

Interactive comment on “¹⁰Be systematics in the Tsangpo-Brahmaputra catchment: the cosmogenic nuclide legacy of the eastern Himalayan syntaxis” by Maarten Lupker et al.

Anonymous Referee #1

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General comments

The manuscript is unusually thorough and generally presents well-considered arguments. The dataset is impressive and the authors should be commended for their painstaking incorporation of previously published data. While I am not convinced that their grain-abrasion argument is the most parsimonious explanation for their observations – it is certainly a contributing factor and their thorough discussion of the model is a valuable contribution to the community. Overall, nicely done.

Specific comments

While grain abrasion may be important for the interpretation of your CRN dataset

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(amongst many other factors) it is not clear to me that it is similarly important for other single-grain detrital studies (as suggested in 27, first paragraph on p. 12, p. 16 line 15). Your analyses of published detrital mineral cooling ages is interesting, but not definitive (e.g. you do not demonstrate a significance difference between these samples using a statistical metric that accounts for sample size) and does not necessarily demonstrate that “care should be taken before relating detrital signals to spatial patterns of denudation within the NBGPm” (p. 12 13-14). I’m not saying we should be careless, but it is not clear that your data allow you to make this claim. Please clarify that, for the interpretation of relatively large aliquot quartz data, you prefer this explanation, and you *speculate* that this may also be true for single-grain analyses of other minerals.

The increased [10Be] between samples at site 7 and 8 is probably due to recycling of quartz rich, possibly high [10Be] terrace and Siwalik (Upper and Middle Siwalik here) sediments and does not reflect any sort of “inherent variability of the sediment transport system” (p. 7, 24). That variability should be reflected in your multi-year sampling, or sampling of the same place by different researchers. You should change the manuscript to reflect, or at least consider this interpretation. See also p. 8, 40 on.

I don’t find your argument on p. 8, lines 38-39 convincing since the sediment flux varies dramatically throughout the catchment area. What really matters is not actually the catchment area, but the ratio of the landslide volume to the catchment-averaged sediment flux. While it is true that the catchment area downstream of the NBGPm is very large, the sediment flux from most of that upstream area is very low (~10 Mt/yr) – so does it matter? Something to think about.

Landsliding is clearly the dominant process transporting sediment to channels in this landscape (see Larsen’s work, primarily), but is it true that these are all deep-seated bedrock landslides that produce coarse debris? These hillslopes are remarkably soil mantled. It would be worth looking at the area/frequency distribution of landslides here to see if they are actually just shallow soil slips that deliver fine sediment, not coarse debris. Something else to think about.

Technical corrections/errors/etc by line

The manuscript is clear, but a little wordy and sometimes the phrasing is awkward. I've highlighted a couple examples, but the text would benefit from a brief read-through by a couple colleagues.

p. 1 31. remove "other to" 35. Awkward phrasing, consider "As a consequence. . . , the Himalayan range is the . . . ocean." 38. Awkward, "because of variable physiography, geomorphological processes and climate." 40. A convenient, but not correct definition of syntaxes, consult geologic dictionary or rephrase to sound less like a definition, e.g. "Interactions between tectonic and surface processes are most likely observed in tectonic syntaxes."

p. 2 25. "simply reading. . ." does not need quotation marks.

p. 3 21. See work by AK Jain, Jain and Thakur (mid-late 70s in Himalayan Geology) for first mapping the region. SK Acharyya for regional compilation. These are better citations. 26. Arunachal Pradesh is the name of the state, not just "NE."

p. 8. 1. Salvi et al. is published. 5. Has this actually been demonstrated by Larsen and Montgomery? I'm not sure that any landslide density estimates are published along the Siang. Consider rephrasing.

p. 11 35. odds with 37. in detail the

p. 12 34. need to be

p. 13 1. if you have mapped the sand/gravel transition, it would be valuable to plot this in your figures. 25. not clear what you are saying after "but thermochronological data. . . " End the sentence after the parenthetical.

p. 14 18. Unclear where these number (50 and 90%) come from, please explain/rephrase.

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