Reply to the Editor

Dear authors,

your research received two clear reviews, however, with contrasting final decisions. I do think you point to an interesting process within soil erosion studies in (winter) snow covered areas. And I do appreciate the difficulty to quantify its contribution to the total soil erosion. Your method is novel, creative but also very sensitive to methodological errors and uncertainty. You present a diverse set of measurements and model results which seems to indicate the contribution of snow gliding in soil erosion. Also the uncertainty associated with this research is honestly mentioned and presented in your paper. Lastly, the methods can be repeated by other research groups and as such verified/falsified.

However, I agree with the first reviewer that the RUSLE-137-Cs approach needs more elaboration: the methodological assumptions strongly influence your outcome. Even to such degree that frankly said your conclusions could not appear that strongly or even disappear. Reviewer 1 challenges you to give some more 'support-proof' which I think are worthwhile to follow-up or discuss in the paper.
In that respect, I also suggest the authors to think of rephrasing the clear and quite strong conclusions such as: "Snow gliding is a key process" to less decisive wording in which the methodological uncertainty also shows (your title shows this with the wording: a first quantification attempt).

I look forward receiving a revised version of your paper.

Kind regards, Thom

Dear Editor,

We are pleased to see that our manuscript entitled " Soil erosion by snow gliding – a first quantification attempt in a sub-alpine area, Switzerland" was regarded as a novel and relevant study.

The Reviewer 2 suggests the publication of our work with only fairly minor revision. The Reviewer 2 understood that this is a pioneer research (i.e. a first attempt) and as such we agree as well with you that we paved the way for furtherer scientific tests and/or refinement by other research groups.

Reviewer 1 questions the methodological assumptions of the RUSLE and 137Cs method. We agree with him, both assessment methods have their own assumptions that cannot be disregarded. Since our first submission in 2013 (i.e. Hydrol. Earth Syst. Sci. Discuss., 10, 9505-9531, 2013) of our original paper entitled ‘‘Impact of snow gliding on soil redistribution for a sub-alpine area in Switzerland’’, we made huge efforts to detail all uncertainties involved and to inform and guide the readers (also by changing the title of the manuscript). Moreover, we even added new independent snow deposition sediment yield measurement to support our results. We were convinced that the uncertainties were already sufficiently highlighted. Nonetheless, we further improved the aspect of conceptual uncertainty in the current revised manuscript as requested by Reviewer 1.

However, we do not agree that the conceptual limitations and uncertainties related to RUSLE/137Cs will alter our conclusion. We have to emphasize that the RUSLE/137Cs approach is only one of two approaches. Reviewer 1’s critics are exclusively directed to the RULSE/137Cs subtraction approach not considering the direct snow deposition measurements we therefore improved the visibility of the snow glide deposition measurements throughout the manuscript. Further, we clarified which results lead us to our conclusion (conclusion that has been ‘‘smoothed’’ following your suggestion).

The co-authors and I are grateful to the reviewers for their valuable comments which have significantly improved our original submission.

With my best regards,

Katrin Meusburger

Reviewer 1 - first comment and reply

This manuscript attempts to quantify the relative importance of water erosion and erosion caused by snow gliding and other 'winter' processes in a sub-alpine area in Central Switzerland. Although it is important to understanding contemporary denudation processes in such environments, there have been few attempts to address this issue in previous work, because of the problems of quantifying the different erosion components. The title of this paper makes it clear that this is a first attempt, rather than a definitive assessment. The water erosion component is estimated using the RUSLE model, Cs-137 measurements are used to estimate the total soil redistribution caused by all processes and erosion rates associated with snow sliding are estimated from measurements of the sediment contained in snow deposits. Measurements of cumulative snow glide distance made during the winter of 2009-10 have also been used to develop the Spatial Snow Glide Model (SSGM), which is used to model snow glide distances within the study area to provide additional evidence of the likely importance of snow gliding in different locations. Although the estimates of snow glide erosion provided by the sampling of snow deposits provide a direct measure of the magnitude of this erosion component, the primary approach adopted by the authors to estimate this erosion component is to assume that the Cs-137 measurements quantify total erosion. Subtraction of the RUSLE estimate from the Cs-137 estimate therefore provides an estimate of the erosion associated with other processes. These processes are assumed to be dominated by snow gliding. These estimates show a reasonably good correlation with estimates of snow glide distance for the sampling points and this is seen as providing support for the 'subtraction' approach. However, the 'subtraction' approach relies heavily on the reliability of the erosion estimates provided by RUSLE and the Cs- 137 measurements and the authors explicitly acknowledge many of the uncertainties associated with these estimates. These are presumably one reason for emphasising that the paper describes a 'first attempt'. Having read the manuscript, I am unconvinced that it represents a significant contribution in its present form, even as 'a first attempt'. A number of problems associated with the study need to be recognised and addressed before it can be considered further.

