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Earth Surface Dynamics

Paris, May 21st 2021

Dear Editor and Associate Editor,

Please find enclosed the manuscript "*Topographic Disequilibrium, landscape dynamics and active tectonics: an example from the Bhutan Himalayas*" by Martine Simoes, Timothée Sassolas-Serrayet, Rodolphe Cattin, Romain Le Roux-Mallouf, Matthieu Ferry and Dowchu Drukpa, submitted to *Earth Surface Dynamics*. It has been slightly revised from the previous corrected version.

We received two reviews, by an anonymous reviewer (RC1) and by Wolfgang Schwanghart (RC2), which helped improve and clarify the presentation of our work. We're pleased that these reviewers appreciated our effort to carefully answer all their previous comments. We thank them for their additional suggestions when reviewing our corrections. We have addressed all these additional comments and provide our answers hereafter.

We hope that you'll find now our manuscript suitable for publication in *Earth Surface Dynamics*.

Sincerely,

Martine Simoes
(on behalf of all co-authors)

Comments by Anonymous Reviewer #1
and associated answers/corrections

Hereafter, all comments posted by Anonymous Referee #1 (RC1) are indicated in italic and preceded by "RC1", and are followed by the authors' response (preceded by =>).

<u>*RC1:*</u> *I have complete my review of the revised version of "Topographic disequilibrium, landscape dynamics and active tectonics: an example from the Bhutan Himalayas". Generally the manuscript is greatly improved and most of my large scale comments were suitably addressed from my initial review. I have a few minor thoughts for the authors, but nothing that should preclude the acceptance of this after a few tweaks (or maybe without a few tweaks depending on how the authors feel about my thoughts).*

L324: This is a pretty minor quibble, but I'm not sure how smoothing is significant for the calculation of chi? Ksn, sure, but as long as the smoothing doesn't change the drainage area *accumulation (which CRS shouldn't, since it operates exclusively on the extracted stream network), smoothing should have zero impact on the calculation of chi. I think you could just be vague here since you haven't introduced all of the metrics yet and just leave it at saying that smoothing is important for some of the metrics you'll use.*

=> We fully agree with reviewer 1 and thank him for this correction!

<u>*RC1:*</u> *L793-794: Though for the area-loss mechanism, would you necessarily expect the spatial consistency that you note? You end up essentially using this to favor a tectonic origin, but it seems reasonable to "kill" this idea here, i.e., the extent to which an area loss mechanism is actually consistent.*

=> The area-loss mechanism is a possible response of the river network - and its geomorphic consequence -, not the process driving this response. River captures - and area loss as their counterpart - may occur everywhere in the landscape, but it could be envisioned that these processes may be more prevalent in places where uplift is ongoing, as a geomorphic response to this active uplift. For instance, defeated hanging valleys may end up losing drainage area, locally, as a response to local uplift.
Given this, we do not have the impression that the spatial consistency between all geomorphic features is inconsistent with the area-loss mechanism. We think here that possible geomorphic responses and driving mechanisms should be clearly distinguished in our reasoning. This is clearly stated lines 842-844. No specific correction related to this comment has therefore been made in the manuscript.

<u>*RC1:*</u> *L895-890: I'm curious how much of an issue you actually expect this to be? You cite this multiple times and imply that it can be quite significant generally and for the Bhutan dataset specifically, but from the cited work, this seems the most problematic for very small basins (e.g., <10 km^2) that are eroding very slowly (with the added caveats of things like erodibility, diffusivity, etc. playing a role). For the data from Adams et al 2016, while some basins are eroding reasonably slow, most of the basins (both the slow and fast eroding ones) are within the size range where this previous analysis suggests that the E/U ratio should be quite close to 1. It's fair to point out that this could be an issue given the documented divide instability you provide here, but it's also important to be honest about how much of an effect you actually expect this to impart, i.e. is the level of distrust you indicate we should have toward this aspect of prior work warranted? At least a qualitative assessment of this should be quite doable since many of the same authors appear on the 2019 paper as this one. This has a little bit of overlap with my comments on the first go around, i.e., that some of the criticism here of prior work seems a little overly harsh. Your study easily stands on its own merits without casting unnecessary aspersions on others work. If you can demonstrate that the majority of basins in previous published datasets may be dramatically influenced by the changes in drainage area and systematically would change the result of what was argued previously, that's one thing, but I think you should either (1) attempt to do this or (2) tone down your criticisms of this aspect.*

