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Interactive comment on "Temporal variability in detrital ^{10}Be concentrations in large Himalayan catchments" *by* Elizabeth H. Dingle et al.

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In this manuscript, Dingle et al. measured the cosmogenic 10Be concentration of sediments exported from the Ganga catchment in the west-Himalaya. Their data-set combines modern river sediments with floodplain and terraces deposits spanning the last ca. 25kyrs to investigate the 10Be concentration variability in such an actively eroding system. Variability in modern 10Be signals had already been documented in such systems, but the present results are surprising in that the authors show that this variability has remained largely unchanged over such long-time spans and despite known changes in climate. The authors provided a sensitivity analysis of the plausible causes for the observed variability mainly in relation to landsliding and sediment evacuation. Their conclusions reach beyond the presented case study since it is implied that in actively eroding landscapes 10Be concentrations and erosion rates could be decoupled. Overall, I think this is an interesting and thought-provoking addition to the 10Be literature that tries to go beyond the simple interpretation of cosmogenic nuclides as "erosion rate meters" and seeks to take into account the actual erosion processes. I would therefore recommend this manuscript for publication in E-surf but I also believe that some aspects would benefit from a more in-depth discussion in the frame of a moderate revision.

General remarks:

- I am convinced by the demonstration that landsliding and landslide characteristics (location, depth etc.) can drive a significant variability in the 10Be signal if this material is mobilised rapidly within the fluvial network. Where I am a bit puzzled however, is how the models and sensitivity calculations in this manuscript compare with, for instance, the work from Niemi et al. (2005) or Yanites et al., (2009). These two papers have a thorough treatment of the landslide impact on 10Be signals (landslides are spatially and temporally resolved and include a proper landslide frequency-size distribution) but suggest that for higher order catchments the bias towards under-estimated CRN-derived erosion rates compared to volumetric rates is limited. In this work it seems that this bias is at least a factor 2 (Figure 8d), if not much more and these biases are also emphasized in section 5.3. Is this a result of how the different models are set-up or is this linked for instance to the hypsometry of the catchment (and large range of surface production rates)? I think, that the authors should be more upfront in comparing their approach with these published studies and better discuss were the apparent different conclusion may come from.

- Maybe somewhat related, I also think that the steady state assumption and what it implies for the conclusions of this study should be addressed in more details. The 0.5% of landslide surface, suggests that the landslide recurrence time at a given point of the landscape is roughly 200 years (assuming that the landslide are randomly distributed over the entire catchments). The 10Be concentration profiles are therefore likely quite far from steady state (depending on background denudation rates and production rate) and the overall 10Be concentration of eroded material much lower than expected. This may therefore well be quite a strong assumption.

- As I mentioned earlier, I find the discussion on how landslides in different sub-catchments can drive a high variability in the 10Be signals convincing but in comparison the role of sediment storage and transfer in limiting this variability quite short. It seems however that this is crucial in interpreting the data since without these dampening effects the expected variability would be much higher. Is there a way to provide a more quantitative approach to this part of the system? Since you can model the expected variability that is induced by landslides, could you for instance estimate the size of the buffer needed to filter this variability to within roughly a factor 2? The fact that this variability is preserved over such a long time-scale would suggest that this buffer capacity is a characteristic of this catchment.

- One of the important messages of the manuscript is that CRN-derived sediment fluxes likely underestimate actual volumetric sediment fluxes (and maybe by a significant amount). Our data of Lupker et al., (2012) suggests that 10Be fluxes appear similar to slightly larger than gauged fluxes for large catchments in central Nepal. This work therefore suggests that the actual long-term fluxes implied by the CRN data might actually be much larger than currently measured (gauged) fluxes if this bias is taken into account. I would be curious to have the authors opinion on whether this could be a sign of a recent decrease in sediment fluxes or just induced by a large uncertainty on both methods?

Minor comments:

- This might be wrong on my side but I would not speak of error when referring to the natural variability in the 10Be concentrations of the river sediments (e.g. abstract l.13) or when referring to uncertainty in measured data (e.g. l.5, p.3).

- The SLHL 10Be production rates that were used in CAIRN for the calculation should

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be mentioned somewhere. On the same topic, it be better to stay consistent throughout the manuscript with the use of CAIRN and not change for the CRONUS calculator for a series of sub-catchment (l.8, p.9). I know CAIRN does not explicitly need to the catchment averaged production rate estimates but it must also compute these values across the catchments. This may also explain why the entire Ganga catchment in table 2 has a production rate of 33 at/g/yr but the rest is modelled with a production rate of 35 at/g/yr in table 3 & 4.

- p.6, l.26 and Figure 4: I would keep the original sample name instead of LUPK09 to make it easier to trace across publications: BR924.

- What is the rationale for choosing the sub-catchments of Figure 7 and l.5, p.9?

- The chosen 0.5% of landslide area applies to co-seismic landsliding but is probably high for inter-seismic landsliding. Why has a such a high co-seismic landsliding value been chosen?

- L.13, p.9: see also Gorkha landslide statistics in Roback et al., 2017 (Geomorphology).

- L.2, p.8: there is a typo: draw a clearer picture

- L.1-2, p. 12: Godard et al., 2012 (JGR-ES) have contradictory results suggesting high glacial erosion in the Marsyangdi.

- L.14, p.13: I did not understand where the 7 and 10% came from.

All suggestions are meant in a constructive way and are open for discussion. I hope they will contribute to further improve the manuscript.

Maarten Lupker - ETH Zürich (17.02.2018)

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