We would like to thank the reviewer 1 for his in-depth comments and his interest in this study. With regards to the contents, we will not reply to the first comments again since the answers given in the short reply were exhaustive and because several issues are raised in the second comment again. Reference to lines and pages refer to the word version with track changes.

These are elaborated below.

1.1) The authors acknowledge the various uncertainties associated with the erosion estimates provided by the RUSLE and the Cs-137 measurements and they attempt to propagate these through the calculations to provide some indication of the uncertainty associated with the final estimates. However, I feel that the issue of uncertainty needs to be considered more broadly, in view of its importance for the 'subtraction' approach.

The authors approach to considering uncertainty focuses on the individual values used as input to the RUSLE and the model used to estimate erosion rates from the Cs-137 measurements. These are clearly important, but it is necessary to recognise that those models themselves involve much uncertainty in terms of their representation of the processes involved and the likely reliability of the output. If one was, for example, calculating the water discharge in a cross section from measurements of channel width, water depth and flow velocity, the key thing would be to consider the uncertainty associated with the input data, because the calculation procedure itself is well defined and involves little or no uncertainty. In the case of the MUSLE, however, there is considerable uncertainty surrounding the reliability of the final estimate, even if all the input data used are highly accurate and precise. The issue here is that the model is an empirical model developed from the USLE which was based on a large volume of erosion plot data. Few people would expect it to provide highly accurate estimates of erosion rates, although it can provide valuable information on likely relative differences between different locations and different cropping practices etc. If the values provided by MUSLE are to be used in the 'subtraction' some indication of the likely reliability of the model output (i.e. both accuracy and precision) must be provided. This is particularly important when the model is being used in a sub-alpine area which is very different from the areas for which the model was originally developed and on which the parameterisation is based. Is it correct to assume that model will not overestimate the erosion rates to the extent that the subtraction approach could be invalid? What is the possible degree of underestimation? The same issue is important for the model applied to the Cs-137 measurements to estimate erosion rates. Even if all the model input is highly accurate and precise, this does not mean that the results are reliable. This depends on the reliability of the model and the extent to which it correctly represents the various processes and controls involved. As indicated, below it seems likely that the model used in this study greatly overestimates the erosion rates, but this is not considered by the authors. If the estimates of erosion rate derived from the Cs-137 measurements are overestimates, this means that the magnitude of the snow glide erosion will also be overestimated. A good correlation with snow glide distance could still exist, even if the estimates of show glide erosion are overestimated or underestimated, and the correlation is not in itself an indication the results obtained from the 'subtraction' are reliable.

The authors do not place great emphasis on the absolute magnitude of the estimates of snow glide erosion derived from the 'subtraction' but, without further explicit consideration and discussion of the uncertainties involved, many readers will undoubtedly assume that they are seen as reliable and make false inferences regarding the relative magnitude of the erosion rates associated with the different processes.

Short reply 1.1) The reviewer addresses the uncertainties associated with the magnitude of winter erosion derived from the subtraction of ^{137}Cs 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 ^{137}Cs 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 ^{137}Cs. **The absolute magnitude of snow glide erosion rate (≠ 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 ^{137}Cs/ RUSLE difference is not significant due to the reduced number of points but indicates that even the magnitude of the difference (^{137}Cs and RUSLE) is plausible.