=> Once more, our idea here is not to sound negative on this previous work that we sincerely appreciate, but rather to call for caution on the classical interpretation of denudation rates in contexts where the shape of sampled drainage basins is continuously changing by river captures, divide migration and therefore area loss/gain. To this respect we have rephrased lines 926-928. This caution applies to the interpretation by Adams et al 2016 on the timing of uplift from denudation rates, but also on the use of such denudation rates to constrain the geometry of the underlying MHT, such as in Leroux-Mallouf et al (2015) cited lines 971-973... a paper sharing many co-authors with the one being reviewed and discussed here! We honestly also apply our criticism to ourselves ;-)

A detailed and quantitative appreciation of whether or not these denudation rates are representative or not of uplift rates in Bhutan is out of the scope of this study (our manuscript is already quite long, as emphasized by RC2), and is presently the focus of a specific project in progress. We therefore hope to be able to provide in the near future a more quantitative analysis on this. Such field based analysis would be complementary from our previous modeling analysis. Indeed field is definitely more complex than the simple model in Sassolas-Serrayet et al (2019): natural variability in space and also possibly in time of parameters such as erodibility or climate, horizontal advection in addition to uplift in tectonically active areas such as mountain ranges, etc. Also our previous modeling analysis did not include basins with knickpoints. Because of this the variability and dispersion of denudation rates relative to uplift rates is most probably minimized in our previous numerical work when compared to nature. We also would like to point out that the dispersion and variability of denudation rates relative to uplift rates is also found numerically to be positively correlated with uplift rates, and is therefore expected to be more significant in tectonically uplifting regions (equation 6 of Sassolas-Serrayet et al 2019, see also Figure 9 of Hu et al 2021 in EPSL).

Comments by Wolfgang Schwanghart (Reviewer #2)
and associated answers/corrections

Hereafter, comments posted by Wolfgang Schwanghart (RC2) are reported in *italic* and preceded by "RC2", and are followed by the authors' response (preceded by =>).

<u>RC2</u>: *First of all, I like to apologize for the delay of my review. In my review of the revised version, I went through the reply to reviewer comments. In addition, I focused on rereading the parts of the text that were changed.*
I thank the authors for providing careful revisions in response to the comments by the reviewers. I think that Martine Simoes and coauthors have addressed all comments and made changes to the earlier draft of the manuscript where necessary. Overall, these edits have improved the manuscript considerably.
With 47 pages, the manuscript is still quite long. This is not to be taken as criticsm, but rather I'd like to encourage the authors to condense the text where possible. Wolfgang Schwanghart Minor comments

=> Most of the subsequent minor comments are suggestions of rephrasing. Unless mentioned and justified, these were all implemented in the revised manuscript.

<u>RC2</u>: *10: consider replacing 'most often' with 'commonly'*
<u>RC2</u>: *11: may however be rare in nature*
<u>RC2</u>: *13: these drainage dynamics*
<u>RC2</u>: *13f: 'particular case example' seems like three words meaning the same thing. Why not simply: Here, we document these drainage dynamics in the Bhutan Himalayas, where out-of-equilibrium morphologies have been noticed from major river knickpoints and high-altitude low-relief regions in the mountain hinterland.*

<u>RC2</u>: 19: these dynamics. Moreover, either landscape response is rapid, or time scale is short. Consider removing time scale
<u>RC2</u>: 28: remove "of uplifted terranes".

<u>RC2</u>: 320: In order to let others replicate your analysis, please provide tau (the quantile) and K (the smoothness parameter) used in the CRS function.

=> This is now indicated (lines 340-341)

<u>RC2</u>: 324: The computation of chi is actually not influenced by smoothing. Other geomorphometric attributes such as stream gradient or ksn, however, are.

=> This was already noted by reviewer 1. We fully agree with both reviewers, and thank them for this correction!

<u>RC2</u>: 326: Perhaps also note here the tolerance value applied in the knickpointfinder function.

=> This is now indicated (line 348).

<u>RC2</u>: 328: Is there a reference supporting the 3800 m cutoff?

=> This is justified earlier in the manuscript (with appropriate references), where the general characteristics of the morphology of Bhutan are reported (now lines 236-240).

<u>RC2</u>: 758: consider to replace "whatever" with "regardless" 765: see comment above
<u>RC2</u>: 857: Make sure to format references correctly
<u>RC2</u>: 860: remove somehow
<u>RC2</u>: 915: "is consistent" rather than "fits"