Hydrol. Earth Syst. Sci. Discuss., 8, C5292–C5304, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C5292/2011/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Estimation of soil redistribution rates due to snow cover related processes in a mountainous area (Valle d'Aosta, NW Italy)" *by* E. Ceaglio et al.

E. Ceaglio et al.

elisabetta.ceaglio@unito.it

Received and published: 15 December 2011

First of all we want to thank all the referees, Dr. Bernhard Kohl, Dr. Peter Molnar and Dr. Tobias Heckmann, for their very useful comments about this work and for their fair and very constructive contributions and suggestions that will definitely help to improve the paper. Here below, the answers (AC) to each comment of the referees (RC):

Answer to Kohl Specific Comments

RC: The relative importance in comparison to runoff processes should be developed more clearly both in the conclusions and in the abstract. page8537 line6: The Hypoth-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

esis was to compare the relative importance of soil erosion related to snow movements with that of runoff processes, but runoff processes have not been discussed anyway. AC: We fully agree with this comment, and we add some more information about the magnitude of runoff processes in mountain areas. In particular we aimed at: 1) extending the concept of runoff processes, and above all the comparison with the snow movement processes, 2) replacing the term “winter processes” with “snow related processes” (or similar), because “winter” could be also related to runoff processes, not only to snow cover processes. Integrations are reported as following:

Abstract: Page8534 line2-5: “Mountain areas are widely affected by soil erosion, which is generally linked to runoff processes occurring either in the growing season or during the snowmelt period. Also processes like snow gliding and full-depth snow avalanches may be important factors that can enhance soil erosion, however the role and importance of snow movements as agents of soil redistribution are not well understood yet.” Page8534 line20-25: “Even though the comparability is limited by the different time scale of the applied methods, both techniques yielded similar magnitudes of soil redistribution rates. This outcome indicates that soil erosion due to snow movements can be considered the main driving force of soil redistribution in this area, with a greater impact in comparison to the runoff processes. Therefore, for the assessment of soil vulnerability in mountain areas, erosion processes during the snowy season have to be considered.”

Introduction Page8535 line6-8: “Generally, soil erosion is linked to rainfall runoff processes. However, in mountain areas, snowmelt runoff and snow movement related processes, like snow gliding and full-depth avalanches, may be important factors that can enhance soil erosion (Konz et al., 2009). In particular, the snowmelt can contribute significantly to the total annual runoff and sediment yield, constituted mainly by fine particles (< 2 mm) (Lana-Renault et al., 2011).”

Page8535 line26: “. . .soil erosion caused by snow movement related processes. . .”

Page8536 line1: "...for longer time periods including all seasons, are needed..."

Page8536 line3: "Taking into account the soil redistribution process related to the snowy season,..."

Page8537 line1-2: "The aim of this study is to quantify soil erosion and deposition rates due to snow movements related processes..." Page8537 line5-6: "...including all the soil erosion processes."

Results Page8546 line11: Added after "..human activity." "Moreover, these values, both from conventional and 137Cs approaches, are higher (about ten times) than the values reported by Lana-Renault et al. (2011), who, in a field experiment in a Mediterranean high mountain catchment, found a yearly sediment yield triggered by runoff processes, ranging between 2.1 and 2.9 Mg ha-1 yr-1 (43-35 % of which due to snowmelt runoff).

Page8548 line2: Added at the end. "Moreover the estimates of the erosion due to the avalanche activity are significantly greater than the erosion rates related to runoff processes during the growing season only. For example, Konz et al. (2010) found values in the range of 0-68 kg ha-1 mo-1, and also other studies generally confirm low erosion rates during the vegetation period (e.g. Felix and Johannes, 1995; Simonato et al., 2002), even if the effective magnitude of this process strongly depends on the precipitations amount during the summer period (Felix and Johannes, 1995)."

Conclusions Page8548 line16 to the end: "...similar magnitudes of soil redistribution rates. Consequently the soil erosion due to snow movements could be considered the main driving force of soil redistribution in the area, with a greater impact in comparison to the runoff processes occurring in the growing season and snowmelt period. In particular in areas with no avalanche release (ASB), the considerable erosion and deposition of soil particles from the upper part of the basin was likely related to snow gliding. Therefore erosion processes due to snow related processes should be taken into account in the assessment of soil vulnerability on mountain areas."