1.2) The estimates of erosion rates provided by the Cs-137 measurements are the central to much of the study and it is important to think carefully about their likely reliability. The authors consider the measurement precision and propagate this through the calculations, but they pay little or no attention to the likely reliability of the model used to convert the Cs-137 measurements to estimates of erosion rate. This involves two important problems. The first is the assumption that it is acceptable to assume that the Cs-137 fallout input to the study site was all deposited in 1986 as a result of the Chernobyl incident, even though the authors are not able to confirm this convincingly and some of the Cs-137 could be bomb-derived. The authors present some data for Cs- 137 activity in surface soils in Switzerland prior to Chernobyl, but, since these data are values of mass activity density, they cannot readily be used as a surrogate for values of areal activity density. The authors state that, as a maximum, 20% of the total Cs-137 inventory was associated with bomb fallout. This introduces considerable uncertainty into the estimates of erosion rate obtained from the Cs-137 measurements, which assume that all the fallout was Chernobyl-derived and occurred in 1986. If 20% of the inventory was deposited in the late 1950s and 1960s rather than in 1986, this will mean that the erosion rates presented will overestimate the true rates and there is a need to assess the likely magnitude of the overestimation. I feel that it is important to establish far more convincingly the likely proportion of the Cs-137 inventory contributed by bomb fallout. This would seem to be relatively easy. The authors have the necessary measurements to establish the contemporary reference inventory in their study area (Bq m-2) and there are various sources of information on the likely magnitude of the bomb-derived inventory for the study area based on studies of the spatial variability of bomb fallout at the global scale. Comparison of the two will provide a more convincing estimate of the likely contribution of bomb fallout to the Cs-137 inventories in the study and thus the uncertainty or error introduced by assuming that all fallout was deposited in 1986. There is, however, a much more serious problem with the procedure used to derive estimates of erosion rates from the Cs-137 measurements which means that the values obtained are not reliable. This relates to the use of a profile distribution model. Use of such a model assumes that the Cs-137 depth distribution documented at the time of sampling existed throughout the period extending from 1986 to the time of sampling.

This clearly cannot be the case. It is well known that the Chernobyl fallout occurred during a very short period after the accident and when this fallout reached the soil surface it would have been contained within a very shallow surface layer (e.g. the upper 5-10 mm) of the soil. Subsequently, the depth distribution would have changed due to downward diffusion and migration. Based on measurements undertaken in Bavaria, Schimmack and Schultz (2006) report that measurements undertaken in June 1986, several weeks after the Chernobyl incident, showed that 82.5% of the Cs-134 (solely Chernobyl derived) was contained in the upper 0-2 cm layer of the soil, whereas by 2001 the equivalent value was 14%. The value is likely to be significantly less by 2009 when the measurements used in this study were undertaken. Failure to take this into account, by using the profile distribution model with the depth distribution documented for the time of sampling, means that the erosion rates will be greatly overestimated. The same problem exists in applying this model to Cs-137 measurements in areas where the radionuclide was supplied solely by bomb fallout, but it is not as severe, since the fallout was delivered over a period of more than 10 years and not within a period of a few days. It is important to recognise that with the erosion rates of up to 30 t ha-1 (i.e. ca. 3 mm year-1) suggested for the study area the erosion could have removed a substantial proportion of the fallout within the first year, whereas the model assumes that even in the first year the fallout is distributed to a considerable depth. For many subsequent years the Cs-137 would still be distributed much higher in the profile than indicated by the profile documented by recent sampling and the amount of soil removed to produce the measured reduction in inventory relative to the reference inventory will be overestimated. I would guess that the estimates of erosion rates derived from the Cs-137 measurements and presented in the paper probably overestimate the true rates by ca. 3 times. However, this needs verification by model tests. The authors do in fact refer to the need to take account of vertical migration and cite the work of Schimmack and Schultz (2006), but do not appear to recognise its important impact on the reliability of their erosion rate estimates. Although no details of the procedure used to estimate erosion rates from Cs-137 measurements are provided, the papers by Konz et al. cited in the ms. make it clear that a profile distribution model was used. I am unsure how best to address this problem. It is clear that the profile distribution model is inappropriate for a situation where the dominant source of Cs-137 is Chernobyl fallout. A model that incorporates post-fallout diffusion and migration such as the Diffusion and Migration Model should be used. I feel that the Cs-137 measurements should be reprocessed using such a model, in order to derive more reliable estimates of erosion rates. However, since some of the erosion rates used in the study were reported in previous publications, this approach might be seen as problematical. The alternative would be to explicitly recognise the inherent unreliability of the erosion rate estimates, to establish the likely degree of overestimation through model tests and to build this information into the interpretation of the results. However, to use results which are known to be unreliable would seem be scientifically unsound.

