Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2018-69-RC2, 2018

Earth Surface Dynamics Discussions
Open Access

Interactive comment on "Long-term erosion of the Nepal Himalayas by bedrock landsliding: the role of monsoons, earthquakes and giant landslides" *by* Odin Marc et al.

Anonymous Referee #2

Received and published: 26 October 2018

This article by Marc and co-authors presents a systematic analysis of landslide size frequency distribution in the Himalaya of central Nepal. They extract these distributions from various datasets, ranging from high resolution imagery over the last few years (covering the period before and after the 2015 Gorkha earthquake) to medium resolution imagery spanning several decades, ultimately incorporating the records of large landslides over the Holocene. One of their focus is the comparison of the distribution parameters and respective landslide volume for different types of forcing such a monsoon precipitation (MIL) and ground shaking associated with the events similar to the 2015 earthquake (EQIL). They also compare the erosion budget associated with landslides over several timescales with integrative estimates of landscapes erosion, derived from three types of methods : river sediment gauging, detrital CRN and low temperature thermochonology, spanning time frames from a few years to a few Myrs. This is, to my knowledge, the most thorough study of the frequency distribution of landslides over several timescales, in the Himalayas. This study represents a great increment in our understanding of this processes in orogenic systems and its relationship with tectonic and climatic forcings (and in particular their respective influence).

I have one main concern, which I think that the authors can easily address with moderate editing of their discussion. The authors infer landslide erosion rates for their computed distribution that they compare with independent estimates derived from other methods, which allows to point out the relative importance of landsliding (either MIL or EQUIL) in the erosion budget of the range, as well as highlighting important methodological issues with some commonly used approaches (integration time scale of detrital CRN). There is, from my point of view, a major caveat that needs to be explicitly acknowledged and discussed, as it make such comparison very difficult or at least a bit fuzzy. Landslides move large masses of bedrock and regolith on hillslopes, but over relatively small distances at the scale of the range. On the short term (to be defined) the result from a single event is basically a movement of hillslope material at a lower position on the hillslope. There is no doubt that such event will trigger an increase in sediment fluxes due to the production and exposition of easily erodible material, but such spike seems to have a time-span of years to decades from the data available in Taiwan and E Tibet. For large landslides most of the material volume (as calculated from the scaling relationship used here) is going to sit passively on the hillslope for 100s or 1000s of years. Its actual time scale of mobilization is unclear but depends on many factors mostly related to local fluvial dynamics (incision rate at the base of the hillslope, variability in transport capacity etc . . .), which are very difficult to constrain and makes the actual timescale of the “real” erosion associated with landslides quite open for discussion. This have probably no incidence over thermochonological timescales but should be explicitly addressed when comparing their results with others methods. I think that the study make a very convincing and interesting point concerning the respective contribution of EQIL and MIL to the global landslide budget over several decades, but I am less convinced by the comparison of the inferred “rates” with other erosion estimates, and I am afraid that putting to much emphasis on this part of the discussion might weaken and blur the message of the paper.

I also have some concerns with the assumption of homogeneity of many climatic and tectonic parameters over the studies area, as it is located across a very strong rock uplift and precipitation gradient. Similarly the preconditioning impact of glacial and periglacial processes in the upper part of the high range should be acknowledged.

Additional comments keyed to line numbers

55-56Âă: this is not clear from the data you present, maybe a supporting figure would help to make your point here

75-76Âă: one or two sentences to explicit the meaning of this cut off size would help understanding the data latter

114Âă: “where the absence of vegetation did not allow mapping” I think I understand why but it should be stated explicitly in this methods part.

126-128 : What about hillslopes with little vegetation cover, especially high in the range? Do you also take into account seasonal effects (e.g. Sal forests)

130 : DEM derivatives, be explicit about what they are (gradient, curvature ?), how they are calculated and what type of information they bring in.

145-146 : not directly the topic of the paper but is there a relationship between these movements and precipitation?

