yet, but the importance and amount of erosion in winter may be significant.*

*Presentation of data I had difficulties to follow the presentation and discussion of the individual sample points. I think the results could be presented in a clearer way. For instance much text is used to describe and discuss the individual data points, but for instance a scatterplot is missing that shows the correlation of modelled and measured data. Furthermore, the caption of table 1 does not explain the meaning of the sample IDs. Hence it is difficult to read the table. I have several suggestions to clarify the text. Show a scatterplot with measured and modelled values so that the reader can see the strength of the relationship (or the lack thereof). Label the individual data points in the figure so that the reader can understand, which data points strengthen and which weaken the relationship.*

Thanks to reviewer 1 for acknowledging the relevance of the topic. We agree with the reviewer and will add a scatterplot with measured and modelled values (Figure 4 will be added in the new version) with labelled data points. We are convinced that the reader will more easily follow the discussion. Since we will add Figure 4 with the scatterplot, Table 2 will be consequently modified in order to avoid duplicate information.

*Improve fig. 3. E.g. add a topographic map, which would help to interpret the different LS values. Also improve the figure caption.*

Figure 3: We will add the background map and improve the caption.

*Improve the readability of the tables.*

The caption of Table 1 will be completely rewritten and integrated with the missing information about IDs. The same will be done for the remaining captions (both figures and tables). As the soil depth was constant (i.e. 10 cm) we will delete the column from Table 1 and insert the information only in the caption, in order to simplify the table and make it more direct and readable.

*If the results are presented this way, it should be possible to shorten the result section and to talk more about the overall finding and less about the individual data points. Right now, I find it difficult to follow the lengthy discussion of individual data points.*

We will try to simplify the section using the new figures.

*Clarity of writing*

*I got the impression that much of the writing throughout the paper could be improved. Below I give a number of examples, but I think it would be helpful if the authors worked carefully through the entire paper to improve the clarity of the text. The text also contains a number of typos that I cannot all mention here.*

We will try to improve clarity throughout the text. For example the introduction and results sections will be better linked, the introduction deeply modified, in order to present the results in a clearer way.

*Abstract: The abstract could be written in a clearer way. E.g. it is not explained what RUSLE is, hence the text cannot be understood by a wider readership.*

As also suggested by Reviewer1, the abstract will be completely rewritten (especially the hypothesis at the basis of the research) to make it clearer for a wider readership. The importance of snow-induced erosion will be underlined at the beginning of the abstract and the differences between the RUSLE and radionuclides estimates will be better described.

*Intro:*

*The introduction should be clearer. For instance, the Alpine factor W is not explained in a satisfactory way. It is simply mentioned that W was implemented. A clear introduction should better explain what W actually is and in which way exactly it improves the model.*

The introduction section will be deeply modified, e.g. we will underline the worldwide importance of soil erosion processes and particularly in mountain regions; we will add information on the research on snow-gliding with proper references and explain their relevance to our research. We will provide more details about RUSLE model, as previously done also in the abstract, and we will better describe the actual gap of knowledge introducing the relevance of our study. More details about the W factor (how it is calculated) will be given also in the introduction and its importance will be better described.

*l 81. I find the last sentence not very informative. We should be informed what was learned by the study of Leitinger and why this knowledge is helpful for the outline of this study.*

The sentence will be rewritten and their findings explicated.

*l 89. Similar to the previous point: as measured and Caesium-derived values were compared, we should be informed what the outcome was because this is an important justification for this study.*

The sentence will be rewritten with more details.

*l 95. I don't find the term snow bridge ideal because with this term I picture a snow bridge over a river, but here snow supporting structure from steal are meant. I suggest to find a different term.*

The term is present in the Swiss Manual for avalanche defense structures and indicates a specific type of defense (wooden crossbeam with vertical support) (http://www.slf.ch/dienstleistungen/merkblaetter/phpUiQM8a.pdf). However, we agree that it may be misleading with respect to recently published research. Therefore, we will change it into "avalanche defense structures". The points formerly called "SB" will be therefore named "DS".

*Methods:*

*l 125. It is unclear what an avalanche shed is*

L141: it is an avalanche shed, in the sense that is the top of an avalanche defense structure (a tunnel for the road).

*Results and discussion:*

*l 302: rather: feedback than chain?*

L327. Right, we will change "chain" into "feedback".

*l 311: rather marginally significant*

We will delete this sentence which was not fundamental.

*Figure 4: I think the quality of the figure could be improved. Labels could be larger and clearer, and colour is not necessary.*

Figure 4 (now 5) will be modified according to the suggestions of the reviewer.

*Conclusions: l 382ff. "three subareas were considered..." This information is rather method-style and does not help the conclusions much. I suggest to rewrite the conclusion in order to clarify the main findings and implications of this study.*

The section will be rewritten considering the requests of both reviewers (mainly the main findings will be more clearly listed).

