Earth Surface Dynamics Discussions

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Comment on esurf-2021-27

Anonymous Referee #1

Referee comment on "Escarpment retreat rates derived from detrital cosmogenic nuclide concentrations" by Yanyan Wang and Sean Willett, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-27-RC1, 2021

Wang and Willett present two methods for converting basin-averaged cosmogenic nuclide-derived vertical erosion rates to horizontal escarpment retreat rates, and they use these methods to infer retreat rates in catchments along the escarpment of the Western Ghats. They find that these millennial timescale retreat rates are in the same range as Myr-timescale retreat rates inferred from the age of rifting and distance of the escarpment from the coast, suggesting a near constant rate of retreat since rifting. Congruent with this observation, they also find that the morphology of escarpment rivers and the scaling between channel steepness and retreat rates are consistent with landscape evolution via constant retreat of a steep escarpment and minor subsequent evolution of the low steepness coastal plain.

I found the manuscript to be well written and illustrated, with a clear and well motivated introduction/problem statement and well conceived analyses to present the methodology and results of the Western Ghats case study. Quantification of non-vertical rates of landscape evolution - and the potential methodologies and proof of concept to implement them - should be of broad interest across the landscape evolution modeling community, and I think the authors have provided a useful contribution advancing towards this goal.

My comments are therefore relatively minor - though I wonder a bit about (1) the necessity to discuss the directional independence of cosmogenic nuclide production, since ultimately the authors simply convert vertical erosion rates to horizontal retreat rates (and this does not depend on any of the systematics of cosmogenic nuclide exposure dating nor complications presented by considering non-vertical production pathways…which, as you will see by my line comments, I think may confuse/over-complicate more than clarify) and (2) methodological details of the river profile analyses (particularly delineation of the escarpment vs. plateau channel reaches and omission of spatially variable rainfall) and retreat rate conversion methods (particularly how local scale products are taken and how erosion of local relief within coastal plain tributary valleys affects the horizontal retreat rate estimates - addressed but not until the end of the discussion section).

GENERAL COMMENTS

- I wonder if it is necessary to discuss in such detail the conversion of catchment-wide cosmogenic nuclide production for arbitrary direction of motion, since in practice you simply change the coordinate system after 10Be concentrations are converted to vertical erosion rates. In particular, I wonder to what extent e.g. effects of topographic shielding and attenuation length matter (also, could these not still be taken into account in the vertical erosion rate calculations, prior to horizontal conversion?)…or if the presentation and discussion should focus more on the technical details of this rate conversion - particularly the assumptions that go into it and its limitations (e.g., How do horizontal retreat rates vary with basin size, and are there scale limitations to these calculations? How do retreat rates from catchments whose headwaters drain portions of the plateau compare to those that do not, and how much uncertainty does this introduce to the escarpment retreat rate estimates?)

- While reading, I had many questions about remnant topography and how erosion of local relief within coastal plain tributary valleys affects the horizontal retreat rate estimates. The authors do not discuss this until line 649, though I feel earlier mention would clarify many points — so I suggest moving this section up, perhaps even into Section 2.3.

- For the Local Scale Product method, what sized elemental surfaces are used and how sensitive are the results to this? Also, how are the normal vectors determined (I imagine by making a slope measurement - over what scale and by what method…steepest descent?)

LINE COMMENTS

Throughout: I've noticed some inconsistency in the use/absence of oxford commas. Suggest standardizing.

Line 30: "their" ☐ "its"

Line 33: I find the sentence "Relatively static escarpments are supported by low denudation rates." to be a bit confusing and presume it should read something more like "This hypothesized evolution towards relatively static escarpments is supported by the observation of slow time-averaged denudation rates."

Line 38: "gynomorphically" ☐ "geomorphically"

Line 52: define DCN (Detrital cosmogenic nuclide?)

Line 53: delete comma. Perhaps it'd be helpful in this comparison of rates to use consistent units (e.g. change this and/or rates above to all be reported in m/Ma, km/Ma, or mm/ka)

Lines 56-57: "Given that the erosion associated with the relief of even the largest escarpments is under 2 kilometers…" confused me. Perhaps "Given that rocks are exhumed from depths of less than 2 kilometers due to retreat of even the largest escarpments, their cooling is likely.."

Line 75: "with a stationary water divide"

Line 82: Does "5 to 20 kms of extent normal to the margin" just mean a 5-20 km-wide coastal plain? Perhaps rephrase if so.

