

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.

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Received and published: 11 November 2011

1. Introduction

The paper addresses the role of snow-related processes on soil erosion in mountain environments. Although snowmelt and especially avalanche driven erosion of the soil and redistribution of sediment are clearly important processes on steep mountain hill-slopes, relatively few studies which quantify the rates involved can be found in the literature. This paper is a good step and contribution in that direction.

The aim of the authors is to quantify soil erosion and deposition rates due to snow glid-
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ing and avalanches in their study site by measuring the sediment accumulation in the snow deposition area over a period of 2 years. Furthermore, by sampling radionuclide (caesium-137) concentrations at several points, they assess the longer-term net soil loss or gain since 1986, including winter and summer erosion processes. The hypothesis is that a comparison of the two approaches will provide information on the relative importance of snow-related processes on soil erosion at their site.

In my opinion the key results in the paper are (a) the estimated rates of erosion/deposition by the two avalanches; and (b) the demonstration of the pattern of erosion/deposition rates in the different areas which the authors sampled (Fig 4 and Fig 9). I find the data and the paper interesting and in my opinion it deserves publication. In my discussion below I would just like to raise a few questions that I had – and I think many readers of the paper will have. Hopefully the authors can expand on these questions in their revision of the paper.

2. Uncertainties in the estimated soil erosion and deposition rates

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? 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. 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. Error bounds on the estimates in Table 1 would be very helpful for the reader to understand the significance of the results.

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 avalanche release and track area. How good is this assumption and how relevant is this question?

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. Do the numbers in paragraph 5 page 8545 agree with Fig 3?

3. 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? 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?

Finally, the hypothesis in the paper is that a comparison of the two approaches will pro-

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vide 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. 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.

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

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