Short reply 1.2) The reviewer questions the magnitude of ^{137}Cs derived erosion rates. We agree that the application of ^{137}Cs 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 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/m^2 using lat/long and long-term averaged annual precipitation. Moreover, Dubois et al. (2001) estimates the Chernobyl fallout around 20 kBq/m^2. 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 ^{137}Cs 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 ^{137}Cs with time. However, with all due respect to the reviewer, we cannot agree that the PDM model is not appropriate for sites dominated by ^{137}Cs 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 ^{137}Cs 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 ^{137}Cs 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.

Reply1.3) The hypothesis employed in the 'subtraction' approach would appear to be that subtraction of the RUSLE estimate of erosion from the Cs-137-derived erosion estimate provides an estimate of 'winter' erosion which in turn represents erosion attributable to snow sliding. It would seem that 'winter' erosion also includes erosion caused by snowmelt and I feel that there needs to be a more explicit attempt to quantify this erosion component and to incorporate it into the final assessment of the relative magnitude of different erosion form.

Short reply 1.3) 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 ^{137}Cs 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 quantification regarding the snowmelt contribution is not possible.

1.4) Although the manuscript is well structured and well written there are several places where the grammar, syntax and vocabulary need minor attention and I would recommend that the authors seek the help of a native English speaker to polish the English. On page 3650 line 10, I assume that it should be 'transverse' rather than 'traverse'?

Reply 1.4: The revised manuscript beneficiated from an additional language review performed by a native speaker. The following changes were done: we replaced deposit by deposition and high difference in large difference etc. Moreover, the changes requested by the reviewer were introduced (see line 146).

Overall, I find myself unconvinced that this ms provides a meaningful and significant contribution regarding the relative importance of water erosion and erosion due to snow sliding, even as a first attempt. I am not convinced that RUSLE can be expected to provide an accurate assessment of water erosion in this environment and the estimates of erosion derived from the Cs-137 measurements are likely to be gross overestimates. As a result the estimates of erosion rates attributable to snow sliding derived by subtraction of the MUSLE estimate from the Cs-137 estimate are cannot be seen as reliable and are also likely to involve errors, due to the uncertainty regarding the relative contribution of erosion caused by snowmelt to 'winter' erosion. The good relationship between the estimates of snow sliding erosion derived using the 'subtraction' approach and measurements of snow glide distance (r2 = 0.64) is encouraging, but does not in itself confirm that the magnitude of the estimates of snow sliding erosion is correct.

A good relationship could still be obtained if all the values are gross overestimates and there is a consistent degree of overestimation across the data. The relationship between the estimates of snow glide erosion derived by 'subtraction' and the values obtained by sampling snow deposits is characterised by a lower coefficient of determination (0.39) with limited statistical significance and no clear conclusion is possible.

Reviewer 1 - second comment and reply

2.1) The authors have provided a brief response to my review comments in which they recognise the need to take more account of uncertainties in the estimates of soil loss provided by the RUSLE and by the Cs-137 measurements, which are subtracted to provide an estimate of 'winter erosion'. The study reported is founded on the assumption that both methods provide both accurate and precise estimates of soil loss and that subtraction of the two estimates provides a meaningful estimate of winter erosion. I remain unconvinced that this assumption is realistic. Both methods are associated with very considerable uncertainty that must be more explicitly recognised.