153-155 : I think it’s an oversimplification of the context, which is quite heterogeneous from my point of view. The bedrock geology of the northern part of the studied area is probably dominated by the Thetyan series, quite different from the HH gneisses, and they are also intensely sheared (and potentially weak) zones at several positions in the series, which might affect rock mass properties. One could also note the important variations in rock uplift between the southern and northern parts of the studied area. Also there is a strong climatic gradient across the high range, with most of the precipitation focused on the southern front. Additionally post-glacial debuttressing might be also a process to consider when analyzing the long-term record.

351 : missing)

450-455 : there are evidences of climatically modulated sediment fluxes for many parts of the Himalayan arc (at for the Holocene), so maybe you could discuss the potential influence of changes in landsliding dynamics, as compared to the contribution of glacial erosion in the high range

457 : can they be related to the glacial history in the highest part of the range?

468-470 : what about the influence of short wavelength variations in precipitation (cf Gabet et al., 2004)?

section 4.2 : Globally I am surprised by the absence of comparison of the results obtained here in the Himalayas with the Wenchuan Earthquake and the corresponding discussion of its influence on long-term topographic evolution as, for example, in the following reference : Li, G., West, A. J., Densmore, A. L., Jin, Z., Parker, R. N., & Hilton, R. G. (2014). Seismic mountain building: Landslides associated with the 2008 Wenchuan earthquake in the context of a generalized model for earthquake volume balance. Geochemistry, Geophysics, Geosystems, 15, 833–844. https://doi.org/10.1002/2013GC005067

493 : define "significant portion of the Himalayan front", topography, climatic conditions and strain partitioning are actually quite variable along strike

498 : "mountain front" is not very clear in the context of the Nepal Himalayas, are you referring to the southern part of the High Range?

519 : "Earthquakes shallower than the Gorkha event ..." : but not located at the same

place, a large portion of the seismic moment would be released farther to the south, in the Lesser Himalayas above the MHT flat

4.3 See main concern about the residence time of landslide material in the range, in particular for large events. Some of the arguments presented here seems to rely on the assumption that once landslides occurred, the corresponding material is instantaneously removed and transported away

555-560 : these comparisons between rates from different methods do not make much sense if you do not emphasis the context and particularities of these, as well as give more information about which data are used and their relevance to your area of interest. They seem to encompass very larges and diverse areas both along and across strike. In particular, to what extent are the lower bounds defined by data from the LH, which are actually outside of the investigated area?

558 : Portenga et al. (2005) actually in Bhutan.

559-560 : given the very large reported ranges are these comparison really meaningful?

570-571 : bedrock landsliding will only be an efficient erosion mechanism if river incision is able to maintain local hillslope gradients close to the critical value, and mobilize the corresponding material over the timescale of interest.

576Âă: “the observed increase in erosion rates from short to long timescales” this is very fuzzy to me, a plot of erosion rates vs integration time scale would probably help (with actual data, not just the ranges). See similar comments above.

Figure 1 : I would expect this introductory situation figure to provide more context concerning the geology and climate of the area. Additional panels (same size and extent) with the corresponding information would be necessary, from my point of view, in particular if you want to support the hypothesis of homogeneity of many of these parameters made above.

Figure 2 : add a vertical bar for the eq(s) date. Lot's of different symbol, some of which are defined in the legend text, not on the figure (orange square). Maybe use a matrix form for the legend (catchments as columns and type of inventories as rows)? Maybe add an upper panel with cumulative monsoon rainfall (same time axis)?

Figure 3 : recall the fitted parameters in the legend. A short title for each panel would help navigation (applicable for other figures)

Figure 4 : for the inset I would draw a vertical bar at 1 (and probably break the bins here)

Figure 5 : I am probably reading that wrong , but for the upper panel Y-axis label should not be "....landslide with size<A"?

Interactive comment on Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2018-69, 2018.