We would like to thank Luca Malatesta for the constructive comments and suggestions, which will guide us throughout the revision of the manuscript. Below, we respond to his suggestions and comments.

Dear editor,
Clapuyt and colleagues present new constraints on geomorphic activity in an alpine catchment from cosmogenic radionuclides and from drone surveys (structure for motion). This new data complements existing constraints on sediment fluxes and allows a view across timescales from 10ˆ0 to 10ˆ3 yrs. The authors observe that the episodic activity of one earthflow leads to the production of a volume of sediment equivalent to that of the entire catchment, but that the evacuation of this material is limited by transport efficiency through the catchment such that earthflow activity is unlikely to imprint the stratigraphic record.
The science in this paper seems sound. I simply have one concern regarding potential recycling of glacial sediment for the cosmogenic radionuclide erosion rates. And I am confused by the interpretation of the catchment as a supply-limited vs. transport-limited system.
The language and the figures of the article are good. Though the dynamics of the earthflow are too summarily described and I have not really understood what is actually measured.
The manuscript, however, could be much improved by reworking its structure. At the moment, the novel contribution of the authors is somewhat buried under a discussion of known elements. I strongly encourage the authors to rethink the introduction and the motivation of their study to increase its impact. I provide some suggestions below.
All in all I recommend to accept this manuscript once the issue of potential sediment recycling is addressed and after 1) the sediment dynamics of the earth flow are more clearly defined and 2) the novel elements of the article are better highlighted.

We take note of the recommendations and will address them carefully when revising our manuscript. Below, we provide a detailed reply to the issues raised.

Sediment recycling: Figures 3 and 4 show a downstream increase in erosion rates and sediment fluxes once the Entle river flows in the inner gorge. The authors attribute this to the fast rate of postglacial incision in the gorge. To me, it however seems that the recycling of buried glacial sediment could be at least in part responsible for the trend of apparent increase in erosion rates caused by the increasing admixture of sediment with lower CRN concentration. If that is not a driver behind the increase in incision rate it should be explained. And if, on the contrary, this plays a role, this should be quantified.

The Entle catchment was covered by glaciers during the LGM (24 ka BP, Bini et al., 2009). In the central part of the catchment, a 7 km-long inner gorge cuts through 100 m thick unconsolidated glacial deposits. The onset of incision of the inner gorge is still subject to debate. Van den Berg et al., (2012) argued that the incision age can be postglacial, given the high erodibility of the glacial material and high incision rate that might have operated during most of the knickzone propagation time.

Norton et al., (2008) showed that the ^{10}Be concentration of the glacial deposits in the Trub area (15 km to the W of our study site) equals $0.76 \pm 0.13 \times 10^4$ atoms g qtz^{-1} at 8.5m depth. Also, they reported concentrations of $3.58 \pm 0.33 \times 10^4$ atoms g qtz^{-1} at 1.5m depth, about 15%

to 50% higher than the catchment-wide CRN concentrations of nearby rivers. These data indicate that the decrease in ^{10}Be along the Entle River (Fig. 3 & 4) is not necessarily linked to the incorporation of buried glacial material in the inner gorge.

In agreement with earlier work by Korup and Schlunegger (2007) and Van den Berg et al. (2012), we attribute the downstream decrease in ^{10}Be concentrations to the contrasting geomorphic regimes between the inner gorge and the hillslopes above the knickzone.

We will discuss this point more extensively in the revised manuscript.

Meaningfulness of the earthflow sediment dynamics: I do not think that I correctly understood what was being surveyed and what that entails for the sediment cascade. I have commented several parts of the manuscript (attached) where I might have been confused by a lack of clarity. Surface lowering on the earthflow is described as being the result of erosion (p. 6 l. 10-13). Isn't it also due to subsidence of the surface? I would expect erosion to mainly affect the bulging parts of the flow and mitigate the rate of surface uplift. Are subsidence and erosion differentiated? The lowering and rising parts of the flow both do so at the exact same mean rate. This is a rather arresting coincidence. It could be useful to add one sentence to explain/confirm this to avoid it being perceived as a red flag! The net mass flux of the earthflow is close to zero. This implies a constant volume though time. Then wouldn't the throughput flow, instead of the net balance, be the quantity that matters for sediment yield from the earthflow? Or alternatively, considering firstly the flux from bedrock to sediment (production) and secondly the loss of sediment from the earthflow to the channel (transfer). It would be potentially useful to reproduce a figure of deformation (bulging/lowering) on the earthflow in the manuscript to contextualize the values.

