Reply Reviewer 1

First of all we would like to thank the Reviewer 1 for his very detailed discussion of the manuscript. Please be aware that this is only a short answer to address the major points raised. A detailed answer to each point addressed by the reviewer will follow in the final reply together with the revised version.

The reviewer raised some very important points that need consideration in a revised version of the manuscript. The major criticism which reviewer 1 addresses several times is that we compare the difference between to methods and attribute this difference to the impact of snow gliding while it very well could be just the error of the methods. And related to this point he criticises (i) the lack of presentation and discussion of uncertainties related to the methods applied and (ii) the lack of information regarding the methodologies used. In an attempt to avoid self-plagiarism the methodology part was too short. The latter raised several questions and doubts of the reviewer, which we believe could be sorted out quite easily by an extended method chapter. We fully agree that we cannot rule out that the difference between the Cs based erosion rates and the RUSLE estimates are solely based on errors of the two methods. However, we think it very interesting and worth publishing that this error is strongly related to snow gliding which can be explained by the concept of the two methods: the ^{137}Cs method includes winter processes, while the RUSLE model does not. Again we fully agree with reviewer 1 that we should give more attention to an error discussion of both methods. Regarding this important issue of uncertainty we propose to add a separate chapter, discussing errors and its potential effects on the magnitude of soil erosion rate. Moreover, we will introduce error bars in the graphs and follow the suggestions of reviewer 2.

We will thus try to give a short answer to the questions addressed by the reviewer (see 2 a-f; detailed information will be provided in the final reply and the revised manuscript):

a) For the reviewer it was unclear, whether the approach used assumes that the local ^{137}Cs areal activity density reflected both bomb fallout and Chernobyl fallout or only the latter. Unfortunately only little data on the pre-Chernobyl ^{137}Cs contribution is available for Switzerland. For example Riesen et al. (1999) measured samples collected in 1986 before the Chernobyl reactor accident from 12 forested sites distributed over Switzerland. The ^{137}Cs activities of the top soil layers (0-5 cm depth) were between 2 and 58 Bq kg^{-1}(Riesen et al., 1999). After decay, in 2007 only 1-35 Bq kg-1 would be left, which means that the maximum contribution of pre-Chernobyl ^{137}Cs might amount to 20% at reference sites. Consequently, our approach assumes that the main ^{137}Cs fallout at our site occurred after the Chernobyl accident in 1986, thus we assume the erosion rates integrating from 1986 up to 2007. The statement that the ^{137}Cs method provides an integrated estimate of the total net soil redistribution rate since the 1950s (page 9508 line 19 in the introduction) was meant in a general sense to introduce the method.

b) Yes, the most common approach is to compare ^{137}Cs inventories to assess soil erosion rates. However, for a previous study in addition to lab measured ^{137}Cs activities an-insitu detector was applied. The insitu detector has the advantage to average small scale variability that is caused e.g. by grazing and mouse activity etc. The output of the peak evaluation of the insitu detector is the activity, thus, Konz et al. (2009) presented directly the activities together with the bulk densities for relative comparison between the plots. In the conversion the bulk density and thus the ^{137}Cs inventory is considered.

c) We adapted the profile distribution model for its use with the in-situ detector. And as mentioned above we referred the erosion rates to 1986. Several assumptions are related to the profile distribution model e.g. that the ^{137}Cs depth distribution we find today is representative over time and that no new soil material is deposited/eroded on the reference plots. Due to diffusion and migration progresses of ^{137}Cs or through mixing of the upper soil layer by bioturbation, usually the ^{137}Cs depth profile does not strictly follow an exponential distribution. Thus, we think it is more valid to include this mixed layer in the model. Otherwise, this would result in an overestimation of soil erosion magnitude.

d) The reference sites are located in the vicinity of the sampling sites: the elevation of the reference sites ranges from 1469-1616m asl and the ones of the sample sites from 1476-1670m asl. Since the values of the reference sites are well within the range of the sample sites, we do not expect a high difference in ^{137}Cs fallout. We will include this information in the updated manuscript.

e) The mentioned paragraph refers to another study: *"The observation of increasing soil erosion with increasing snow glide rates is congruent with the findings of Leitinger et al. (2008), who observed that the severity of erosion attributed to snow gliding (e.g. torn out trees, extensive areas of bare soil due to snow abrasion, landslides in topsoil) was high in areas with high modelled snow glide distance and vice versa."* But also in our study site landslides are frequent and might potentially impact the application of the ^{137}Cs method. Thus, we chose sites free of landslides, which have been well documented in the valley by aerial photos in 1986, 1993, 2000, and 2004 (Meusburger and Alewell, 2008).

f) We could have excluded these two points instead we left them. The explanation provided for the different behaviour of these two points is, that the ^{137}Cs input to the soil surface was likely different below the alder. This is quite reasonable; however, to prove this, would require an additional set of reference sites with alder cover, which is difficult to find in the valley (9516 line 25). Moreover, we think it's valid to present data, which is still open to discuss, since outlier points are potential starting points for new research.

A third important point of the reviewer 1 is that the difference between RUSLE and ^{137}Cs may be caused by multiple processes not exclusively snow gliding but also snow melt, wind erosion etc.. We thought we made this point clear (page 9505 line 17). But it seems that it requires more discussion. Already the title might direct the reader too much into the direction of snow gliding and we will think of an adaptation of the title. Further, we propose to include more discussion to highlight the potential importance of snow melt. However, it seems not meaningful to include snowmelt according to the USLE procedure in the erosivity by "multiplying the precipitation falling in the form of snow by 1.5 and then adding the product to EI, the kinetic energy times maximum 30-min intensity" since "The redistribution of snow by drifting, sublimation, and reduced sediment concentrations in snowmelt confuses the problem tremendously" (Renard et al., 1997). Excluding the subsequent snow melt, snow fall itself does not trigger soil detachment, thus reducing the rainfall erosivity by excluding snow fall is more meaningful (Meusburger et al., 2012). Regarding wind erosion, we did some preliminary attempts to measure wind erosion in the valley but could never even find significant amounts of material transported (unpublished data).

Regarding the limited literature available investigating the interaction between soil surface and snow movement and the difficulties to investigate this process, we think the data is worth publishing if the different uncertainties are clearly presented and their respective impact on our results properly addressed.

References

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