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Earth Surface Dynamics

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Dear Editor and Associate Editor,

Please find enclosed the revised 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*.

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 answered all their comments and posted our answers in the discussion appended to our manuscript (AC1 and AC2, respectively). Hereafter, we recall all their comments, recall and complement our answers, and indicate the subsequent associated revisions of our manuscript.

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> *In the submitted paper "Topographic disequilibrium, landscape dynamics, and active tectonics: an example from the Bhutan Himalayas", Simoes et al perform a topographic analysis of the Bhutanese Himalaya, with a special focus on the low-relief, high-elevation surfaces that have attracted prior attention in this portion of the range. They primarily approach this landscape from the perspective of evaluating drainage divide instability and the extent to which this either complicates prior results or helps to demonstrate what may be driving the landscape form in this region.*
Technically the paper is fine, the analyses are appropriate and the individual interpretations

that flow from these analyses are mostly warranted and/or logical.

=> We do sincerely thank RC1 for appreciating the quality of our analyses and of our work on the dynamics of the river network in the hinterland of the Bhutan Himalaya.

RC1: My main issue is that the motivation of the paper, and their addressing of this motivation in the discussion/conclusion, seems a bit problematic. They set up the paper by describing the landscape, its interesting morphology, and some of the prior tectonic and geomorphic interpretations. The problem is that they end up misrepresenting the interpretation from Adams et al, 2016 in the motivation and then after their analysis essentially confirm most of what Adams was arguing for, but still indicating that it wasn't what Adams was arguing for (e.g. they suggest that the Adams model was inconsistent with nearly static knickpoints, where in the Adams model explicitly argued for nearly static knickpoints). Similarly, they describe the Adams paper as arguing for the preservation of a "relict- landscape", where in detail the Adams paper explicitly argues against a relict landscape preservation hypothesis (some of this may be semantic, i.e. it seems like they are using an odd, non-standard definition of relict landscapes which differs from what is normally used, so this could be fixed by clarifying what they mean by specific terms).

=> We do regret the misunderstanding on how we decribe how Adams et al (2016) meet our final conclusions on the fact that active uplift is the most probable supporting mechanism for the observed peculiar morphologies, even though in the details some of their model inferences deviate from our observations. The work by Adams et al (2016) appears as the most advanced interpretation of these peculiar morphologies and we wish to give good credit to their work. By emphasizing the differences between their model and our observations, we also wish to point the way to move forward in the future in the modeling of the observed morphologies - and not to discredit their work.
We have accordingly substantially modified section 5.3.4 where we discuss previous interpretations in ligh of our findings. More specifically, the title has been modified to "Discussing previous interpretations in ligh of our findings" (line 848), instead of "Testing previous interpretations", which may sound like setting a controversy. Also, we took good care to indicate that Adams et al (2016) reached the same idea that uplift in the mountain hinterland was needed to support the observed morphologies, interestingly coming at it from a different angle (lines 873-878). We also indicated clearly the details of their model and inferences that do not fit our observations and deductions, and use this to propose a way to move forward (lines 879-894). These differences arise most probably because these authors set their conclusions by comparing the observed morphologies to a landscape evolution model where the geometry of the network is fixed. Future models will need to integrate the mobility and dynamics of the network as a possible landscape response.

Additionally, in the details:

- static versus migrating knickpoints: we disagree with RC1. Indeed, Adams et al (2016) suggest that major knickpoints migrate upstream. This is repeatedly stated in their manuscript that we have carefully re-read.
Some examples citing the text by Adams et al (2016), just by a simple search for the words "migrating knickpoint" throughout the text (pages refer to the PDF): "*Failure to match the rising local base- level set by the migrating knickpoints with a similar deposition rate would have led to a defeated, ponded river and an internally drained basin* " (p15); " *The stippled pattern marks the packages of sediment accumulating upstream of a migrating convex*