**Reply to Referee #2**

*General comments*

*The presented paper quantifies winter erosion by comparing 137Cs measurements and estimates with RUSLE. It thereby (1) attaches importance to winter erosion in general and (2) proposes the use of a correction factor in RUSLE to account for winter erosion. Although the study is based on a relatively low sample size, the used methods, drawn conclusions and presented data should be seen as an important step forward in appropriate consideration of winter erosion in soil erosion calculations. The article includes relevant literature and discusses the limitations of the used materials and methods as well as presented results in a satisfying way. However, modifications in presentation of results and delineated conclusions as well as strong discussion of used materials and methods will be necessary to meet the high standards of NHESS. Finally, the paper addresses a very relevant scientific question and uses (partly) relatively simple methods (simple soil loss estimates with RUSLE) to point out the importance of winter erosion. This is a promising concept as it addresses a key point in natural hazard research: how to obtain reliable and generalizable results using simply available data. I recommend the manuscript for publication after moderate revision. My main concerns are: very clear presentation of results and the link to the drawn conclusions; inclusion of more external literature in the' Introduction' section (many citations from authors or co-authors) to better underpin the scientific relevance and gap of knowledge in soil science; further comments please refer to 'Specific comments' section.*

We thank referee 2 for recognizing the relevance of the research topic.

As required also by reviewer 1, we will almost completely rewrite the introduction trying to evidence more the gaps of knowledge and relevance of our research, the meaning and significance of methods, and the weakness points.

We will try to improve the clarity of the results and we will rewrite partly the conclusions, trying to link them more with the introduction and results sections.

New citations will be included. Details are given below in the punctual notes.

*Specific comments*

*Abstract:*

*- please revise / 'adapt' the abstract after modifications of the manuscript (suggestions below)*

The abstract will be reformulated, as required by both referees. The hypothesis at the basis of the research will be better presented, in order to clarify the aims of this work and make it clearer for a wider readership. The importance of snow-induced erosion will be underlined and the differences between the RUSLE and radionuclides estimates will be better described.

*Introduction:*

*- please include more external literature (other mountainous regions, no authors of this study included)*

Several references (less local) will be added:

Ackroyd, P.. 1987. Erosion by snow avalanche and implications for geomorphic stability, Torlesse Range, New-Zealand, Arctic Alpine Res., 19, 65–70.

Bell, I., Gardner, J., Descally, F., 1990. An estimate of snow avalanche debris transport, Kaghan Valley, Himalaya, Pakistan, Arctic Alpine Res., 22, 317–321.

Buehler, Y., Christen, M., Kowalski, J., Bartelt, P., 2011. Sensitivity of snow avalanche simulations to digital elevation model quality and resolution. Annals of Glaciology, 52(58), 72-80.

De Vente J., Poesen J., 2005. Predicting soil erosion and sediment yield at the basin scale:Scale issues and semi-quantitative models. Earth Sci. Rev. 71, 95-125.

Garcia Rodriguez J.L., Martin C., Gimenez Suarez C., 2012. Methodology for estimating the topographic factor LS of RUSLE3D and USPED using GIS: Geomorphology 175-176, 98-106.

Gardner, J. S., 1983. Observations on erosion by wet snow avalanches, Mount Rae area, Alberta, Canada, Arctic Alpine Res., 15, 271–274.

Heckmann, T., Wichmann, V., Becht, M., 2005. Sediment transport by avalanches in the Bavarian Alps revisited – a perspective on modelling, in: Geomorphology in Environmental Application, edited by: Schmidt, K. H., Becht, M., Brunotte, E., Eitel, B., and Schrott, L., Zeitschrift für Geomorphologie, Supplement Series, Gebruder Borntraeger, Stuttgart, 11–25.

Jomelli, V. and Bertran, P., 2001. Wet snow avalanche deposits in the French Alps: structure and sedimentology, Geogr. Ann. A, 83, 15–28.

Korup, O. Rixen, C., 2014. Soil erosion and organic carbon export by wet snow avalanches, The Cryosphere Discuss., 8, 1-19.

Litschert S.E:, Theobald D.M., and Brown T.C., 2014. Effects of climate change and wildfire on soil loss in the Southern Rockies Ecoregion. Catena 118, 206-219.

Prasannakumar V., Vijith H., Abinod S., and Geetha N., 2012. Estimation of soil erosion risk within a small mountainous sub-watershed in Kerala, India, using Revised Universal Soil Loss Equation (RUSLE) and geo-information technology. Geoscience Frontiers 3(2), 209-215.

Sass O., Hoinkis R., and Wetzel K. F., 2010. A six-year record of debris transport by avalanches on a wildfire slope (Arnspitze, Tyrol), Z. Geomorphol., 54, 181–193.

*- present briefly other 'commonly applied' methodologies to assess soil erosion in slopy terrain and state pros and cons of each method and reasons for your decision to use RUSLE (or at least comment on this in Results and Discussion)*

We will refer to a broad review in the introduction underlining that no fully satisfactory method can be identified, at present. We will provide a wider set of references for the RUSLE model, and gave more details on its significance and inputs. We will try to explain the complementary information provided by the RUSLE and radionuclides estimates.