RC: page8539 line6: As the release and track areas were calculated with GIS on the basis of the georeferenced pictures taken after both avalanche events, it surprises, that both events span exactly the same area (Tab.1) page8544 line14: the total area was considered as equal - why? AC: Sorry, we made a mistake in the table reporting two times the value of the release and track areas of 2010 event; the correct number for the 2009 event was a little smaller than the value found in 2010 (50796 m2). We will correct it in the table.

RC: page8544 line2: comparable: identical? AC: here the area values referred to the deposition area (LDA), measured with GPS, equal to 6889 m2 in 2009 and 7810 m2 in 2010.

RC: page8542 line17: A more detailed description of ho could be helpful. AC: we agree and we would like to change/add as following: ho = profile shape factor ($Kgm-2$) is a coefficient describing the rate of exponential decrease in Cs-137 concentrations with depth for a soil profile in an uncultivated site.

RC: page8544 line6 and line17: There probably could be more references than the results of Freppaz et al. (2010) and Bozhinskiy and Losev (1998). AC: According to our knowledge these are the few papers, dealing with measurements of solid transport by snow avalanche, that have results, in term of applied methods and measure units, comparable with our data. Thanks to your suggestion, we continued the bibliographic investigation, and we will cite more references in the revised version, as for example your interesting contribution on Soil erosion due to avalanches: measurements on an avalanche cone, presented at International Symposium on "Snowmelt and Related Problems" that took place in Oslo and a work by Heckmann in the Bavarian Alps.

RC: page8563 Fig.9: There is some description missing: blank dark points .. outlier? a .. ? AC: We integrate the missing descriptions as following: Old: The full dark points indicate mean values. New: The dark full circles denote mean values, the blank dark circles represent outliers (i.e. more than 1.5 times the interquartile distance). The black

HESSD
8, C5292–C5304, 2011

line is the median. Letters a) indicate that there are not significant differences between the means.

Answer to Molnar Specific Comments

Uncertainties in the estimated soil erosion and deposition rates RC: My first suggestion is that the authors expand on the uncertainties involved in estimating the soil mass, or total sediment load, in the avalanche deposits and subsequently the deposition rate. Could the authors propagate the uncertainties in the sediment concentration measurements (surface and subsurface) which they actually report as a range in Table 1, into the volume and rate estimates? It should also be clearly stated where this uncertainty comes from, i.e. what are the involved errors, e.g. the vertical and spatial distribution of sediment in the avalanche mass, the assumption of uniform erosion rate over the avalanche track, the point sampling of the caesium-137 concentrations, etc. The same is true for the Caesium-137 derived estimates, which have their own uncertainties, which could be propagated into the eroded volume and rate. Error bounds on the estimates in Table 1 would be very helpful for the reader to understand the significance of the results. AC: According to your useful suggestions, we will underline the uncertainties present in both methods and we will describe the assumptions that originate from those major uncertainties. Moreover we will modify Table 1: we will present the data separated between surface and subsurface and then we will expand the error bounds to all the other data. The same will be done for Caesium-137.

RC: I have some additional questions about the assumptions. For instance the authors assume that the sediment is well mixed in the avalanche body for computing the total sediment load. Is this truly the case in nature, where I imagine the accelerating snow mass is entraining sediment primarily at the front of the avalanche, and the likelihood of complete mixing in the avalanche body is rather low? Or am I wrong? What did the authors find from their sampling of the avalanche surface and body? Similarly, the source of the total sediment load is assumed to be uniformly distributed over the entire avalanches release and track area. How good is this assumption and how relevant is this question? AC: Relatively to the assertion that the accelerating snow mass is entraining sediment at the front of the avalanche, this can be true, but also at the basal level, i.e. at the bottom of the avalanche body, the soil erosion is very active, due to the shear friction. And during the movement the mass can be mixed up. Most part of the sediments was concentrated in the upper few cm of the snow deposit. To confirm this, we dug snow pits in the avalanche body and we sampled the snow vertically: the sediment found in the inner body was very low and spatially homogeneous, with a low standard deviation. So we assumed a good vertical mixing. Regarding the horizontal spatial distribution of sediments at the avalanche surface, we tried to make geostatistical analyses (GIS), but we didn't find a clear pattern of spatial distribution. This confirmed the first visual impression, that the surface sediment on the avalanche deposit, in both years, seemed homogeneous in its distribution. The total absence of a sediment distribution pattern in the run-out area might be explained by the morphological characteristics of the avalanche path and by the kind of avalanche (full-depth, wet snow, small size). The avalanche path is very steep and channeled in the lower track area and suddenly the steepness decreases in the run-out area, so that the heavy mass of snow suddenly stopped without the space and power necessary to run more and to redistribute the sediments horizontally.

