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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.

Anonymous Referee #1

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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. 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).

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.

Line-by-line comments:

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”.

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

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.

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.

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.

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

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

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.

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.

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?

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

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

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