Reply 2.1) Since Reviewer 1 mentioned again that the uncertainties of ^{137}Cs and RUSLE need more conceptual discussion; we added further discussion on it in line 497-506. Reviewer 1 places too much emphasis on the "subtraction" approach. She/he might have missed our results of the direct measurements in the snow glide deposit. The latter results which are not subject to any conceptual uncertainties, highlight that erosion by the process of snow gliding can reach up to 23 t/ha yr. We think this value (even if not set in relation to water erosion rates modelled with RUSLE) clearly allows the conclusion that snow induced erosion is a crucial process of soil redistribution. To visually illustrate and to help understanding the process, we added a photograph (Figure 1 and 2 here and Figure 4 and line 401 in the manuscript). On the site where the photograph was taken it is obvious that several cm of soil were redistributed which translates to 70-350 t/h and winter (assuming a bulk density of 0.7 g/cm^{-3} and a spatial dimension of one hectare). We gave the snow deposit based results in the manuscript more emphasis and hope that it is thus clearer that the subtraction approach underpin these results. Changes can be found in the abstract (line 20-36), introduction (line 114), in chapter 3.2 the order of results was changed and the conclusions (line 548-557). The snow glide deposit measurements are subject to high spatial and temporal variability. Thus the combination of the two methods is of merit: the subtraction approach extrapolates over time and space.

2.2) However, once this is recognised, I fear that the subtraction approach cannot be expected to produce meaningful results. The authors suggest that RUSLE provides 'better' estimates of soil loss than other models such as PESERA. However, the estimates provided are still only gross approximations and I would not expect them to provide an accurate estimate of the soil loss. Although the errors are probably less than the order of magnitude errors associated with other models they are still likely to be very large. If the authors are going to provide a convincing argument that estimates of soil loss provided by the RUSLE and by Cs-137 measurements can be directly compared and therefore used in a 'subtraction' they need to point to results from a study in an area with no winter erosion where RUSLE estimates and Cs-137 measurements provide results that are essentially identical. I am not aware of any study that has done this, although I may be wrong. As indicated in my earlier comments, I feel that the values of soil loss generated from the Cs-137 measurements are almost certainly substantial overestimates of the true rate of soil loss. If this is the case, the fact that the Cs-137 measurements provide estimates of soil loss that exceed those provided by the RUSLE, which in turn means that subtraction of the two apparently provides an estimate of 'winter' erosion, is likely to be totally spurious.

Here the reviewer raises the very good idea that the subtraction approach can be validated in a site with no snow induced erosion. Basically we show such data in our study. On the north facing slopes were usually snow glide rates are smaller (In der Gand and Zupancic, 1966; Newesely et al., 2000; Hoeller et al., 2009) we also find a smaller difference between RUSLE and ^{137}Cs. In the existing literature – as highlighted by Mabit et al. (2013) and Benmansour et al. (2013) – there is as well several other evidences of similar result obtained using both approach, especially when the same time scale was considered.

Moreover, we can provide the opposite – an avalanche site, were almost all erosion is snow induced (Ceaglio et al., 2012). Here we find a good agreement between ^{137}Cs based erosion rates (calculated with the profile distribution model) and the avalanche deposit sediment measurements. The study was mentioned before and is now mentioned in line 410-416. In a very recent study on this avalanche deposit site we could show that the difference in ^{137}Cs and RUSLE is systematically related to snow induced erosion (Stanchi et al., 2014; line 493-495).

