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Solid Earth
Discussions

Interactive comment on "Spatio-temporal dynamics of sediment transfer systems in landslide-prone alpine catchments" *by* François Clapuyt et al.

Anonymous Referee #2

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Dear Editor,

Clapuyt et al. investigate the sediment contribution of the Schimbrig earthflow (Switzerland) to the sediment flux of the entire Entle catchment with a particular focus of hillslope-channel coupling. They quantify the sediment flux of the earthflow on annual, decadal and millennial timescales by combining previously published data of sfm-analysis of aerial photographs (annual) and time-series of photogrammetry-derived DEMs (decadal) with new and previously published 10Be-derived denudation rates (millennial). They conclude that sediment contribution from the earthflow to the fluvial system is highly stochastic and that the contribution of earthflow material of the last

∼50 years makes up for more than half of the total sediment volume exported from the Entle catchment on average over millennial timescales.

Different techniques of measuring sediment fluxes allow us to estimate average fluxes exported from catchments over different timescales. Our knowledge on the variability of sediment production on hillslopes and its supply to river channels however is still limited. As such, I consider the manuscript of Clapuyt et al. as a valuable scientific contribution. While I appreciate the presented datasets and their comparison, I have two major concerns regarding (1) the analyses and interpretation of the 10Be data as well as (2) the presentation of the concepts. In addition, I raise a few minor concerns and provide further line-by-line comments, which are mainly related to the clarity of the manuscript and should be considered as suggestions. I suggest the manuscript for publication once the main concerns have been addressed.

Major comments

(1) The authors measure 10Be concentration in fluvial sediments, from which they calculate catchment average denudation rates as well as sediment fluxes by multiplying the denudation rates with the according catchment areas. When catchment-average denudation rates are calculated from detrital 10Be concentrations, one of the main assumptions is that each part of the catchment is equally represented in the sampled material. This assumption is violated when a sample is taken within or just downstream of a landslide deposit, because landslides are highly stochastic processes (as stated by the authors for example on p. 2 l. 3, p. 3 l. 10&12 or p. 13 l. 29). This is the case for the samples collected within the Schimbrig river. In such settings, the 10Be concentration in fluvial sediments collected at a certain moment in time is not necessarily representative of the long-term average and might be highly variable from year to year. Previous studies that have nicely demonstrated this are for example Dingle et al. (2018) or Lupker et al. (2012). For that reason, 10Be concentrations in fluvial sediments in landslide-prone areas are rather indicative of certain hillslope-erosion processes, but should be handled with care regarding the calculation of absolute values,

such as denudation rates or sediment fluxes. This problem also becomes apparent when the 4 data points from the Schimbrig catchment are compared with each other (Fig. 2). The last row within each box gives the calculated sediment flux (in volume per year). The sample located highest up within the catchment (CH-ENT-3) indicates a total annual sediment flux of 900 m3. When moving down the channel, the total annual sediment flux must increase, as the sediment discharge includes at least 900 m3 from the upstream part and additional sediment from the newly added catchment area. The values downstream, however, are about two thirds lower. As such, a reduction of sediment flux in downstream direction, despite total sediment flux being a cumulative parameter, clearly indicates a bias in the method. For the reasons listed above, I recommend the authors to be more careful with any of their mass-balance analyses that are based on calculated denudation rates and sediment fluxes from the landslide/ earthflow affected catchment. In particular, I disagree with the statement given for the temporal upscaling (section 4.1, p. 15 l. 8-9). The disagreement between decadal and millennial sediment fluxes can be purely a methodological problem. This also includes the comparison between the two Rossloch sub-catchments (p. 13 l. 7-11). The authors mention in their manuscript that also the gorge area is affected by landslides (p. 4 l. 27-29). Consequently, also the sample taken at the catchment outlet (E-7a) might be biased by mixing with low 10Be concentrations from landslide material. If so, the mass-balance exercise within the spatial upscaling (section 4.2., p. 15 l. 16-20) might also be biased. To address the above challenges, I suggest the authors to carefully re-evaluate their denudation rate and sediment flux analyses and interpretations and include a new section to the discussion that critically discusses the potential biases of the applied 10Be method and how this would affect their presented results.