Line 124: I think "on top of the escarpment" could be clarified since I read "on top of the escarpment" to mean "on the plateau." Perhaps "have a headwater divide defined by the escarpment edge" (if I understand correctly?) or maybe " have a headwater divide on the escarpment" (if this is what you mean?)?

Lines 156-157: How sensitive are your results to the definition of this search area? I wonder in particular about the upper bound slope threshold, since there is not a clearly defined break in the data.

Line 159: From a slope-area plot or by collapsing the tributaries onto similar trends in chi-elevation space? Clarify. Guessing the former, but did you also compare your results to the latter?

Line 166: Perhaps the l in Sl should be subscripted to avoid confusion with e.g. channel length?

Line 173-174: Suggest clarifying "but not for individual rivers." Does this imply "gradually retreating rivers"?

Line 176: Pluralize "red square"

Line 180: Should this say "a concavity of 0.42"? Otherwise the main text and figure caption seem to disagree? Or I guess this is the local best-fit concavity rather than the global mean you use in other analyses? Perhaps this should be stated in the figure caption or text to clarify.

Line 196: "the response of temporal" ☐ "the result of temporal" or "they are not a response to temporal". Also suggest adding "they" after "i.e."

Lines 201-205: Here I assume you use a concavity of 0.45 (to give ksn in m^0.9) and not 0.42…? This should be clarified.

Line 203: "erosion resistant" ☐ "resistant to erosion"

Line 205: How do spatial variations in rainfall (i.e. I presume significant orographic rainfall gradient?) potentially affect river profile morphology? Have you tried incorporating rainfall into your chi-elevation analyses?

Line 208: Not sure what ESurf convention is but I recall a British "focussed" earlier but an American "focused" here. Suggest standardizing.

Lines 217: What is meant by "conventional"?

Line 220: Suggest changing "steady" which seems like an odd way to describe the concentration profile. Perhaps "time-invariant" or "…depth does not change over time"

Line 220-221: Suggest rewording "require only a weaker assumption" to perhaps "relax this assumption and require only that equilibrium…"

Line 241: Minor point, but perhaps this should just say cosmogenic nuclides instead of cosmogenic radionuclides since the theory could be applied to stable isotopes (e.g. Helium-3) as well. Perhaps change throughout.

Lines 242-245: This sentence is super long and seems unnecessarily redundant…suggest streamlining and shortening.

Line 247: Add "the" before "free path"

Lines 247-248: I think the sentence "A single exponential represents production of nuclide by spallation." needs to be clarified (or I guess I'm confused about whether or not this was already stated above at line 244? I see the need to clarify between production by spallation vs capture of muons for your next point, but I think the sentence should still refer back to eq 3).

Line 250-251: Suggest rephrasing “The penetration distance is in a general direction” which I presume just means attenuation of particle rays into a bedrock surface can occur in any direction?

Line 265: In the case of a radionuclide, shouldn’t eq 7 be C_0 = P_0/(lambda + rho*e/Lambda) where lowercase lambda is the decay constant? If this only applies to stable isotopes this should be stated more clearly here (as perhaps it is in line 277?)

Line 277: What is meant by the calculation of production rates? E.g. altitudinal and latitudinal variations? Perhaps you can be more specific here (as I see you are in the following sentences)…

Lines 288-289: Huh? I don’t understand why local shielding would sum to zero during integration…? This seems to contradict the results of DiBiase 2018, which I now see could be stated more clearly in the preceding sentence (the effect of increasing vertical attenuation length as a function of hillslope angle and skyline shielding offset the effect of decreasing surface production rate, such that both can effectively be neglected in the calculation of mean production rates for catchments with surface slopes < 30°)

Line 343: This is impingement upon the the slope/aspect of the surface exposure surface not significantly affecting the vertical attenuation length and/or topographic shielding no? As a (I’m not sure related or unrelated?) example: exposing the base of a cliff whose height exceeds the attenuation depth could expose deeper rock to cosmogenic nuclide accumulation that could not be analogously considered as a product of spatially variable surface lowering…right? Perhaps I’m missing something. This seems like an important consideration and how this plays in (or under what conditions this is applicable) should be stated more explicitly here I think (as is displayed by the theta<30 deg label in Fig 7 and discussed further later)

Lines 355-370: I appreciate the acknowledgement of the limitations here, but I still struggle to understand how production calculations can in practice be even quasi-independent of rock motion or the path of integration when one considers the effects of topographic shielding and attenuation length. Perhaps the finite range/variability of e.g. valley spacings or river network morphology limit this to some extent, but I can imagine radically different catchments that could project to the same horizontal (profile) area but different vertical (planform) area or vice versa…and this would seemingly complicate the coordinate transformation and application of equations 8-13 in potentially drastic ways.