knickpoint (black dot) and forming the migrating concave knickpoint upstream (white dot) " (caption of Figure 6); " *Comparisons with our landscape evolution model and the observed sediment deposits both suggest that the low-relief landscapes of Bhutan were actively aggrading as they adjusted to the local baselevel rise created by a migrating convex knickpoint* " and " *I is the incision rate into bedrock at the position of the migrating convex knick-point*"(p17); " *Our landscape evolution experiment also supports the hypothesis that such low-relief landscapes are transient features whose positions are controlled by head- ward migrating, convex knickpoints, as evident from the dichotomy in erosion rates between the low-relief landscapes and adjacent canyons*. " (p. 23).
This is also illustrated in figures 6 and 11 of Adams et al (2016). Indeed in their figure 6, the knickpoint migrates from position ~90 km in b) to position ~110 km in c) while the model evolves in time; in their figure 11, the knickpoint migrates upstream from positions ~12 km (river 2) and ~15 km (river 1) to positions ~15 km (river 2) and ~18 km (river 1), respectively. This migration remains limited - even though the time that separates each of these model snapshots is not reported - , but goes together with the idea stated in the manuscript that knickpoints migrate ustream in the model.

- relict landscapes: we agree with RC1 that our initial terminology and phrasing may have been confusing and misinterpreted. By "relict landscapes", we referred here to the fact that former valleys of the mountain hinterland had been preserved (even though subsequently filled in-situ with sediments) and uplifted in Adams' model. This lead Adams et al (2016) to use the uplifted position of these alluvial valleys as a marker of uplift above a theoretical initial river profile. In fact, in their model, the overall shape of the valleys are remnants of former incisional valleys (explaining that we used the term "relict" for 'remnant', initially), and that only alluvial filling occurred in-situ during uplift (what Adams et al 2016 termed 'in situ formation of the valleys' - probably also confusing). We recognize that the term "relict landscape", classically used in the case of landscapes formed along mountain foothills and grading to the foreland, is not adapted here.
We corrected for this in the text when referring to Adams et al (2016) work (lines 269-270, 885-88). Also to avoid any confusion with the term 'relict landscape', we substantially modified section 5.3.2 on the characteristics and possible interpretations of the low-relief regions. We believe that our point is now much clearer.

RC1: There is certainly value in documenting some of the interesting and nuanced drainage network reorganizations that are occurring in this landscape, but the paper suffers from seeming to set up sort of a false controversy (and it is unfair to the Adams paper in that ultimately, most of the observations here confirm, or are consistent with, hypotheses put forward in the Adams paper). I think recasting the introduction / conclusion of the paper to be less about testing or addressing a controversy and more about exploring another interesting aspect of this landscape that wasn't really addressed in the prior work by Adams et al (various years), i.e. drainage network instability, and thinking about how this is being driven / influenced by the tectonic context seems much more appropriate. Ultimately, coming at it from this approach may allow for more interesting and meaningful interpretations and/or implications.

=> We regret that our work has been seen as setting any kind of controversy, as our objectives were not those. Indeed, we aimed at documenting and understanding the dynamics of the river network and the relative time scales for landscape response from the particular example of the Bhutan Himalaya where out-of-equilibrium morphologies have been documented. Our study also provides an interesting field example where the classical use of morphology to derive rates

of active tectonics is to be done with great caution.
When comparing our results to previous work and interpretations, we wish to give good credit to all previous work, and in particular to the work by Adams et al (2016) that proposed up to now the best model that fits most of our observations. Even though we agree with Adams et al 2016 on the idea of active uplift in the mountain hinterland, our results emphasize the limits of their model, and by doing so aims at pointing out future directions of work, in particular by proposing to better include the dynamics of the drainage network when modeling the lansdcape response to active tectonics.
We have modified the end of our introduction to clarify our objectives (lines 113-123), and, as indicated previously, the way we discuss previous work by Adams et al (2016) (lines 873-900).

<u>RC1:</u> L70-71: This statement at least does not reflect one of your cited references, i.e. Adams et al, 2016 argue for in-situ development of the low-relief surfaces from blind duplexing, which they argue may be structurally linked to the development of the Shillong plateau, but definitely is not representative of "relicts of former climatic or tectonic conditions".

=> As stated above, we agree that using the word "relict" in the case of the Adams' model may be confusing and should be avoided. We have corrected for this (lines 66-68).

<u>RC1:</u> L118: The Gilbert metrics are formally defined in Forte & Whipple, 2018, not in the Whipple et al, 2017 JGR-ES paper.

=> We do not fully agree with RC1. The idea of the Gilbert metrics was first proposed in Whipple et al 2017 JGR Earth Surface (see for instance section 5 of this 2017 manuscript "Topographic Metrics for Recognizing Mobile Divide", p 263-265 ; in addition to their section 7.2. "Utility of Topographic Metrics of Erosion and Divide Mobility", p 269-270) - even though these metrics were not initially termed "Gilbert metrics". These metrics were named as such, further expanded and discussed in the Forte and Whipple 2018 paper. We added this citation for Forte and Whipple (2018) wherever missing.