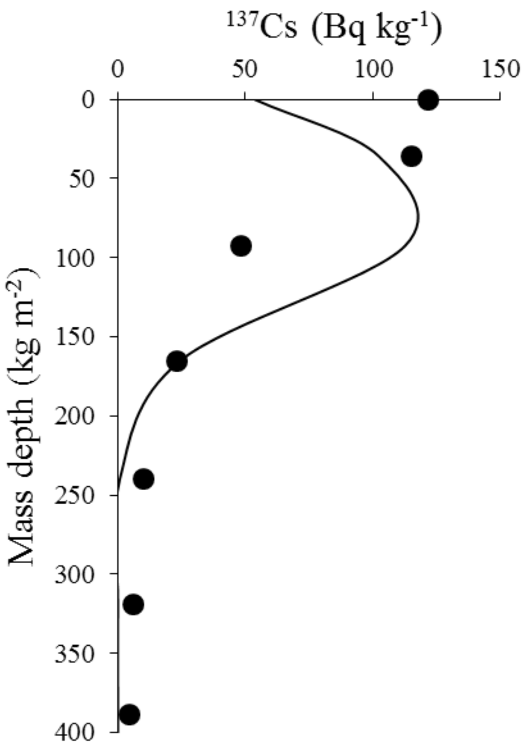
2.3) The authors use a variant of the profile distribution model (PDM) to estimate soil loss from the Cs-137 measurements. This assumes that the depth distribution of Cs-137 measured at present can be assumed to be representative of the entire period between the time of fallout (1986 in the case of Chernobyl fallout) and the time of sampling. This is very unlikely to be the case unless the depth distribution shows that downward migration was very limited. Walling et al. (2011 –IAEA TECDOC)) clearly indicate that a PDM model is likely to overestimate the soil loss. They recommend use of a diffusion and migration model as providing more reliable estimates of soil loss. The basis of this problem is clearly demonstrated by the fact that available information on the depth distribution of both Chernobyl and Fukushima fallout measured shortly after the fallout receipt (see Schimmack and Schultz, 2006 and Kato et al, 2012 – JENVRAD 111, 59-64) indicates that the fallout was initially largely contained in the upper 1-2 cm of the soil, whereas the depth distribution used as the basis of the PDM employed by the authors appears to indicate that only 50% of the activity was contained in the upper 5cm. Within this upper 1-2 cm the activity is likely to have evidenced an exponential decrease from the surface down. In the years immediately after Chernobyl, erosion of a given depth of soil would have removed a much greater proportion of the Cs-137 than would be calculated using the depth distribution existing at the end of the study period. If erosion rates were, as reported, as high as 3 mm per year most of the Chernobyl Cs-137 could have been removed within the space of 3-4 years before downward migration and mixing of the surface layer had a significant effect. As indicated previously detailed calculations would be necessary to estimate the degree of overestimation associated with the current estimates, but I would guess that it is around x3.

Despite the authors' strong disagreement, I would reiterate that a PDM is particularly problematical for a situation with Chernobyl fallout for two reasons. Firstly, all the fallout occurred within a very short period of time (i.e. ca. 1 week). This means that ALL the fallout would have been concentrated at the surface. In the case of bomb fallout, the fallout occurred over a much longer period i.e. 1956-1975 and downward migration would have soon moved the first fallout input downwards.

Secondly, much will depend on the rate of downward migration and its change (reduction) through time. However, in most cases the Cs-137 depth distribution can be expected to change relatively little after about say 20 years. As a result the failure to take account of the change of the depth distribution through time will have MUCH LESS EFFECT for a site dominated by bomb fallout than one dominated by Chernobyl fallout. In the latter case, the period of rapid change in the depth distribution will occupy a large proportion of the total period since fallout receipt. The measured depth distribution obtained in an area where bomb fallout is dominant will be much more representative of the overall period covered by the model. The authors appear to suggest that a diffusion and migration model cannot be applied to the study area. It is difficult to understand why this should be the case. As I understand it, in the model described by Walling et al. (2011 –IAEA TECDOC) the diffusion and migration rates are empirically derived and can be estimated from the measured depth distribution and the mixing described in the top 5 cm would be reflected by the values obtained. It is not necessary to model the erosion rates for each year. The model assumes a constant erosion rate for each years and this is represented by the final estimate of mean annual soil loss.

The problem with the downward migration may lead to an overestimation as pointed out by Walling et al. (2011). We added this to the discussion (line 482-486). Moreover, we explained why we selected the PDM (line 303-305). The reasons were already mentioned in our short reply: "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 ^{137}Cs 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." The latter is illustrated below.