Thank you for this comment. The paragraphs related to the annual sediment dynamics of the Schimbrig earthflow will be rephrased to clearly highlight what is measured and how we can further interpret these datasets. We will also add figures of the earthflow and its deformation, so that this part can stand alone, without making much reference to our previous work in Clapuyt et al. (2017). The sentence in p.6 l.10-13 stating that surface lowering is equivalent to erosion is an overstatement. Indeed, surface lowering does not necessarily mean erosion, especially in this context, as surface lowering occurs in the upper part of the earthflow while surface bulging occurs in the lower parts, i.e. highlighting the rotational structure of the mass movement. From the annual assessment of the earthflow sediment dynamics, we show that surface lowering is nearly equal to surface bulging. The mass balance of the earthflow is therefore roughly in equilibrium, meaning that the sediment mobilised by the earthflow is accumulating on the hillslope, indicating that the throughput flow is negligible during the short period 2013-2015. This is, however, not the case at the decadal scale (1962-1998) where Schwab et al. (2008) highlighted the very dynamic activity of the earthflow and quantified sediment fluxes to the river network.

Sediment system: transport-limited or supply-limited? The catchment is framed as being supply-limited (p. 4 l. 24 and p. 17 l. 6-7). But it seems that the authors provide arguments for it being transport-limited at least in the first orders tributaries (p. 15 l. 25) the two conflicting accounts need to be reconciled. It is possible that a supply-limited catchment switches to being transport-limited when a landslide pulse overwhelms the transport paths.

We agree with this comment. Both field observations and quantitative data presented in this research point to the hypothesis of a transport-limited drainage system. In the discussion, we argue for the capacity of the landscape to buffer stochastic sediment pulses from landslides. We will modify the text in this sense to gain in consistency.

Structure: As it stands, I find that the article fails to properly motivate the study and to highlight the novelty the authors provide. In the discussion section, the authors use a significant amount of space to present already well-established conceptual models (sediment cascades, buffering of sediment pulses, stochasticity of landslides). The effect is to dilute the author's work. I believe it would be much more effective to introduce all these known/established elements at the beginning of the manuscript. This would allow the authors to explicitly define the gap in knowledge that their work directly addresses: a dataset across timescales, and not a conceptualisation of sediment transfer. I believe this would make it easier for the reader and increase the impact of the presented work. This section would also be the good place where to describe how the different processes affecting earthflow dynamics contribute to the sediment routing system.

We will revise the structure of the manuscript according to your suggestion, which was implemented as is in a first draft of the manuscript. To address this comment, we will first clearly state the focus of our research in the introduction, i.e. the quantification of the propagation of stochastic sediment pulses across the landscape based on a multitemporal dataset of sediment fluxes. Then, we propose to distinguish the introduction section from the theoretical section about sediment transfer in landslide-prone environments, in order to keep the introduction quite short. In the discussion, we will only focus on our results and on what we can learn from integrating sediment fluxes over different time scales in a landslide-prone environment.

I would like to encourage the authors to make better use of their data. Instead of synthetic data on the last figure, why don't they actually plot a distribution of erosion rates vs. timescale of integration (not time!) to present what is their truly significant contribution (data across timescales)? See Fig. 1 of Sadler 1981 for reference.

We will add a figure in the discussion, depicting denudation rates over time scale of integration based on our data.

The authors will find my line-by-line comments in the annotated pdf file hereby.

We thank Luca for the thorough annotations of the manuscript, which will help to significantly improve its quality. We will address the line-by-line comments in the revised version.

If any of my comments are unclear, the authors are welcome to contact me for clarification.
Kind regards, Luca Malatesta

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