Concerning the source of the total sediment load, the hypothesis of a uniform distribution over the entire avalanche release was chosen because it is extremely difficult to investigate if the considered areas are differently affected by soil removal. In the calculations we didn't use the maximum event reported in the Regional Avalanche Cadastre, but we used the area effectively covered by the two considered events. By this way we avoided an overestimation of the area where soil erosion due to these events could have happened. We will add some of these considerations in the revised paper, both in the material and methods and results sections.

RC: The statement that “snow related soil deposition rates varied between 28.2 and 160.7 Mg/ha/event“ is misleading. It gives the impression that these are the lower and upper limit of a range. But you only have two events and two estimates, so it would be more fair to say that the snow related soil deposition rates were 28.2 and 160.7 Mg/ha respectively for both events. The same is true for the soil accretion rate mentioned in the same paragraph and elsewhere in the paper. AC: We fully agree and we will correct it also reporting uncertainty ranges in brackets.

RC: Do the numbers in paragraph 5 page 8545 agree with Fig 3? AC: Yes they agree: the sum of the average areal activity density reported for each 3 cm layer is the Caesium-137 baseline level.

RC: Comparison of avalanche and radionuclide data. The authors conclude that the estimated rates of erosion by the two avalanches roughly correspond to the annual point erosion estimates from radionuclide dating within the avalanche track. This correspondence would be true if on the average one avalanche occurs per year in the study area. In the conclusions the authors indicate that they have some information on avalanche occurrence, writing that "in the last few years the frequency of full-depth avalanches has increased". Would it be possible to give some more data about this? AC: The study area is for sure a typical avalanche site prone to full-depth, wet snow avalanches. According to the Regional Avalanche Cadastre in the last four years the frequency seemed to have increased. But we have to consider that, historically, the Avalanche Cadastre of the Region Valle d'Aosta was used to record mainly the events involving human activities. Specific investigations carried out in the area, based on the folk memory, let us assume that the frequency of this kind of avalanche events (ground, wet snow) in the last decades was constant and almost yearly. So we will modify the assertions relative to the frequency of the full-depth avalanche in the revised paper.

RC: Furthermore, the author's own results suggest that it is not only avalanche occurrence but also individual avalanche size that matters in terms of erosion capacity. How comparable are then the long-term rates with the collected avalanche data? AC: The two investigated events were large-sized slab avalanches (more than 10000 m3 involved), even if, as reported by Freppaz et al. (2010), the erosion intensity sometimes is higher in the case of small avalanches than bigger avalanches. For sure they were quite important in terms of amounts of transported sediment. We think that the comparability between the two methods could be limited by their different time scale, but, even hypothesizing that not every year similar events could have occurred, the magnitudes of soil redistribution rates estimated with the two methods are comparable, and can suggest that soil erosion due to snow movements may be considered the main driving force of soil redistribution in the area. We will add consideration on the comparability between the two methods also in the results section 3.4 and 3.5.

RC: Finally, the hypothesis in the paper is that a comparison of the two approaches will provide information on the relative importance of snow-related processes on soil erosion at the site. The authors seem to avoid this question in the end. AC: A similar comment was made by Dr. Kohl, and we tried to solve this with some revision in the discussion and conclusion (see answer to Kohl)

