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Interactive comment on "Topographic disequilibrium, landscape dynamics and active tectonics: an example from the Bhutan Himalayas" *by* Martine Simoes et al.

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NB: Hereafter, comments posted by Wolfgang Schwanghart (RC2) are preceded by "RC2", and are followed by the authors' response (preceded by =>).

RC2: I enjoyed reading the manuscript by Simoes et al. It summarizes the controversies around the enigmatic high-elevation low-relief landscapes in Bhutan. Based on geomorphometric analysis of river profiles and drainage divides, the authors emphasize the role of divide migration in shaping the low-relief regions and conclude that existing denudation rates should be reevaluated given these dynamics. Overall, the manuscript is well written, although lengthy at times. Figures have a high quality, but

could be simplified and better annotated for better readability (see comment below). The number of figures seems adequate, but some of the plots appear in very similar form twice (for example Fig. 7a and the map in Fig. 8). This could be avoided. The methods are sound and described in a way that they are reproducible. In parts, the results are intermingled with interpretations which would be better placed in the discussion (e.g. 502-506).

=> We thank RC2 for his positive appreciation of our work and of our analyses, and thank him for providing interesting comments and suggestions that will help improve the manuscript. We agree that some of the figures could be better annotated for an easier reading, and that some may be merged together (ex: Figures 7 and 8 could be merged into 1 single figure, the same for Figures 10 and 11). In our subsequent careful revision of the manuscript, we will make our best to separate results and their interpretations, from discussion whenether appropriate.

RC2: As reviewer #1 notes, I also find it difficult to see how the results of this study cor- roborate or contradict the findings of the studies by Adams et al. Moreover, I find it difficult to follow why other concepts of tectonic rejuvenation (Duncan et al. 2003) are dismissed, based on the grounds that there is an absence of a coeherent wave of incision. Shouldn't it be expected that such a coherent wave is missing given that drainage divide mobility may be a process that prevails throughout this landscape?

=> In the case of how our results compare to those of Adams et al 2016, we suggest to see our detailed response to the various comments by reviewer #1 (AC1). Our morphometric analyses get to the conclusion that the peculiar morphologies in Bhutan are a response to active uplift in the mountain hinterland - a conclusion already reached by Adams et al (2016) from a different perspective. When compared to this earlier work, we additionally document the dynamics of this response, with river captures and migrating divide (ie instability of the river network), an observation that was not reached by previous authors and that allows for refining their initial ideas. The comparison to this previous study will be rephrased to make clear credit to their initial findings, and to clarify our input and step forward.

The idea that an upstream coherent wave of incision may not be straightforward to observe and extract in the case of divide mobility throughout the landscape is quite interesting, and should be considered indeed - we do thank RC2 for this interesting comment! We agree that pervasive area gain/loss by divide migration may alter transformed river profiles in such a way that it may be difficult to observe a potential wave of incision migrating upstream, as expected theoretically (Figure 4b). This has been somehow illustrated by Schwanghart and Scherler 2020 in the case of the Parachute Creek Basin (Co, USA), where the dispersion in the knickpoints related to the upstream migration of a wave of incision is interpreted to relate to coeval progressive changes in upstream drainage area related to divide migration. However, in the case of the Bhutan Himalayas where erodibility is much greater, the landscape appears to respond relatively fast to progressive changes in drainage conditions (section 5.2) so that progressive divide migration is not expected to alter profoundly transformed river profiles. This is not the case for captures and sudden large gains/losses of drainage area, which leave a greater imprint on transformed river profiles (ex: Giachetta and Willett 2018). Given this, we agree that these limits should be further mentioned (section 5.1), and that our conclusions on the absence of a coherent wave of incision from chi profiles should be further nuanced (ex: absence of clear evidence for such an upstream migrating wave of incision - rather than concluding that this wave of incision is absent).