Line 404-412: I’m a bit confused by these statement since any erosion that e.g. occurs within local depressions/ mid-elevation tributary valleys and/or that changes surface concavity but not relief may not be captured in this projection no? Perhaps this should be acknowledged (and/or the limited cases in which this can effectively represent Vrock should be stated) if I understand correctly?

Line 424: Delete comma after “catchment surface”

Line 447: Suggest clarifying “an example of the conversion of vertical basin-averaged erosion rates to horizontal escarpment retreat rates”

Line 484: I find it hard to assess the comparability of the results from the two methods in tabulated form. Could you also include a 1:1 plot of the retreat rates for the two methods plots against one another?

Line 519: Delete first “expected”

Line 546: Average catchment channel steepness? Or some more local steepness measurement? I see in the figure caption that channel steepness is calculated from regression of slope-area plots using a concavity of 0.42. I assume this is limited to the topography on the coastal plain, as shown by the red points in figure 4? I’d suggest clarifying these details in the text (e.g., “steepness” ☐ “basin-averaged channel steepness on the coastal plain”)

Line 560: Do the “short solid lines” refer to the error bars? I’d suggest calling them the latter if so to avoid confusion with the dashed line.

Line 562: Units of K should be meters^(1-0.84*n)/yr no?

Units of K = meters^(1-2m)/yr

m/n = 0.42

m = 0.42n

Line 574: “referential” ☐ “reference”

Line 581: Is is correct to say “is accounted for in the horizontal retreat calculation”? Perhaps I am missing something (?), but it’s not clear to me how this is actually accounted for in these calculations [or, if not, how it affects the inferred retreat rates differently than the flexural uplift of the coastal plain (besides the greater ability of the downstream coastal plain portions of the basins to respond to/erode any flexurally uplifted topography…and perhaps this is the point you are making?)]. In any case, I’d suggest clarifying since I’m otherwise confused about why uplift of the escarpment plateau could not also potentially contribute to the mass balance of the basin for basins whose headwaters do drain the plateau…?.

Line 610-625: I think it may aid understanding to change the terminology used to describe this calculation. For instance, I’d suggest changing “area” to “volume per unit width of the escarpment” to avoid confusion with planview/drainage area and perhaps also rename the “area ratio” to something more descriptive, perhaps “relative volume per width isostatic”. I think this terminology would be particularly useful to clarify the sentences at line 623-625, “Area ratio…” , which confused me (Perhaps instead: “Ratio showing the volume per unit width of the escarpment uplifted by flexural isostasy relative to the volume per width eroded by escarpment retreat for different effective elastic thicknesses. Higher ratios signify greater proportions of un-accounted for isostatic uplift and correspondingly greater overestimation of retreat rates.” Does the overestimation of retreat rates indeed scale linearly with the underestimation (omission) of relative isostatic volume per width uplifted (☐eroded)? (in other words, does neglecting 10% of the volume per width eroded by ignoring isostasy truly translate to a 10% error in the retreat rates, as seemingly implied by the wording now? I’d guess not…?)

Lines 642-648: Isn’t this also a problem in the Basin Projection Method, which will be insensitive to any local relief (below the scale of the total projected relief of the basin)? I guess the effect here may be of different sign, where neglecting this interior basin erosion via Basin Projection will tend to overestimate horizontal retreat rates rather than produce negative fluxes as in the Local Scalar method case. Perhaps I’m missing something (?), but if not, it seems incomplete to discuss the limitations solely of the Local Scalar method in this regard.

Line 649: Ah ok here’s the discussion of many of the points I’ve been raising regarding internal basin relief. Perhaps this section should be moved up to clarify these points sooner…? I'd suggest moving it up to Section 2.3 even.

Line 674: What does a "well-defined regional…form" mean? Suggest clarifying or deleting

Line 687: "over a thousand-year timescale" ☐ "over thousand-year timescales"

Line 689: delete comma

Line 692-694: "Average retreat rates are in the same range of magnitude as of the DCN 10Be-inferred retreat rates, although they are systematically higher,…" ☐ "Average retreat rates are in the same range, but systematically higher than DCN 10Be-inferred retreat rates, …" Also suggest changing "historical average" to "long-term" or "average since rifting"

Line 704: "historical" ☐ "long-term"

Line 705: "open" ☐ "unknown" (if I understand correctly?)

Line 715: "such as occur" ☐ "such as those that occur"