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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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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. 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. 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 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

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