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Interactive comment on "Soil erosion by snow gliding – a first quantification attempt in a sub-alpine area, Switzerland" *by* K. Meusburger et al.

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We would like to thank the reviewer 1 for his in-depth comments and his interest in this study. The reviewer acknowledges the interest of the topic and its novelty. The main critic is related to the uncertainty of the RUSLE and the 137Cs approach and their resulting differences. We believe that we already highlighted the uncertainty involved well enough, even in the title. However, with the comments of reviewer 1, we think this could further be improved. We are convinced that the HESS readers will not draw false inferences from our manuscript but will realise the importance of a process that is so far not considered in soil erosion studies. Only a short reply to some of the comments is given here. A detailed list of changes will be provided after the closure of the discussion.

1) The reviewer addresses the uncertainties associated with the magnitude of winter erosion derived from the subtraction of 137Cs erosion rates from RUSLE erosion rates. We are well aware of these uncertainties and especially of the empirical character of RUSLE. However, RUSLE was successfully applied in many different environments all over the world and several parameters are adapted accordingly. We did a model comparison in our alpine sites (Konz et al., 2010; Meusburger et al., 2010) which included PESERA, WEPP and USLE-type models. With the exception of the USLE-type models, the other models underestimated erosion rates by a factor of ten or even hundred compared to FRN based erosion rates. Visual judgement of soil degradation in the field (e.g. assessing erosion rates in mm soil loss each year) make FRN and USLE type model results very plausible. Another important point is that we do not intent to use the difference of 137Cs and RUSLE to directly derive the magnitude of the erosion process but to show that we are missing a process that is so far not considered in any soil erosion model. This systematic deviations that we interpret as winter erosion rates remain significant even if we use the upper uncertainty value for RUSLE and the lower for 137Cs. The absolute magnitude of snow glide erosion rate ($\neq$ winter erosion rate: snow glide + snowmelt) is derived from field measurements in snow glide deposits. The relation between the erosion rates from the snow glide deposits and the 137Cs/ RUSLE difference is not significant due to the reduced number of points but indicates that even the magnitude of the difference (137Cs and RUSLE) is plausible.

2) The reviewer questions the magnitude of 137Cs derived erosion rates. We agree that the application of 137Cs in the Alps is problematic mainly because of heterogeneous Chernobyl input and snow cover in some Alpine areas. However, we spent a lot of effort in validating the reference sites in the Urseren Valley with getting the CV down to 14% (Ramp, 2013). The sampling points as well as the reference points are of similar and limited spatial extend. For this reason some of the spatial heterogeneity

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can be avoided. Furthermore, reference sites at the Urseren Valley a more at the lower slopes which reduces the likelihood of the ground being snow covered in early May 1986. The remaining part of uncertainty is considered in the uncertainty analysis.

Origin of fallout: At our site the bomb fallout can be estimated at 2 kBq/m2 using lat/long and long-term averaged annual precipitation. Moreover, Dubois et al. (2001) estimates the Chernobyl fallout around 20 kBq/m2. Thus, this would even result in a 90% Chernobyl contribution. If we assume that the erosion rates at our highly degraded sites were already as high in the 60s during the bomb fallout, by 1986 even a smaller proportion of bomb fallout would contribute to the total inventory. Therefore, we do believe that this error is of minor importance compared to the error we already applied to our estimates. Our manuscript can still be reinforced in adding more reliable information on Chernobyl contribution (at least 90% of the 137Cs total inventory) that will strengthen the validity of the assumption that erosion rates relate to 1986.

Use of profile distribution model: Effectively, the profile distribution model (PDM) will tend to overestimate the erosion rates, due to the diffusion and migration of 137Cs with time. However, with all due respect to the reviewer, we cannot agree that the PDM model is not appropriate for sites dominated by 137Cs Chernobyl input. The model requires the year of the major fallout (i.e. 1963 in the case of Chernobyl non affected area [NB: it does not mean that all the fallout occurred in 1963 in reality the first international occurrence of 137Cs fallout is 1954...] ; or 1986 if the major fallout occurs with Chernobyl accidental fallout). We expect that it is even more appropriate for such sites, since the time since the fallout and the measurement is shorter and thus the extent of diffusion and migration is less.

According to Walling et al. (2011) the selection of conversion model in uncultivated area should be based on “the approximate assessment or no sign of migration in the reference profile”. This is the case in our investigated site. Therefore according to the guidance provided by Walling et al (2011), we selected the Profile Distribution Model for assessing soil erosion rate in this area. Moreover, our 137Cs soil depth distribution clearly follows an exponential function, which is the underlying assumption of the Profile Distribution Model. In contrast, we could not find a suitable fit of the diffusion and migration coefficient to our reference site data. Hence, we think that the profile distribution model is prior to the diffusion and migration model.

The changes of the depth distribution over time - that are not considered in the profile distribution model - are another point of uncertainty that was not discussed in the manuscript yet. It is difficult to precisely quantify this error since we would need besides the diffusion and migration rate (that we cannot observe from our reference profiles) the erosion rate of each single year since 1986. Our model works with the 50%-depth of the accumulated activity and is very insensitive to changes in the order of magnitude as described by Schimmack and Schultz et al. (2006). This is because we assumed a mixing of the upper 5cm due to bioturbation and trampling (Konz et al. 2009). This assumption is another source of uncertainty (especially because the mixing probably also did not occur immediately after the fallout). We have not discussed this specific point yet but this could be done in our revised version. However, the resulting uncertainty due to variation of the mixing depth was again minor compared to others.

3) Here the reviewer asked for "a more explicit attempt to quantify this erosion component (snowmelt) and to incorporate it into the final assessment of the relative magnitude of different erosion form." According to our definition the winter erosion rate also includes snow melt. The regression between the winter erosion rate (difference of 137Cs and RUSLE based erosion) and snow glide related sediment yield we observed an intercept. Our interpretation regarding this intercept is directed towards the contribution of snow melt: "The resulting intercept might be either to a deviation of the weather conditions in the winter 2012/13 from the long-term average condition captured by the other methods or due to the impact of occasional wet avalanches and/or snow melt. For instance, following the USLE snowmelt adaptation for R-factor would result in an on average 2.1 t ha-1 yr-1 higher modelled erosion rate for all sites." For sure, the reviewer will agree with us if we conclude at this point that a more in-depth

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quantification regarding the snowmelt contribution is not possible.

4) The revised manuscript will beneficiate from an additional language review performed by a native speaker to make sure that the potential remaining mistakes (i.e. typos and grammatical errors) are properly corrected.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 3675, 2014.