<u>RC1:</u> L145-144: You might also consider citing the recent Adams et al, 2020 (Adams, B.A., Whipple, K.X., Forte, A.M., Heimsath, A.M., Hodges, K.V., 2020. Cli- mate controls on erosion in tectonically active landscapes. Science Advances 6. https://doi.org/10.1126/sciadv.aaz3166) as their analysis of this region is also consistent with a relative invariant erodibility for much of the Bhutan region.

=> We thank RC1 for this suggestion, which has been integrated in our revision (line 144).

<u>RC1:</u>L281-282: "low-relief hanging fill valleys can be interpreted as relict landscapes formed locally", this seems like a very odd way to describe a landscape, that in the interpretation you're describing, is actively maintained by uplift of blind duplexes and the original authors describe as forming in-situ and explicitly reject the idea of these being "relict landscapes" in the traditional sense. I would consider rewording this to avoid confusion.

=> As stated and explained in detail above, we recognize that the term "relict" was confusing and not used following the classical meaning. This has been rephrased (lines 269-270).

RC1:*L293-294: See previous point, i.e. at least when considering the Adams model, they explicitly reject the idea of these being relict landscapes, at least in the way this term is typically used (e.g. the Whipple et al – Willett et al paper/comment and reply chain that you cite). I think you either need to reword this and other places or be much more explicit about how you are using/defining relict landscape, because this seems to be a non-standard way of describing them and it is (1) confusing and (2) misrepresents the results of previous work if you apply the more standard definition of relict landscapes.*

=> See previous answers above. Indeed, we agree that "relict landscape" was not meant in our manuscript in the classical way, but rather in the sense that alluvial valleys are interpreted as remnants (and not "relicts") of former incising valleys that were filled in-situ with sediments while uplifted. This has been corrected (lines 280-283), and section 5.3.2 has been modified to avoid further confusion.

RC1*: L406: Yes, but for a relatively short time, this is one of the key points of Whipple et al, 2017 (JGR-ES) paper.*

=> We kind of agree with RC1, as the (short or longer) time for return to an equilibrium profile depends on the response time of the river network to this perturbation. This is already further discussed and illustrated in section 5.2 based on this earlier work (Whipple et al 2017, but also Schwanghart and Scherler 2020) and on our observations. We modified to clarify that these are transient features (lines 395-396).

RC1*: L412: As earlier, Forte & Whipple, 2018 is the more appropriate reference here as this paper highlights the complications of base level choice.*

=> As mentioned previously, Forte and Whipple 2018 provide an expansion of some earlier ideas and conclusions reached in Whipple et al 2017 JGR ES. But we agree that this 2018 paper could also be cited here. See previous corrections.

RC1*:L663-665: It would be useful perhaps to consider this in the context of the aforementioned Adams et al, 2020 paper. I.e. they demonstrate that the large magnitude variations in precipitation rate have an important control on the scale of the topography (ksn) and its relation to erosion rates. The Gilbert metrics shouldn't be influenced by this, but you've calculated chi assuming static K and precipitation (as most do), but in the context of the Adams result, I wonder if calculating chi with the modern spatially variable precipitation would alter the chi patterns? My hunch would be no, and I don't necessarily think you need to demonstrate this, but I think it would be good to acknowledge that there are pretty significant precipitation gradients and they have been shown to influence topography and the reflection of erosion rates within topography.*

=> We thank RC1 for mentioning the Adams et al 2020 paper, with specific focus on how large precipitation variations impact topography and erosions rates, and taking the Bhutan Himalayas