It should be however noticed that transformed river profiles are much more diverse, and that major knickpoints are much more dispersed in chi, altitude AND amplitude, when compared to the Parachute Creek Basin (Schwanghart and Scherler 2000) or the Upper Blue Nile (Giachetta and Willett, 2018) examples, so that a coherent wave of incision migrating upstream into a relict landscape or uplifted terrane remains a weak potential mechanism. Following on this, the earlier interpretation of Duncan et al 2003 considers the large-scale uplift and rejuvenation of the whole mountain range in

Bhutan, and not only locally along the longitudinal band where we observe the morphologic dynamics described in our manuscript. As such the more local tectonic rejuvenation proposed by Adams et al 2016, with recent local uplift over a blind ramp/duplex in the Bhutan hinterland is a much more plausible interpretation. Therefore the absence of evidence for a coherent wave of incision migrating upstream (despite its limits) AND the spatial organization of the geomorphological dynamics documented here are the best arguments to dismiss the earlier interpretation by Duncan et al 2003. This will be better explained in the revised version of the manuscript.

RC2: The observation that catchments downstream of knickpoints are expanding is intriguing, but the mechanism that generates the expansion remains unclear. The studies by Struth et al. (2019) and Giachetta and Willett (2018) are referenced in this context, but these studies show examples where expansion happens downstream of areas with internal drainage and that were integrated in the flow network. Are endorheic basins a possible explanation for the preservation of these landscapes? And if not (which is quite likely given the humid climate), what could be an alternative intepretation? An hypothesis that might be brought forward could be the availability of sediments mobilized from the alluvial plains upstream that would act as tools accelerating incision downstream which would propagate towards the divides.

=> We agree that the studies we refer to (Struth et al 2019 in particular here, but also Giachetta and Willett 2018) both report captures of internal drainages. As mentioned in our manuscript (lines 750-752), because there is no clear evidence of drainage area loss in transformed river profiles, even in the case of potential large-scale captures such as for the Wang or Chamkhar Chhu, we propose that these captures may have been at the expense of the low-relief regions themselves if once isolated from the main river network - ie supposing that they may have been temporary internally draining hanging valleys, before capture. After this comment and to further document such potential large-scale captures, we have been exploring this idea following the above-cited studies, by calculating the theoretical transformed profiles of the possible proto-

Wang and -Chamkhar rivers in the case that the drainage area upstream of their major knickpoint has been captured. Such transformed profiles are broadly concordant to those of the large rivers that have supposedly equilibrated (ie Kuri, Puna Tsang) - further supporting our interpretation of large-scale captures. This will be added in the revision of the manuscript.

Such large-scale captures, possibly of internal basins, may be surprising in the case of a tropical climate, even though some internal drainages exist or may have existed in similar climatic and tectonic contexts, for instance in the region of the Sun Moon Lake reentrant of Central Taiwan (ex: Toushe Basin, or formerly Yuchi basin south of the Sun Moon Lake), with suspected similar captures of parts of this region in the past (Simoes et al 2014).

We agree with RC2 that additional drainage area by capture may enhance incision and base level lowering by adding discharge but also by remobilizing sediments (and therefore tools) from the captured upstream alluvial plains. This would certainly favor the incision of tributaries downstream of major knickpoints, and therefore the outward expansion of the downstream drainage area by divide migration. The tool effects of sediments drained out of the captured alluvial plains do however not leave a clear imprint on our transformed profiles (Figure S8, and lines 678-684) as expected after the work of Giachetta and Willett 2018, so this mechanism may not be dominant here. Additional comments on this will be added in the revised manuscript.

RC2: I find it difficult to read some of the figures. The combination of a grayscale depiction of topographic relief (which is quite printer-unfriendly), and colored networks makes some maps really busy and difficult to read. The colored stream networks (e.g. in Fig. 2c and 5) have variations in blue and green that are quite subtle or not resolved by my printer. Consider to label the river profiles in the plots rather then using a legend.