(2) Secondly, I consider the discussion as largely under-cited. Although I really appreciate the detailed analysis of a single earthflow and the quantification of its contribution to the total sediment flux, the presented study is not the first study that has measured 10Be concentration in a landscape with stochastic sediment input, looked at evacuation timescales of stochastically supplied sediment or the potential alteration of sedimen-

tary signals along sediment routing systems. None of the previous studies are cited in the discussion though. Rather, large parts of the discussion do not refer to any other studies at all. This includes most parts of the spatial upscaling (section 4.2) as well as large parts of the conceptual upscaling (section 4.3). To better highlight the novel findings of this work, the current study needs to be better embedded in the existing literature. A few suggestions for different topics are listed below, but many more are available. 10Be concentration in regions with stochastic sediment input: Puchol et al. (2014), Kober et al. (2012), West et al. (2014) Modification of sedimentary signals: van de Wiel and Coulthard (2010), Simpson and Castelltort (2012) Timescales of sediment removal provided by stochastic events: Hovius et al. (2000), Wang et al. (2015)

Minor comments

To better understand the novel contribution of the presented study, I suggest a clearer statement of the knowledge gap/ open question that is addressed by this work. In the current version the according statement within the abstract is rather vague (p. 2 l. 5-7). In the Introduction, the background knowledge is built up, but no clear research question is formulated. A good opportunity would be to insert a sentence on p.3 after line 25. Maybe it would also help to move this explaining sentence (p. 3 l. 27-29) further up before stating the question, as it can be seen as a motivation.

Please provide a more detailed characterization of the Schimbrig catchment, especially regarding the activity of hillslope processes apart from the earthflow itself (maybe add to p. 4 after l. 29). Could other processes within the catchment also affect the fluvial 10Be concentration? Along the same line, I would very much appreciate a photo of the Schimbrig earthflow.

p. 6 l. 18-26 and p. 8 l. 10-22: Please provide a more detailed explanation of decadal sediment flux method, as it is done for the other two methods. In particular, please indicate the areal extend covered by this methods (for example in figure 2). If I understand correctly, the annual analysis only covers the earthflow itself, while the decadal analysis covers the entire catchment. To be able to compare the two, it would be interesting to know what other erosion processes are active in the catchment (see comment above) and what percentage of the catchment is affected/covered by the earth flow. Also, how is the displayed mass calculated (p. 8 l. 15-17)? I don't understand how this data is derived.

To ensure reproducibility of 10Be calculation and potential later re-analysis, please provide the raw data with the manuscript. This includes the original 10Be/9Be ratios from the AMS, as well as all the parameters needed to run the CAIRNs model. Also, was a correction for non-quartz containing areas within the catchments, as for example the carbonates, applied?

Line-by-line comments

p.3 l. 33-34: The sentence does not make sense as it is, please correct.

p. 4 l. 6-10: I suggest to number the analyses that are performed, as it makes it easier for the reader to follow the manuscript. However, I don't fully find the structure indicated here in the rest of the manuscript. Rather, the addressed topics are (i) temporal upscaling, (ii) spatial upscaling and (iii) conceptual upscaling. For clarification, I suggest to adapt this sentence, at least its order, or the way the data is later presented.

p. 4 l. 6: Inconsistent use of tenses, stick to one: 'discuss' is present tense, 'quantified' in past tense

p. 4 l. 16-19: As the 10Be concentration in fluvial quartz is measured later, it would help to provide information on the lithology/ quartz content in addition to the depositional types (molasse, flysch).

p. 4 l. 24-25: I don't follow the argument here. Why do differences in denudation rates point to a supply-limited system?

p. 6 l. 19: Is 'sediment yield' the same as 'sediment flux'? If so, consider changing it to flux to be consistent. Otherwise please define yield.

p. 6 l. 24: Was loose sediment or solid rock converted from tons per year into cubic meters per year? If it was converted from sediment, I would expect a lower density than 2.70 g/cm3.

p. 7 l. 1: In this sentence the authors state twice that their sample preparation was similar to other studies. What does 'similar' mean? Please be precise. Same accounts for the term 'several' in line 3.

p. 7 l. 7: Change 'is' to 'was' to be consistent in tenses.