*Materials and Methods:*

*- p1412, section 2.4 (line 18): the used RUSLE does not account for melting water (as it seems to me from your description); following the USLE procedure snow melt can be taken into account – please state why you did not account for snowmelt; at least discuss in the 'Results and Discussion' section – but it would also be nice to see what the impacts on your results could be: : : or do you have severe arguments of NOT using the suggested procedure on your study site? please comment on this.*

In the introduction we said that the snow-water equivalent can be computed, however we preferred not to use it for R calculation for the following reasons:

-it does not consider the qualitative aspect of snow precipitation, i.e. the fact that snowmelt is differed in time and does not have a direct and immediate effect on soil as instead occurs with rainfall (e.g splash effect). Therefore melting water from snow cannot easily be assimilated to liquid precipitation.

-not all melt water flow at the snow/soil interface. It is likely that a large proportion of melt water flows laterally within the snowpack and thus is not erosive.

Therefore, in order to avoid further uncertainties in the RUSLE model, we preferred the traditional approach for R calculation, because we wanted to keep the snow and water processes separate.

In the introduction section we will try to clarify the sentence providing additional literature.

*Results and Discussion:*

*- this section should be shortened (data is extensively shown in the tables) and more discussion should take place (as proposed above and further below)*

The result section will be shortened and a new scatterplot (new Figure 4) will be introduced as suggested by referee 1. The discussion part will be improved.

*- please briefly discuss sensitivity of each factor in the RUSLE; e.g. which effect on the results might a 10% change have? or which factors are the most decisive ones in your calculations, on which factors should the focus be on (in terms of correct specification); most readers might know this implications, but I think it is worth to mention in your paper as the results from RUSLE are an 'important driver' for your conclusions / results.*

We will briefly discuss this issue, as required, in the methods and results sections, starting from the accuracy values provided by Yoder et al (2001) and Bazzoffi et al. (2001) for the A estimate.

According to Yoder et al. (2001), the accuracy of the RUSLE estimate (A) is expected to vary depending on A magnitude, i.e. lower A values will have more uncertainty. In particular, the accuracy will be: ±50% for A < ca 2 t $ha^{-1}$ $y^{-1}$; ±35% for A between ca 2 to ca 7 t $ha^{-1}$ $y^{-1}$; ±25% for A ranging from ca 7 to ca 40 t $ha^{-1}$ $y^{-1}$. In our study the calculated A values ranged from close to 0 to a maximum of ca 17 t $ha^{-1}$ $y^{-1}$, but most of them fell in the 2-7 t $ha^{-1}$ $y^{-1}$interval. Therefore, in our case the estimated accuracy according to Yoder et al. (2001) is around 35% for the majority of sampled points.

Considering this accuracy, we calculated a variation range for each A computed in our study (e.g A + 0-35*A when A between 2 to 7 t $ha^{-1}$ $y^{-1}$) and compared these values with the $^{137}$Cs estimates.

We still observed a significantly higher value for $^{137}$Cs estimates, and three W ranges for the 3 subareas. This is an encouraging result from the practical application of the method proposed.


References

Yoder, D. C., G. R. Foster, G. A. Weesies, K. G. Renard, D. K. McCool, and J. B. Lown., 2001. Evaluation of the RUSLE soil erosion model. IN: Agricultural Non-Point Source Water Quality Models; Their use and Application. J.E. Parsons, D.L. Thomas, and R.L. Huffman, eds. Southern Cooperative Series Bulletin #398, July, 2001, ISBN: 1-58161-398-9. Available electronically at: http://www3.b.


*- p1416 line 17: how did you check statistical significance? based on values shown in Table 1 comparing the mean among subareas? add relevant information to table/text.*

Statistical significance was assessed with ANOVA, and this will be specified in the text.

*- p1417 line 18: how does LS change when using another cell size? might there be implications by using 1m or 20m cell size compared to the 10m cell size you have used? please briefly discuss in general the effects of using different cell size / DEMs*

We agree that a different cell size might change the LS values, but, in our study area, which is quite smooth with only few small-scale changes of topography, we preferred to use the available DEM (10m resolution), which is able to describe the terrain characteristic in a satisfactory way. Therefore, we expect the uncertainty in LS in our case to be acceptable. Moreover, a general warning on LS computation is about the cell size resampling, that may affect deeply the curvature of the DEM (Buehler et al., 2011) and should be therefore avoided.

*Conclusion*

*- I think this section should be thoroughly revised (it is more a draft of an abstract than conclusions) or skipped at all; I propose to 'merge' suitable content to the abstract (which should be slightly extended anyway); the 'Results and Discussion' section is clearly understandable and the main conclusions are already obvious at the end of this section.*

We almost will rewrite it according to the suggestions of both referees.

*Technical corrections:*

*- please improve / extend captions of tables and figures; I often had difficulties to find links or/and relevant information in the text*

We will introduce the scatterplot of figure 4 (new version) requested by referee 1 and make the captions more explanatory.

*- please check typos throughout the manuscript and ensure correct wording to improve*

The text will be checked thoroughly.