^{137}Cs depth distribution and the best fit we found using the diffusion and migration model:



Obviosly the use of the diffusion and migration model to the above depth distriburion will result in wrong and biaised erosion estimates.

2.4) The authors do not really discuss the need to consider the effects of grain size selectivity when using Cs-137 measurements to estimate soil loss. However, they can have an important effect of the final estimates and must be recognised. Failure to take them into account could again result in overestimation of the soil loss. The degree of overestimation could therefore be even greater than suggested. Contrasts in the grain size selectivity associated with rainfall-driven and snow slide erosion add further complexity to using Cs-137 measurements to estimate soil loss.

We agree that the particle size factor is important if water erosion triggers a preferential loss of small grain size fractions. In fact, a particle size correction factor is incorporated into the conversion model, in order to take account of the potential grain size selectivity of erosion and sedimentation processes and the likely preferential association of ^{137}Cs with the finer fractions of the soil or sediment. Thus, for example, if erosion is associated with the preferential removal of fine particles, which are characterized by high concentrations of the radionuclide (i.e. ^{137}Cs), the erosion rate is likely to be overestimated if this is not taken into account.

However, the particle size factor will only improve erosion rate estimation if erosion by water transport is the major process. We actually found no preferential transport or preferential transport of the coarser grains during soil erosion at our sites (see Figure below and Konz et al., 2012). The latter might again point to the process of snow and animal induced erosion at our sites. Further investigation would be needed to identify whether for animal and snow induced soil erosion preferential transport occurs and which size fractions are involved. The results of Konz et al. (2012)

indicate a preferential transport of the coarse fraction. In not considering the preferential transport of the coarse fraction, we rather underestimate ^{137}Cs based erosion rates.

We added a paragraph on the particle size factor in the revised version (line 299-301).

References

Benmansour, M., Mabit, L., Nouira, A., Moussadek, R., Bouksirate, H., Duchemin, M., and Benkdad, A.: Assessment of soil erosion and deposition rates in a Moroccan agricultural field using fallout 137Cs and 210Pbex, Journal of Environmental Radioactivity, 115, 97-106, 10.1016/j.jenvrad.2012.07.013, 2013.

Bostick, B. C., Vairavamurthy, M. A., Karthikeyan, K. G., and Chorover, J.: Cesium adsorption on clay minerals: an EXAFS spectroscopic investigation, Environ. Sci. Technol., 36, 2670-2676, 2002.

Ceaglio, E., Meusburger, K., Freppaz, M., Zanini, E., and Alewell, C.: Estimation of soil redistribution rates due to snow cover related processes in a mountainous area (Valle d'Aosta, NW Italy), Hydrology and Earth System Sciences, 16, 517–528, 2012.

Konz, N., Baenninger, D., Konz, M., Nearing, M., and Alewell, C.: Process identification of soil erosion in steep mountain regions, Hydrology and Earth System Sciences, 14, 675-686, 2010.

Konz, N., Prasuhn, V., and Alewell, C.: On the measurement of alpine soil erosion, CATENA, 91, 63-71, 10.1016/j.catena.2011.09.010, 2012.

Mabit, L., Meusburger, K., Fulajtar, E., and Alewell, C.: The usefulness of 137Cs as a tracer for soil erosion assessment: A critical reply to Parsons and Foster (2011), Earth-Science Reviews, 137, 300-307, http://dx.doi.org/10.1016/j.earscirev.2013.05.008, 2013.

Meusburger, K., Konz, N., Schaub, M., and Alewell, C.: Soil erosion modelled with USLE and PESERA using QuickBird derived vegetation parameters in an alpine catchment, International Journal of Applied Earth Observation and Geoinformation, 12, 208-215, 10.1016/j.jag.2010.02.004, 2010.

Ramp, A.: Resampling von 137Cäsium zur Validierung der Referenzstandorte im Urserental, Bachelor, Environmental Sciences, University of Basel, Basel, 42 pp., 2013.