as a field example.
We agree that Gilbert metrics, with across-divide contrasts in various morphometric parameters, are not to be affected by precipitation gradients as these metrics are local observations and are therefore expected to reflect similar background forcing conditions. When calculating transformed chi coordinates and river profiles, RC1 is right in that there is the underlying assumption of constant precipitation rates over the drainage basin (as drainage area is being taken as a proxy for river discharge) - an assumption not verified here, and in fact in most large-scale drainage basins. Locally higher precipitation rates may mistakenly lead to chi profiles resembling those related to drainage area gain (more water), and vice versa. In the case of our morphometric analysis of the Bhutan Himalayas, we do believe that this classical limitation of transformed coordinates does not, however, impact our results. Indeed, strong north-south precipitation variations are found similarly everywhere in Bhutan (See for instance Figure 1 of supplementary material of Grujic et al 2006 that clearly illustrates this). As large-scale rivers in Bhutan are flowing north-south, perpendicular to this climatic trend, they are all similarly affected: the cross-comparison of river profiles, as done in our study, is therefore permitted. In the case of secondary tributary streams, these are compared to their trunk stream locally at their confluence, and therefore most often encompassing similar local climatic conditions. Finally, extreme precipitation rates are found in Bhutan only along the mountain front (up to 50 km from the topographic front T1), ie south of the region of greatest interest of our study of the morphology of the mountain hinterland.
Rather than demonstrating this and not to lengthen the paper with unnecessary calculations, we added some clarifications to this in section 5.1.2 on the potential limits of our approach (lines 698-707).

<u>RC1</u>:L745-766: A fundamental problem with applying the Yang et al hypothesis and/or the Willett criteria for recognizing area loss/gain in chi-transformed river profiles to this landscape is the hypothesized presence of relatively discrete structural breaks (i.e. the blind duplex of Adams). This fundamentally violates some of the underlying assumptions in a pretty big way. More specifically, in chi-transformed space, a river profile responding to a growing duplex is going to look like a river having gained area. The key as you allude to elsewhere is the spatial consistency of the pattern, and thus probably not all of the area gain signatures are tectonic related, but some might be. You ultimately exercise caution in terms of applying the area loss feedback mechanism, which is warranted, but I think a more nuanced look at what you might expect in a structurally complex setting like this is important.

=> We agree with RC1 in that linear transformed river profiles (as those illustrated in Figure 4) are expected in the case of constant forcing and boundary conditions (uplift, climate, lithology...) throughout the river course. In the case of locally higher uplift, as expected over a blind ramp, the river steepness (and therefore the river slope in chi coordinates) is locally higher, so that the river profile moves "higher" in transformed chi plots. This was already stated and explained in our section 3 (Lines 374-375). Such pattern could indeed be mistakenly taken as reflecting river captures. However, to avoid this confusion in the analysis of chi profiles, it is important to define a reference equilibrium profile, and only river profiles that move above this equilibrium reference, whatever the slope and the pattern of this reference, should be considered as reflecting drainage area gain by captures. This is why we do not conclude that there are river captures only from the high steep chi profiles of some of the rivers, but by comparing these profiles to a reference local profile (now clarified lines 398-399). This reference profile is either that of the main trunk stream when analyzing the profiles of secondary tributary streams (ex: Figure 8, section 4.3), or that or large Himalayan rivers such as the Puna

Tsang or the Kuri Chhu over the same region when analyzing the profiles of the Wang and Chamkhar Chhu (ex: Figure 5, sections 4.1 and 4.2). In the case of uplift over a blind ramp - or in the case of any other structural complexity as found in tectonically active areas-, all profiles should be affected, and only conclusions relying on this above-mentioned cross-comparison are too be considered.
This is now also further clarified when discussing the previous interpretation by Baillie and Norbu (2004) (lines 856-872 in section 5.3.4), where we point out how lateral (random) variations in uplift can be discriminated from river captures.

RC1:L865-867: This is confusing, as Adams et al, 2016 explicitly argues for the knickpoints generated by the duplex to be fixed in longitudinal position (e.g. their figure 6 or figure 10), which seems consistent with your observations, but you cast it as though this is an observation that disagrees with the Adams et al, 2016 model?

=> See previous answer above. Even after re-reading carefully Adams et al 2016, we do not agree with RC1. In this earlier work, knickpoints are mentioned throughout the manuscript as migrating upstream, and this is further illustrated in the figures mentioned by RC1 as detailed previously.

RC1:L868: See prior comments about the confusing use of relict-landscapes.

=> See prior answer to these comments on relict landscapes.

RC1:L885-887: As noted previously, this seems to at least misrepresent the conclusions of some of the prior work.

=> As mentioned previously, this has been corrected and rephrased.

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 =>).

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 helped improve the manuscript.