RC:I think their data provide a very nice quantification of soil erosion rates along an avalanche path, and there is some agreement with long-term rates, which suggest that avalanche-driven dynamics are important. However, what about taking a catchment vision? How many avalanche paths are there in a typical Alpine catchment where erosion at these rates is taking place? What is then the part of avalanche driven soil loss in the total soil loss on a catchment scale? In fact the deposition rates above the snow bridge (Fig 4) are also quite high suggesting that snow gliding, soil creep, and other processes like vegetation trapping which the authors mention are probably also very important. I would like to read more of the author's opinions on these questions, even if they are hypotheses connected only to their single study site, in their paper. AC: Considering a larger catchment scale, it is not easy to say how many avalanche paths could be normally affected by these kinds of phenomena. If we consider, for example, the Ferret Valley ($\sim$ 700 ha), in Northwestern Italy, close to the Monte Bianco Massif, where our study area is located, the data from the Regional Avalanche Cadastre didn't normally report if the avalanche events in the past have caused a significant sediment transportation. In the last six years the Regional Avalanche Cadastre has been improved and more photos and data have been collected than during the previous years, allowing the possibility to detect the avalanches characterized by a "dirty snow" in the runoot zone. The intensity of soil erosion driven by these events can obviously change on the basis of the different morphological features, such as aspect, elevation, and soil and vegetation characteristics. We think that it is a good idea what Dr. Heckmann suggested in his last comment, to calculate the areal proportion of terrain units favourable for "winter processes", e.g. steep alpine meadows or hillslopes with a low roughness. In fact we are already working with Gis, trying to cross the avalanche perimeters registered in the Avalanche Cadastre with a land use map available for the Valle d'Aosta Region. In this way we would like to detect the amount of avalanches which have been released in the same kinds of terrain that we found favourable for snow gliding and full depth avalanches (e.g. steep alpine meadows with low roughness), potentially prone to soil erosion. We would like to add some of these considerations in the paper, focusing on the Ferret Valley.

Answer to Heckmann Specific Comments

RC: In addition to the comments given by the previous reviewers, I would like to ask the following questions: âĂć p8537 l15: Does that imply multiple events during one season ? Or does "frequent" mean that a full-depth avalanche (which removes per definition all of the snow in the release area) occurs almost in every year? AC: In the study area full-depth avalanches occurs almost every year, and during some winter seasons also multiple events occurred. We will add this specification in the study area description (see also answer to Molnar).

RC: p8539 l8: It should be explained how snow depth (and its spatial distribution) was measured/estimated. The accuracy of this assessment is important because it directly influences the calculation of sediment mass deposited by the avalanche. I can imagine, however, that the area which is sufficiently accurately determined by GPS and/or georeferenced (aerial ?) photos should be much more important as the lion's share of the sediments is located on the surface of the snow deposits (see Fig. 2). Could the authors also comment on why this might be the case ? AC: The snow depth was measured by a 3.5 m snow probe, even if the snow density in the run-out area was really high. For the calculation of sediment deposit we used a mean value. Since the deposit was quite small and uniform in the shape, and considering also, as you said, that the area was much more important than the snow height, we believe that this approach is valid and sustainable in the field. The fact that most of the sediment accumulated in the surface of the avalanche deposit, might be explained by the hypothesis of the inverse segregation: the turbulent movement of the snow mass is able to push most of the sediments (soil rock and organic debris) to the surface and then the latter remain on the surface because of the different density and viscosity between the two materials (snow is more dense and viscous than sediment).

RC: p8539 l20(Fig.2): I have the impression that the small size of the sampling mask is problematic and has to be dealt with in an error estimation of the avalanche sediment yield. Looking at Fig.2, it can be clearly seen that the sediment cover on the avalanche snow is not at all homogeneous at that scale, at least not as much as it would look within a 50x50 cm or 100x100cm mask (sizes which have been used in previous studies on avalanche sediment transport). If one had taken the samples 20 cm away, it looks like one could have sampled only half of the material, or much more, e.g. with the single rock particle left of the sampling mask. This rock particle led me to another question: Did the authors only sample "soil", i.e. without larger rock particles (the procedure outlined in 2.2.2 suggests that)? If so, is this due to the comparison with 137Cs derived erosion rates? If not, is there at least an estimate of the proportion of coarse sediment in the bulk sediment? Do the samples include inorganic sediment only? If the samples contain a large amount of organic debris, it would lead to an overestimation of sediment volume. In my own work, the proportion of organic particles could sometimes reach >40 % ? of the total mass... AC: We fully agree, this is maybe a rather weak point: we chose this mask size in order to: a) make sampling easier and faster as the avalanche run out area was obviously non a safe zone; b) try to avoid the extreme irregularity of the avalanche surface; c) to reduce the amount (and the weight) of the samples. Anyway we think that the use of a rather dense grid sampling could be enough to assure a good estimate of the amount of sediments entrapped by the snow avalanches. We observed that most part of the sediment was represented by “soil”, a mixture of small mineral and organic particles. We excluded large organic debris and rocks with a diameter greater than 5 cm.

