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

RC1: 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

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Discussion paper

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. 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 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. 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 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 will take good care to modify the text so as to make sure this is clearly stated in the revised version of the manuscript.

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 (examples are provided below), and is illustrated in their figures 6 and 11. Indeed in figure 6 of Adams et al 2016, the knickpoint migrates from position ∼90 km in b) to position ∼110 km in c) while the model evolves in time; in figure 11 of Adams et al 2016, 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), repectively. 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 their model.

Citing some examples of Adams et al 2016 for what concerns migrating vs. static knickpoints, just by searching 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).

- 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 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 will take care in our revision to correct for this, and will rather refer in the case of the Adams' model to remnants of formerly incising valleys, subsequently uplifted and filled with sediments.

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 (see our conclusion). We will take good care when revising the manuscript to clarify this so as to keep fair with this pioneering work.

RC1: 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.

RC1: 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 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, and we agree that this manuscript should be also cited here.

RC1: 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. Climate 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 will be integrated in the revised manuscript.

RC1: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 sentence will be rephrased.

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 will be rephrased in the revised version of the manuscript.

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.

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.

RC1:L663-665: It would be useful perhaps to consider this in the context of the aforemen- tioned 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 will add some clarification to this in section 5.1 on the potential limits of our approach.

RC1: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 (Initial text lines 385-386: "For U and K varying along the profile of the river, steeper (gentler) segments in chi profiles relate either to locally higher (lower) uplift rate or to lower (higher) erodibility."). 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. This reference profile is either that of the main trunk stream when analyzing the profiles of secondary tributary streams (ex: Figure 9, 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 can be further and additionnally clarified in the methodology section (section 3.2.4 and Figure 4) to avoid any misunderstanding.

RC1:L865-867: This is confusing, as Adams et al, 2016 explicitly argues for the knick-points 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 manuscript 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 responses to these comments.

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

=> As mentioned and proposed above, this will be easily rephrased to make better credit to previous work.

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