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Discussions

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

**Anonymous Referee #1**

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This manuscript attempts to quantify soil loss associated with snow gliding in an alpine environment by comparing estimates of mean annual soil loss obtained using the USLE/RUSLE and Cs-137 measurements. The assumption is that the estimate of soil loss provided by the USLE/RUSLE reflects erosion associated with rainfall during the warmer months, whereas that provided by the Cs-137 measurements reflects the total erosion or soil loss during the year. It is further assumed that subtraction of the former from the latter provides an estimate of the erosion caused by snow gliding. Although the authors recognise that this approach will be sensitive to errors in the two different estimates, this problem receives little detailed attention. The results presented are seen by the authors as providing a meaningful estimate of the magnitude and relative importance of soil loss occurring during the warmer months as a result of storm rainfall



and the erosion occurring during the winter months caused by snow gliding. Within the study area, snow gliding is estimated to account on average for 67% of the total annual soil loss.

The conclusions regarding the likely importance of snow gliding are heavily dependent on the difference between the estimates of soil loss provided by the USLE/RUSLE and the Cs-137 measurements and it is important that the uncertainty associated with these two approaches should be carefully explored before basing conclusions on their relative magnitudes. It could be suggested that the USLE/RUSLE is able to provide an indication of the likely magnitude of rainfall-induced soil loss, and its variation in response to different controlling factors, but that the magnitude of the estimates obtained should not be seen as highly precise or reliable. This is particularly the case when the approach is extended to environments, such as alpine environments, that are very different from the conditions for which the models were originally developed. Equally, the use of Cs-137 measurements involves many uncertainties and these again need to be recognised. The Cs-137 approach is arguably better at demonstrating the spatial variability of rates of soil loss in response to various controlling factors in relative terms rather than providing highly precise estimates of these rates. Furthermore, the assumption that the difference between the two estimates represents erosion caused by snow gliding assumes that no other processes are operating to account for the difference and this again requires careful evaluation.

Further consideration of the approach employed by the authors suggests that the two sets of estimates involve very considerable uncertainty and that as a result the conclusions presented regarding the relative importance of erosion caused by snow gliding are unlikely to be reliable. The existence of a relationship between the estimates of soil loss induced by snow gliding and measurements of the snow gliding intensity are seen as supporting the conclusions presented. However, although this relationship could indicate that the two variables are related and show similar spatial trends, it does not provide confirmation that the magnitude of the estimates of soil loss caused by snow gliding are meaningful and that the conclusions presented regarding the relative contribution of snow gliding to total soil loss in the study area are correct.

In considering the various uncertainties associated with the two estimates of soil loss employed in the study, the reader will find that the manuscript provides very little information concerning the methods employed to obtain those estimates. The study reported essentially uses data obtained by previous studies undertaken by the authors using the USLE/RUSLE and Cs-137 measurements without providing details as to the procedures and methods employed. As a result it is necessary to refer to a number of other papers to obtain this information and the reader is likely to find this to be a time consuming and possibly frustrating process. Although the desire to limit the length of the paper must be applauded, it could be suggested that the need to consult other published papers for such basic information on the methods used is unsatisfactory.

To highlight some of the uncertainties that could be seen as prejudicing the direct comparison of the two estimates of soil loss and thus the conclusions presented in the manuscript, the following examples are provided.

(1)It is unclear why the manuscript refers to the use of the RUSLE, whereas publications describing the studies that provided the data used in the current study refer to the USLE. However, this is not a significant issue. More important is the fact that the model as applied does not incorporate the erosion caused by spring snowmelt into the estimate of annual soil loss. The original USLE included a factor to account for this which effectively increased the magnitude of the factor R. In this study the method used to estimate R reduces its magnitude in proportion to the relative contribution of snow to the total annual precipitation. As a result it would seem that the estimate of annual soil loss provided by the USLE/RUSLE will underestimate the total soil loss caused by rainfall and runoff. This in turn means that the difference between the two estimates of soil loss will not reflect simply the erosion caused by snow gliding, but also the erosion associated with the spring snowmelt, which could be considerable and possibly of a similar magnitude to the estimate provided by the USLE/RUSLE. As indicated above,



use of the USLE/RUSLE in what could be viewed as ‘extreme’ environments relative to those for which the models were developed also increases the uncertainty associated with the final estimates of soil loss obtained. For example, use of the standard procedure for estimating K may not be meaningful for soils that are regularly subjected to freeze thaw and disturbed by solifluction and which may at times be frozen at depth and saturated by snowmelt. In short, any estimate of ‘summer’ erosion provided by the RUSLE/USLE must inevitably involve a high degree of uncertainty and cannot be viewed as a precise estimate and there is a need to consider whether erosion associated with snowmelt is included in the estimate or explicitly excluded. In the former case the estimate of soil loss will underestimate the total erosion associated with rainfall and runoff. In the latter case there is a need to recognise that the difference between the estimates of soil loss provided by the USLE/RUSLE and Cs-137 measurements will reflect the erosion associated with spring snowmelt, as well as that caused by snow gliding.

(2)Perusal of the procedure used to derive estimates of soil loss from Cs-137 measurements again identifies many potential uncertainties which raise questions regarding the likely precision of the final estimates of soil loss. These include the following issues.

