

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

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We also thank reviewer 2 for the valuable and concise comments! We will briefly address each main comment in the following: 1) We agree that the limited number of data points is troublesome, however, for all correlations presented in the manuscript we also provide the p-value, which accounts for the number of samples and as such is losing significance for a limited number of data points. It would have been desirable to have a more comparable range of the slope angles between south and north slopes. But, since the snow glide behaviour is generally very different for south and north slopes with comparable slope angles, we consider this of minor importance. 2) The reviewer addresses the issue of uncertainty related to the two methods. We agree that

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this issue would have required much more attention. We will add a separate section (see reply to reviewer 1), discussing the effect of the different errors. Moreover, we will follow the very helpful suggestions of the reviewer and include the errors in the regression analysis and the error bars in the graph. Here the reviewer addresses as well another point: whether a systematic deviation of one of the RUSLE factors e.g. LS-factor correlates with the snow glide rates. But comparing the sites h1-h3, which have similar slope angles and land cover but very different snow glide rates, can be hold against this point. 3) We apologise for the confusion with the data points: In fact, for 2 of the 4 *Alnus viridis* sites we did not have 137Cs measurements, which explains why the Figure 2 has 2 data points more than the Figure 3. We will improve the description of the study site in the revised version. 4) As discussed above it would have been desirable to have a more comparable range of slope angles between north and south slopes. Nonetheless, we think the variance of snow gliding between north and south (partly explained by the friction coefficient) is meaningful, since the effect of different slope angle is considered in the calculation of the friction angle through the deviation by the normal force F_n (see equation 5). 5) Reviewer 2 suggests removing the section on the snow glide modelling. However, in the light of spatially limited point measurements, we think it is important to give the reader an idea of the relative potential magnitude, the possible spatial variability and the relevance of the snow glide process. A detailed answer to each point addressed by the reviewer will follow in the final reply together with the revised version.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/10/C5131/2013/hessd-10-C5131-2013-supplement.pdf>

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