=> Topographic relief is commonly and classically depicted with gray-scale for an easier reading of other metrics (with colors) over relief maps. Therefore we'd rather keep

this gray-scaling to keep it simple. In the case of the colored networks on maps and of colored river profiles, an easier reading will be hopefully permitted by additionally labeling rivers on maps or on profiles, and/or by thickening some of the lines in profiles in the subsequent revised version of the figures

RC2: In addition to above major comments, I have numerous minor comments listed below:

=> Most of the subsequent minor comments are suggestions of rephrasing. Unless mentioned and justified, these will be easily implemented in the subsequent revision of the manuscript. We do thank RC2 for his suggestions and improvements.

RC2: 29: Remove "indeed". In general, the text contains numerous filler words, which could be avoided.

RC2: 35: Remove "first-order". I have seen this term a couple of times in this manuscript, but I don't know what it actually means in most contexts. For example, in line 63, I don't understand the term "first-order consistency".

=> "First-order" refers to the fact that there is a general broad consistency in the patterns, although some variations in the details. We will simplify whenever appropriate.

RC2: 253: the term "rather relative" is quite vague, as is the term "rather similar" in line 256.

RC2:394: remove 'long-distance'

=> We do agree, the term 'longitudinal profile' is here more appropriate, and more consistent with the terminology used throughout the text.

RC2:395: migrate upstream in response

RC2:395: what do you mean by 'common process'.

=> We mean a common mechanism, here a common change in forcing or boundary conditions. This will be rephrased and clarified.

RC2:396: perhaps rephrase "are expected to cluster in transformed coordinates".

RC2:411: , however,

RC2:419: Consider shortening this sentence: These complementary methods enable a more careful assessment of divide migration direction and drainage network reorganization.

RC2:424: Perhaps rephrase: Based on visual interpretation of longitudinal and chi profiles, we identify three profile types of major rivers in Bhutan.

RC2:425: Avoid the term 'simple'. Rather write that these profiles are concave upward with no remarkable knickpoint.

RC2:426: Remove 'rivers like'

RC2:433: 'intermediate characteristics' is a bit vague.

RC2:441: above 3800 m

RC2:456: Not sure what "better organized" means

=> We mean here that the various trends in river profiles are more visible and differentiated in transformed coordinates when compared to longitudinal profiles. This will be rephrased.

RC2: 459: Remove 'clearly' twice

RC2:462: Remove 'first-order'

RC2:465: You may better write "analyze the geometry of". The dynamics will be inferred from the geometry.

RC2:476: The sentence is vague: rivers compare well and are colinear to the very first-order. I am also not sure what you mean by 'first order' as used in the next sentence.

In addition, this part mixes observations (or results) and interpretation.

=> "First-order" is here used as "broadly", ie profiles are broadly concordant despite some secondary variations when getting into details. As of mixing observations and interpretations, this is quite minor as only sentence "Altogether, these observations suggest that these rivers share and have adjusted to the first order to similar tectonic and/or climatic forcing conditions, even though located throughout Bhutan" is an interpretation here, an interpretation that needs to be associated with results and not with discussion. So we'd rather keep it here or eventually move it to section 4.5 (Results: general conclusions) where we summarize our main findings and interpretations before getting into discussion, depending on whether repetition of our various observations and arguments could be avoided by doing so.

RC2: 479: On which basis do you judge that a knickpoint chi-value is discordant from an- other. Consider providing quantitative evidence. One possible way to report these differences in chi values could involve calculating the necessary change in area required so that the locations of knickpoints are the same in chi space. This would allow readers to appreciate the differences in knickpoint locations and would provide a way to eventually exclude or consider divide dynamics as potential mechanism that creates the differences in knickpoint locations.