Stanchi, S., Freppaz, M., Ceaglio, E., Maggioni, M., Meusburger, K., Alewell, C., and Zanini, E.: Soil erosion in an avalanche release site (Valle d'Aosta: Italy): towards a winter factor for RUSLE in the Alps, NHESSD, 2, 1405-1431, doi:10.5194/nhessd-2-1405-2014, 2014, accepted.

Walling, D.E., Zhang, Y., He, Q., 2011. Models for deriving estimates of erosion and deposition rates from fallout radionuclide (caesium-137, excess lead-210, and beryllium-7) measurements and the development of user friendly software for model implementation. Impact of Soil Conservation Measures on Erosion Control and Soil Quality. IAEA-TECDOC-1665, pp. 11–33.

Reviewer 2 – comment and reply

In general, empiricism is not an evil, but a means. Especially because the authors explicitly acknowledge many of the uncertainties associated with RUSLE and FRN and claim attention on the weaknesses and uncertainties of In general, empiricism is not an evil, but a means. Especially because the authors explicitly acknowledge many of the uncertainties associated with RUSLE and FRN and claim attention on the weaknesses and uncertainties of the methodology, this paper is in my opinion as a first attempt an interesting and significant contribution. RUSLE estimates underwent a simple but replicable uncertainty assessment; errors associated with the spatial variability of FRN are taken into account. Probably estimated errors may be undervalued or are somewhat arbitrary (e.g. assumed error of static friction coefficient _0.1), but weaknesses and uncertainties of the methodology are pointed out and illustrated. Therefor I agree to the authors' results "Even though all presented data are subject to high natural variability and methodological uncertainty the results imply that (i) the observed discrepancies between the RUSLE and 137Cs based soil erosion rates are indeed related to snow gliding and (ii) snow gliding is an important agent of soil redistribution". Of course, a larger quantity of sediment sampling would be desirable, but as a first quantification attempt, the data indicate the applied hypothesis.

Many thanks to reviewer 2 for his/her support and for identifying some inconsistencies in the manuscript. We added some more explanation why we chose different error ranges (line 244, 248 and 802.

In my opinion some minor revisions should be done:

1. Chapter 2.1: Agrostis capillaris instead of Agrostis capillaries

   Was corrected (line 141, 144 page 6).

2. Chapter 2.3.2: (US Department of Agriculture, 1977) is missing in the references

   Thanks, the reference was added.

3. Chapter 3.3: The difference of 137 Cs and RUSLE ranges from minus 3.3 to 31. Minus values should be explained (no erosion)

   A sentence was added for explanation (line 447-449).

4. Chapter 3.3 p3691 line 13: a dot is missing : : :Alnus viridis stocking.

   Was corrected (line 473).

5. Table 1: sites p should be labelled p1 and p2

   Was changed.

6. Table 2: 137Cs values for pw1 and pw2 are probably interchanged (compared to table 3)

   The ^{137}Cs values in Table 3 were corrected.

7. Fig 4: resulting from several sediment measurements instead of resulting from sediment several measurements

Was corrected (now Fig. 5).

8. Fig 5: It is not clear if the data are plotted accurately. Within the data, there is no value found for 137 Cs-RUSLE at -7 and snow glide distance of A2N should be 28cm according to table 1. Regression lines should be described.

   This was due to the interchanged value in Table 3 (see comment no. 5) Now the -7.3 data point does appear also in Table 3.

   Regarding the graph, we did not plot A2N in Figure 5 (now Fig. 6) since we did not observe any snow glide deposit for this site in 2012/2013. Regression lines were explained (line 811 and 819).

9. Fig 8: It seems that winter precipitation of 2009/10 was used instead of using long-term average winter precipitation as mentioned. Using formula 8 the hayfields should reach distances longer than 300cm applying 430mm?

   We used the long-term winter precipitation for this graph (Fig. 8, now Fig. 9) and the map (Fig.7, now Fig. 8). Values illustrated in Fig. 8 (now 9) do not refer to our experimental sites but to mean values (± SE) for the study site (Fig.7, now Fig. 8). Consequently, values in Fig.8 (now 9) are incomparable to values in Fig.6 (now 7) as minima and maxima are not shown.