p. 7 l. 27: What is meant by the term 'dynamic equilibrium'? Does it summarize what has been explained in the previous line, i.e. no net changes in volume? The way the sentence is written sounds to me like an interpretation, which would be miss-placed within the results sections.

p. 8 l. 22: I suggest to stick to one term, for instance earthflow when referring to the Schimbrig earthflow. In this sentence it is unclear if the 34 % come from the earthflow or also from other landslides that are active within the catchment? This is what motivated my comment above regarding a more detailed characterization of the hillslopes in the Schimbrig catchment.

p. 9 l. 15: It is unclear to which samples the term 'landslide-affected' refers to. For clarification, it would help to indicate in Table 3 which of the samples are considered as landslide-affected. I assume the term includes the 4 samples from the Schimbrig river. But why are 5 stars (= landslide-affected) displayed in the Fig. 3 and 4, but only 4 samples in that catchment? And is the Schimbrig earthflow the only landslide in the entire study-area, or could other samples also be considered as 'landslide-affected'?

p. 10 l. 6-7: I don't follow this interpretation. An increase in denudation rates in downstream direction could also be related to different local uplift rates, changes in lithology or recycling of the glacial till material (and as such not give 'true' denudation rates). Also, as this phrase is rather interpretation than a description of the results, the authors could consider moving it to the ‘Discussion’ section of the manuscript.

p. 10 l. 6-7: I don’t understand the sentence. What is meant by ‘Accounting for the drainage area. . .’? Is the data displayed in Fig. 4 normalized by catchment area? If not (and it doesn’t seem so), wouldn’t an increase in sediment flux in downstream direction be expected as the sediment flux gives the total volume of sediment evacuated from a certain area per time? Consequently, the larger the area, the higher the sediment flux, even if denudation rates were constant across the entire area. Along the same line, I don’t follow the statement on p. 12 l. 2-3.

p. 12 l. 16 - p. 13 l. 2: This sentence is rather discussion than a description of the results. Regarding its content, another possible explanation is that the fluvial sediments gets mixed with other, high 10Be sediment from within the catchment. This depends on what other processes are active within the catchment (see earlier comment).

p. 13 l. 6: Consider to also refer to Fig. 2 as this figure shows the variability in sediment fluxes across the entire study area.

p. 13 l. 20: km-2 yr-1, is that the correct unit?

p. 14 l. 18 – p. 15 l. 1: I suggest to replace ‘the difference in denudations rates. . .’ with ‘the difference in 10Be concentration’ as the denudation rates calculations are biased by the landslide and thus not reliable (see comment above).

p. 15 l. 2-3: What difference? The difference in sediment flux? And if it refers to the sediment flux, what about the other samples within the Schimbrig catchment? The uppermost sample (CH-ENT-3) already suggests an annual sediment evacuation of 900 m3, which is significantly higher than 230 m3 (CH-ENT-9). As such, I think the calculation of sediment flux from 10Be concentration in the earthflow affected catchments needs to be taken with care.

p. 15 l. 10: The importance OF landsliding. . .?

p. 15 l. 11-12: Or by a bias in the method, especially the 10Be derived sediment flux

calculations (see comments above).

p. 15 l. 19-20: If a mass-balance analysis is done, how about the other tributaries? If the contribution of all catchments is summed up, does it result in 100%?

p. 16 l. 6: Remove n from Entlen?

p.17 l. 15: ‘pulses’ instead of ‘pulse’?

p.18 l. 11: Redistribution on the hillslopes, or just within the earthflow affected area? Please clarify.

p. 18 l. 21: Where does the 90% come from? Is this calculated from the data?

p. 18 l. 25-29: This statement is rather an interpretation about the evolution of such landscapes, which cannot directly be drawn from the presented data. Or if it can, I did not understand how it can be known from the presented dataset that once a sediment source is depleted, another landslide will be activated. Unless I missed something, I suggest reformulating the sentence to indicate it as an hypothesis that needs to be tested in the future.

Fig. 1: The elevation as supposedly shown in grayscale (legend) cannot be seen in the figure. I suggest to have two maps: one showing the DEM, and one showing the geological map. Maybe include a photo of the earth flow.

Fig. 3 and 4: The authors should consider to use different colors as red and green cannot be distinguished by a certain number of people.

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