=> We agree with the fact that the variability of natural conditions, with respect to theory, may lead to some secondary discordance in the details of chi profiles, even though theoretically concordant. This is illustrated in Figure 4b. Defining an acceptable degree of discordance in profiles is a solution to quantitatively define discordant from concordant profiles, but the definition of such a threshold is by essence totally arbitrary, whether this threshold is defined from the observed average dispersion of chi coordinates of knickpoints (as in Schwanghart and Scherler 2020), or from the definition of an acceptable calculated gain or loss of drainage area needed to have the profiles considered as concordant (as suggested here). As clearly visible in Figure 5b, all river profiles south of T3 are discordant, with variable positions of knickpoints, in terms of chi coordinates (dispersion over 1000 m) AND in terms of amplitudes or altitudes (from altitudes of 1200 m to 2700 m) - a situation quite different from that depicted by Schwanghart and Scherler 2020 for the Parachute Creek Basin (Co, USA), where knickpoints are dispersed over a same range of chi values, but clustered around an altitude of 2400 m. We will better explain this in the revised version of the manuscript. In our case study, the situation is therefore quite straightforward, and we'd rather keep it simple.

Following this comment, rather than calculating the drainage area gain/loss needed to have profiles more concordant, we calculated the theoretical transformed profiles of major rivers (Wang and Chamkhar Chhu) before a possible capture of the area upstream of their major knickpoint, and found that the profiles of these theoretical proto-rivers are clearly more concordant with those of the other large rivers (Kuri, Puna Tsang) over the region south of T3. This further supports our interpretations and we thank RC2 for giving us indirectly this idea. This will be added in the revised version of the manuscript.

RC2: 497: remove words like "clearly"

RC2:502: this paragraph should be better placed in the discussion

RC2:549: Avoid the term "dramatic" (which is found several times in the manuscript).

=> The term 'dramatic' has been used to refer to potential large-scale river captures along major rivers. It may in fact not be appropriate here and a clear reference to the spatial scale ('large-scale' instead of 'dramatic') is probably better.

RC2: 550: While expansion is the right term, I don't like the term contraction is this context, because it implies that there are processes that exert a stringent force. I would rather use "specific pattern of drainage area loss and expansion".

RC2:565: Better place this sentence in the discussion.

RC2:640: Such summaries are generally helpful. However, you may consider moving it to the beginning of the discussion, also.

=> We'd rather keep this summary in the results section, as it provides the basic interpretations that can be driven directly from our various observations. This summary section could in fact take up the text that has been previously suggested to be moved from results to discussion.

These interpretations are rather straightforward from observations, and should be separated from a discussion section devoted to discussing the limits of the approaches/interpretations, but also the implications of our results and interpretations in moving a step forward. In fact, we distinguish results/interpretations from discussion - and do not wish to mix direct interpretations with discussion. A solution could be also to modify the title of this section to "Summary of key results and their interpretations", or something alike.

RC2: 645: robustly? Robust in statistics usually means insensitive to outliers. I am not sure what it means here.

=> 'Robustly' is used in the sense that the comparison between trunk and tributary profiles is more rigorous than between various trunk channels that may not share the same outlet - and therefore interpretations less weak. 'Rigorously' may be a more appropriate alternative.

RC2: 659: remove "whatever the dimensions of their drainage basins"

RC2:691: replace "the classical" with "known"

RC2:692: replace "more generally speaking" with "in general"

RC2:699: rephrase to avoid "dramatic"

RC2:756: what are "stable soils"? There is no Fig. 3g.

=> We meant here well-developed soils, as expected in places where weathering is dominant over mechanical erosion. There is indeed no figure 3g, and it will be corrected to Figures 3b-d

RC2: 835: This assertion of an "absence of a coherent wave" needs better quantitative justifi- cation, as mentioned above. And given that divide dynamics are an important process, isn't that what you would expect irrespective of the absence or presence of a large- scale tectonic or climate signal?

=> As explained previously, the definition of a threshold to distinguish acceptable concordant profiles (within dispersion) from discordant profiles is arbitrary and we'd rather keep things more simple, in particular given the large and obvious dispersion in chi, amplitudes and altitudes of the knickpoints considered in our study case (see Figures 5b or 9c for instance).

As also answered earlier, we will nuance our conclusions on the absence of a coherent wave of incision (ie absence of evidence for a coherent wave of incision, with respect to what is theoretically expected) as we do agree that the captures observed throughout the studied landscape may weaken our related previous interpretations to some extent.

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