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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Impact of snow gliding on soil redistribution for a sub-alpine area in Switzerland" *by* K. Meusburger et al.

Anonymous Referee #2

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The topic of the study is the interesting question of how snow gliding impacts soil erosion. Although this question has been addressed in earlier studies and snow gliding is known to play a role, there is always room for improvements in quantitative research in this topic. The methodology of the study, by comparing measured snow gliding rates with measured erosion rates and modelled erosion rates is interesting, but has some weaknesses. My general feeling is that these weaknesses are not incorporated well enough in the presented analysis. The conclusions drawn by the authors and the quality of the results cannot be judged without a proper error analysis of their methods, as I will point out below. Unfortunately, I'm a little concerned that an error analysis will show much less convincing results, especially in the light of the small experimental dataset, so I cannot recommend publication right now. However, if after thorough error

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Discussion Paper

analysis of the data the results still hold, the authors should be invited to resubmit a manuscript.

The paper is well-structured and mostly well readable (although some sentences could be reformulated). It is written in a concise way. This is something to appreciate, although the paper should discuss more the measurement procedures and methods, because now the reader is directed too often to referenced literature to get important information.

Main comments:

1) First of all, the dataset is very limited. It consist only of a few measurement sites, in one Alpine valley, for one particular year (for snow gliding distances). Although the spatial extend of the measurements is difficult to enhance, I would strongly encourage the authors to continue the measurements of snow gliding distances to have at least multiple years in the analysis, where different soil states and meteorological conditions are present. The quality of the results would heavily benefit from this. I think a more evenly distributed range of slope angles for both north (currently 22-30 degrees) and south (currently 18-39 degrees) would be welcome. Especially the combination of error prone analysis methods and the sparse dataset is a bit troublesome in this paper.

2) The analysis should be enhanced with an error analysis. The authors are well aware that their analysis methods have some drawbacks (as they discuss in the introduction), but the rest of the analysis is presented without such error analysis. The choice of some of the coefficients for the RUSLE model (Equation 1) is somewhat arbitrary. This may cause the strong correlation shown in Figure 3 to be a coincidence. For example: Table 1 shows that snow gliding distances are larger for steeper slopes. Slope angle is also present in Equation 1. That means that if one of the other coefficients is too low, it will multiply with the slope angle, such that higher slope angles (and thus higher snow gliding distances) lead to stronger underestimated erosion rates in RUSLE. This will give a higher discrepancy with the observed erosion rate for steeper slopes and

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thus slopes with higher snow gliding distances. What I think is essential for the paper is that errors are explicitly taken into account in the analysis. So what are reasonable ranges for the coefficients use in the RUSLE model, and what error margin would this introduce in the RUSLE estimated erosion rates? Same for the measured erosion rate: what are typical errors in determining these? If you than take the difference, is the difference between both statistically significant? In this case, both Figures 2 and 3 can be amended with error bars and the regression analysis can be done by taking these errors into account, making the analysis much more robust.

3) Why are there 14 data points in Figure 2 (equal to the number of measurement sites) and only 12 in Figure 3? Figure 3 is the main figure, so why are only 2 of the 4 Alnus viridis sites shown? Also they should make sure that a reader looking at Figure 3 directly recognizes (either in the graph, or in the caption) that the regression equation is ONLY based on the grassland sites (black dots), ignoring the grey dots, without reading the manuscript.

4) p9514: please note that the north slopes have a smaller slope angle range compared to the south slopes. As shown in Table 1, steeper slopes also tend to have higher snow gliding distances. So the difference in explained variance of snow gliding distance by the friction coefficient for north and south slopes (80% vs 50%) is maybe not so meaningful in this light.

5) As I expect the manuscript to become much longer with an appropriate error discussion, the authors should consider removing the discussion about the SSGM model, as it's role here is not really clear. Moreover, the authors themselves are also not so certain about the quality of their results, given the conclusions in p.9519L7-15.

Minor comments:

p. 9512: After Equation 1, I think it's much better to just first introduce all variables in the equation and then discuss them more in-depth one by one. Now p9511L20-p9512L13 is a bit a mess.

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p. 9509: In the site description, please mention typical snow depths.

p. 9514: although most readers will know what is meant by the variable y, it's formally not introduced. Please use a symbol or abbreviation for snow gliding distance.

p. 9516: please also report regression coefficients for this weak correlation too!

Table 2: please report also the Konz (2009) values for South slopes, instead of only mentioning the average. This gives the reader also a sense of spread in the values, without having to refer to the original publication.

Fig 3: although most readers will figure out what x and y mean in the regression equation, they are not officially introduced. So please provide symbols for the quantities along the x and y axis, and use them in the regression equation. By the way, as you want to explain soil erosion as a function of snow glide distance, I would suggest to swap the x and y axis!

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 9505, 2013.