We agree that some of the figures may be better annotated for an easier reading (see answer hereafter, in the case of a specific comment on this). Other figures were similar and appeared repetitive. This was the case for former Figures 7 and 8 - which are now reported as Figure 7 in main text and Figure S3 in supplementary material - and for former Figures 10 and 11 - which are now reported as Figure 9 in main text and Figure S6 in supplementary material.

In our careful revision of the manuscript, we attempted to clarify and separate results, interpretations and points of discussion. The various suggestions of interpretations to be moved to a "discussion" section, were however kept mostly in the "results" section to avoid repetitions that would inevitably lengthen the manuscript. In fact, these were direct and straightforward interpretations from results, whereas our 'discusion' section is devoted to discuss the limits of our approach and the implications of our results/interpretations to move a step forward. However, in order to simplify this, we re-wrote the various sections of our results, so as to first simply describe observations, and end each "results" 'section with the straightforward interpretations. This is specified line 466, done in sections 4.2, 4.3 and 4.4, and summarized in section 4.5. We hope this is now clearer.

<u>RC2</u>*: As reviewer #1 notes, I also find it difficult to see how the results of this study corroborate 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 and corrections to the various comments by reviewer #1. 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 has been rephrased to make clear credit to their initial findings (lines 873-878), and to clarify our input and step forward (lines 879-894).

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 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, and illustrated theoretically in Giachetta and Willett (2018) in the case of river captures. However, in the case of the Bhutan Himalayas where erodibility is significant, 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 - leaving the possibility to extract from the profiles of such streams the signal of a wave of incision if existant. This is not the case for captures and sudden large gains/losses of drainage area, which impact much more transformed river profiles. We added a discussion on these limitations (section 5.1.2, lines 680-690) and rephrased our conclusions on the absence of a coherent wave of incision from chi profiles (ex: absence of clear evidence for such an upstream migrating wave of incision - rather than concluding that this wave of incision is absent) (lines 629-630, 849).

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 is now better explained in the revised version of the manuscript (lines 849-851).

<u>RC2</u>: *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 770-772), 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 had been captured (Figure 5c). 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 (Lines 514-516).

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, which is internally drained, or the Yuchi and Puli basins where recent captures are suspected) (see discussion by Simoes et al, 2014). This has been added in our revision (lines 772-775).

We agree 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 691-697) as expected after the work of Giachetta and Willett 2018, so this mechanism may not be dominant here, even though we cannot discard it. This discussion has been added lines 743-750.

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 choose to 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 the additional labeling of rivers on maps and on profiles (Figures 1, 2c, 5 and S1).

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 were all implemented in the revised manuscript.

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".
RC2: 253: the term "rather relative" is quite vague, as is the term "rather similar" in line 256.
RC2:394: remove 'long-distance'
RC2:395: migrate upstream in response

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

=> We meant a common mechanism, here a common change in forcing or boundary conditions.

This has been rephrased and clarified (lines 384-385).

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 has been rephrased (lines 456-457).

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 (and elsewhere) used as "broadly", ie profiles are broadly concordant despite some secondary variations when getting into details. As of mixing observations and interpretations, we suggest to read our previous answer and corrections to a similar earlier comment.

RC2: 479: On which basis do you judge that a knickpoint chi-value is discordant from another. 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 quantitatively an acceptable degree of discordance in profiles is a solution to discriminate 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. In our case study, the situation is therefore quite straightforward, and we'd rather keep it simple. We have justified and further explained this in the revised version of the manuscript (lines 400-404).

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) prior to 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 WS (RC2) for giving us indirectly this idea. This has been added in the revised version of the manuscript (Figure 5c).

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. Corrected throughout the text.

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 straightforward interpretations that can be driven directly from our various observations and results These interpretations 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. We have modified the title of this section to "Summary of key results " to clarify this.

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' is a more appropriate alternative. Corrected

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

<u>RC2</u>:691: replace "the classical" with "known"
<u>RC2</u>:692: replace "more generally speaking" with "in general"
<u>RC2</u>:699: rephrase to avoid "dramatic"

<u>RC2</u>: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. Corrected
There is indeed no figure 3g, and it has been corrected to Figures 3b-d

<u>RC2</u>: 835: This assertion of an "absence of a coherent wave" needs better quantitative justification, 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?

=> See previous answers and corrections.
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 simple, in particular given the large and obvious dispersion in chi, amplitudes and altitudes of the knickpoints considered in our study case.
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 to some extent our related previous interpretations.