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Discussions

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

Anonymous Referee #1

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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. These are elaborated below.

(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 correla-

tion 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.

(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

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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.

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.

4)Although the manuscript is well structured and well written there are several places

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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’?

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.

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