(a)It is unclear whether the approach used assumed that the local Cs-137 areal activity density reflected both bomb fallout and Chernobyl fallout or only the latter. On page 9508 line 19 the authors refer to Cs-137 measurements providing an estimate of average soil loss over a period extending back to the 1950s and thus a period of ca. 50 years. However, the paper by Konz et al. (2009) reporting work in the study area indicates that it was assumed that bomb fallout was unimportant and that only Chernobyl fallout was considered. This means that the estimated soil loss was only attributed to a period of 22 years rather than ca. 50 years. No information regarding the relative importance of bomb and Chernobyl fallout in the study area and thus the reason for discounting bomb fallout is provided by Konz et al.(2009). Published data suggest that in the study area the two could be of similar magnitude. If this is the case, the estimates of soil loss derived from the Cs-137 measurements and used in the study could significantly overestimate the mean annual soil loss. Overestimation will still occur even when bomb fallout represents a smaller proportion of the total inventory.

(b)In most studies that use Cs-137 measurements to estimate rates of soil loss emphasis is placed on reduction of the areal activity density or inventory, rather than the mass activity density or concentration, as used in this study. No explanation for this different approach is provided, but it makes it difficult to assess the magnitude of the inventories at the sampling sites and, for example, to assess the likely magnitude of the contributions of bomb and Chernobyl fallout. Readers of the manuscript are referred to a publication by Schaub et al. (2010) as providing information on the calibration of the in-situ detector used to obtain the Cs-137 measurements. Unfortunately this is not listed in the References. A literature search suggests that this is a paper published in Journal of Environmental Radioactivity. This paper highlights the many uncertainties associated with in situ measurements and the need to make assumptions as to the form of the Cs-137 depth distribution and to take account of the effects of soil moisture content on the measurements obtained. Although the overall magnitude of the uncertainty associated with the Cs-137 measurements used in the study is not likely to be great it could be of the order of 20% and this uncertainty should be explicitly attributed to both the reference activity and other measured activities and propagated through the procedure used to estimate rates of soil loss.

(c)Beyond this, it is also important to consider the likely reliability of the estimates of soil loss provided by the Cs-137 measurements. No direct validation of these estimates is available and although other publications by the authors point to the similarity of the estimates to those provided by the USLE as providing some degree of validation, such comparisons must be treated cautiously, particularly when the two estimates are subsequently assumed to be highly precise and are directly compared. Further consideration of the assumptions underlying the procedure used in this study to derive estimates of erosion rates. Again, it is necessary to look at other publications to find



details of the procedure used. Konz et al. (2009) provide such details. It would seem that a variant of the depth distribution model used by other workers is used here. This assumes an exponential depth distribution of Cs-137 and estimates the soil loss as a function of the reduction in inventory compared to the reference inventory and the time elapsed since the fallout input. This raises two important issues. The first is whether all the fallout input can be attributed solely to Chernobyl fallout or whether some can be attributed to bomb fallout. In the former case the period elapsed since fallout is indicated to be 22 years. However in the latter case it will be closer to 50 years. If some of the fallout is attributable to bomb fallout the approach used by the authors will overestimate the true erosion rate. Turning to the second issue, a key assumption of the model used by the authors to derive estimates of erosion is that the depth distribution documented at the time of sampling has been the same over the past 20 years since the original fallout. This is clearly unlikely to be the case since the depth distribution will have evolved through time, gradually extending in depth and with surface activity reducing through time due to downward ‘diffusion’. As a result the estimates of soil loss rate obtained will overestimate the true rate. Equally the description of the procedure used to estimate soil loss rates provided by Konz et al. (2009) suggests that the depth distribution is characterized by a zone of surface mixing with near constant activity. It is not clear how this is reconciled with the assumption of an exponential depth distribution used to estimate the erosion rate. If it is not taken into account it is again likely to lead to overestimation of erosion rates.

(d)No explicit information is provided as to the relative altitude of the reference site and the other Cs-137 sampling sites. However it is well know that precipitation and associated Cs-137 fallout are likely to vary with altitude and the possibility that the reference inventory employed underestimates the fallout input to other sampling sites at a higher altitude must be considered. If it does, the estimates of erosion rate will again be overestimated.

(e)There is also a need to recognise that the use of Cs-137 measurements to estimate erosion rates is applicable primarily to sheet and rill erosion and assumes that the surface is progressively lowered by removal of a thin surface layer by erosion. This raises the question as to whether the occurrence of other processes such as solifluction and the landsliding of surface soil referred to in the manuscript (Page 9516 line 10) will mean that the Cs-137 approach is to some degree compromised and that the results obtained are associated with further uncertainties.

(f)The apparently inconsistent results provided by the Cs-137 measurements undertaken beneath the alder cover are conveniently excluded from the analysis of results without a convincing explanation of the cause of the inconsistency. The inconsistency of these results must inevitably introduce doubt regarding the reliability of the other results.

Overall, the manuscript is well structured and well produced, although it would benefit from editing by a native English speaker. For example, the word 'exposition' is frequently used to denote what I assume should be 'exposure'. I found the description of the sampling site provided on page 9515 line 21 potentially confusing as it refers to sites downslope of the Alnus stands whereas the previous description of the sampling sites suggests that they are within the Alnus stands.

It can be concluded that the very considerable uncertainty associated with the estimates of soil loss derived using both the RUSLE/USLE and Cs-137 means that neither can be viewed as precise and therefore that any attempt to derive estimates of the magnitude of the soil loss associated with snow gliding by subtracting one estimate from the other is unlikely to yield reliable results. Some of the issues outlined above suggest that the RULSE/ULSE is likely to underestimate erosion cased by rainfall and runoff by failing to take account of, or excluding, erosion occurring during snowmelt and that the procedure used in processing the Cs-137 measurements is likely to overestimate rates of soil loss. This would lead to overestimation of the importance of erosion caused by snow gliding. However, other sources of uncertainty are likely to introduce further errors into the results obtained. Against this background the results presented must be



seen as involving too many uncertainties to be seen as reliable and meaningful. I am unable to recommend this manuscript for publication.

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