RC: p8540 l5: Error estimation is important here (methods: which errors were assessed and how) and in the results section (result +/- error estimation) (c.f. comments by Peter Molnar) ? In Table 1, only the sediment concentration data are given with an error estimation, and it is not clear if the latter comes from the determination of snow volume or from the scatter of the samples. AC: (c.f. answer to Molnar); the error estimation in table 1 comes mainly from the scatter of the samples.

RC: p8544 l7: I would like to join Bernhard Kohl in his plea for (a little) more reference data here. AC: (c.f. answer to Molnar); and we would like to add also Luckmann, 1971 and Caine, 1969.

RC: p8544 l10: In my opinion, this is a major point of concern: The authors state that in previous years, full-depth avalanches had not occurred once a year ("only few events") with the exception of the last four winter seasons (2007/2008-2010/11). I have my doubts that under these circumstances data from two events can be taken 1:1 as yearly accretion rates, a) because of the large natural variability of event sediment loads (previous work has shown that the sediment load of avalanches is only loosely related to factors like avalanche size or substrate) and b) because of the temporal variability of avalanche occurrence. In the present study, the obviously well-documented record of avalanche activity (back to 1986 or even further ?) should make it possible to make at least an "educated guess" on the yearly transport rates on the basis of a) the measured data of two events and b) the frequency of potentially sediment-transporting avalanches as documented in the official record. This is important because it might affect the conclusion concerning the ratio of directly measured and 137Cs-inferred accretion rates, p8548 l10ff. AC: see answer to Molnar

RC: p8544 l18: see comment in l7 and, again, Bernhard Kohl's reviewer comments. However, a comparison of erosion rates in the existing literature is difficult because different authors use very different reference surfaces (contributing area of the snow deposit or a subunit of it, total area of the avalanche, only release and track areas etc...). AC: see answer to Kohl

RC: p8546 l20: This suggests that snow-gliding is a selective transport process which entrains fine material while leaving coarser fractions behind. From my personal understanding of the process "snow gliding" (more or less homogeneous movement of the whole snow cover over smooth surfaces, tearing out clods of soil e.g. where vegetation or other roughness elements were frozen in the snowcover, and/or "bulldozing" effects), I cannot fully follow here. I can however imagine that slope wash (or even rill erosion) may occur on areas stripped of vegetation/topsoil by snow gliding, and the transport of this material then occurs selectively leading to the observed sorting of the deposits. Hence, snow gliding not only directly causes removal and redistribution of soil but also conditions other soil erosion processes. Here is a possible reference: Stocker, E. (1985): Zur Morphodynamik von Plaiken, Erscheinungsformen beschleunigter Hangabtragung in den Alpen, anhand von Messungsergebnissen aus der Kreuzeckgruppe, Kärnten. In: Mitteilungen der Österreichischen Geographischen Gesellschaft 127: S. 44–70 AC: We fully agree, and we add that in our case we observed soil sedimentation in the above snow bridge (ASB) site during early spring (Fig. 7), just after the starting of the snowmelt and before rain storms, which could have been occurred under the snow, mainly in the areas stripped of vegetation/topsoil by snow gliding. Maybe in the assertion p8546 l20 we could explain better that not in general, but in this case, with a gentle slope (<25°), and not yet evident signs of soil erosion like in other avalanche sites, snow glide has not enough force to transport stones. Thanks for the reference.

RC: Fig8: Is it possible to include all sampling areas in their topological order in this

HESSD

8, C5292–C5304, 2011

Interactive Comment

diagram to identify the consistent pattern of erosion and deposition derived from your samples? AC: of course, good idea!

RC: Peter Molnar has made an interesting suggestion concerning the interpretation of the results with a catchment perspective. In this respect, it would be interesting to see not only the number, but the areal proportion of terrain units favourable for "winter processes", e.g. steep alpine meadows or hillslopes with a low roughness. AC: Yes, this is a good idea, by using GIS and also the avalanche perimeters registered in the Avalanche Cadaster…see answer to Molnar last comment. Besides the integration added in this paper, we think it could be very interesting to further carry out this research on a larger basin scale, for example for the whole Valle d'Aosta Region , using all the data available, such as land use map and data from the avalanche cadastre.

Referees Technical Corrections: AC: Thanks, we will correct all the technical suggestions in the revised manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 8533, 2011.