Review of 'Geomorphic signatures of the transient fluvial response to tilting' by Beeson and McCoy

The paper under review is a revision of a previous manuscript looking at geomorphic response to tilting in fluvially dominated landscapes (I was not a reviewer of the original manuscript). I think this is an interesting study which sets out nicely different analytical and numerical models of tilting, and that the authors did a good job in the first revision of adding these scenarios and expanding the discussion of the 2D numerical modelling. I also think it is good to add in the second real landscape test site due to the ongoing controversy about Sierra Nevada uplift. However, I have some general concerns detailed below, specifically about whether the suggested signatures of tilting from χ plots are robust, followed by some more minor comments. The line numbers below refer to line numbers in the tracked changes document.

My main issue with this study is that of equifinality: many of the diagnostic features that the authors attribute to tilting could be the result of different processes, or simply in the choice of how you extract the channel networks and calculate χ. For example, in real landscapes the location of the channel head will have a big impact on the resulting χ plots. Performing χ transformation above the channel head will lead to non-linearity of the chi plot which may result in a very similar looking plot to the scenario of higher uplift at the outlet shown in Fig 1d (see Clubb et al. (2014), Fig 8). For the model runs this isn't a big deal, but if you chose an unrealistically low area threshold in a real landscape this could look the same as a spatially variable uplift field. This is not to say that this invalidates the study, but I think the authors could more carefully consider and elaborate on other reasons for non-linearity within χ plots. Other issues could be the influence of glaciation or mass-wasting processes, although I'm not aware of work at present which explores how these processes might influence χ plots.

A related, and more important problem, is that varying the concavity value used to calculate the χ plots can lead to very different curvatures: changing θ from 0.2 - 0.8 will completely change the curvature of the tributaries from convex to concave. I ran the chi analysis quickly on the 90 m SRTM data from the Middle Fork American River site in the Sierra Nevadas (Figure 1) which demonstrates this effect. The authors state in their discussion (Page 47, Lines 26 - 30) that changes in m or n might violate their assumption of uniform concavity. However, even if there is uniform concavity, if you have selected an incorrect m/n value for the basin, this could lead to an identical signature in the χ plot as that of a rigid block tilting scenario. I am concerned that the conclusions of the paper might lead other studies to assume that negative curvature in χ elevation space is always a result of tectonic processes, when in fact it could be a simple artefact of how χ was calculated.

There are a lot of different scenarios presented in the paper which were not related to tilting (e.g. change in elevation of the channel profile, drainage capture, uniform change in uplift rate). A lot of these results, and the discussion of knickpoint celerity, are not new to this paper and are similar to the work of other authors (Royden and Perron, 2013; Willett et al., 2014). I would suggest to either remove these analyses to focus on the novel points of this paper (the tilting scenarios), or to put these results more in context of the work that has already been done.

For the real landscapes, the authors state that they used slope-area plots to estimate θ. I found this odd, as the noise present in SA plots often makes the calculation of concavity challenging, and is one of the reasons that χ plots were initially developed. Why did the authors not use a χ based method of calculating concavity? I think the statement that 'Standard techniques for finding θ cannot be applied given the disequilibrium state of Sierra rivers' (Page 33, Line 8) is unsupported - there have been many techniques that have developed ways of estimating concavity in transient systems which are not cited in the study (e.g. Mudd et al., 2018; Hergarten et al., 2016).

Page 2, Line 10: I would suggest to also cite Morisawa (1962) here, as it reports slope versus drainage area prior to that of Flint and is often missed out in subsequent literature for some reason.

Page 3, Line 1: change to 'documented across many tectonic settings'

Page 3, Lines 18 - 22: were all these scenarios with the same tilt angle? It would be good to clarify this here.

Page 3, Line 30: very long sentence, split up.

Page 5, Line 10: typo in subheading - repetition of χ symbol and misspelling of 'transience'

Page 5, Line 21: Replace 'curved' with 'concave-up'

Page 5, Lines 24 - 26: This is only the case if the correct m/n ratio is chosen for the channel. I think it is worth mentioning this caveat.

Page 6, Line 25: more recent studies have since demonstrated that concavities outside 0.4 - 0.7 are common and that n is commonly greater than 1 (Lague, 2014; Harel et al., 2016).

Page 9, Line 7: remove word 'very'

Page 9, Lines 7 - 17: I found myself getting confused by all these scenarios. Would it be possible to put this in a table to make it clearer?

Page 10, Line 1: why equal to five? This could be explained more clearly.

Page 10, Line 5: repetition of 'in the'

Section 4.2: Which scenario was actually ran? Increase in K or decrease in U?

Figure 2: what timescale does the dashed blue line represent? Is this the same as the response time τ?

References

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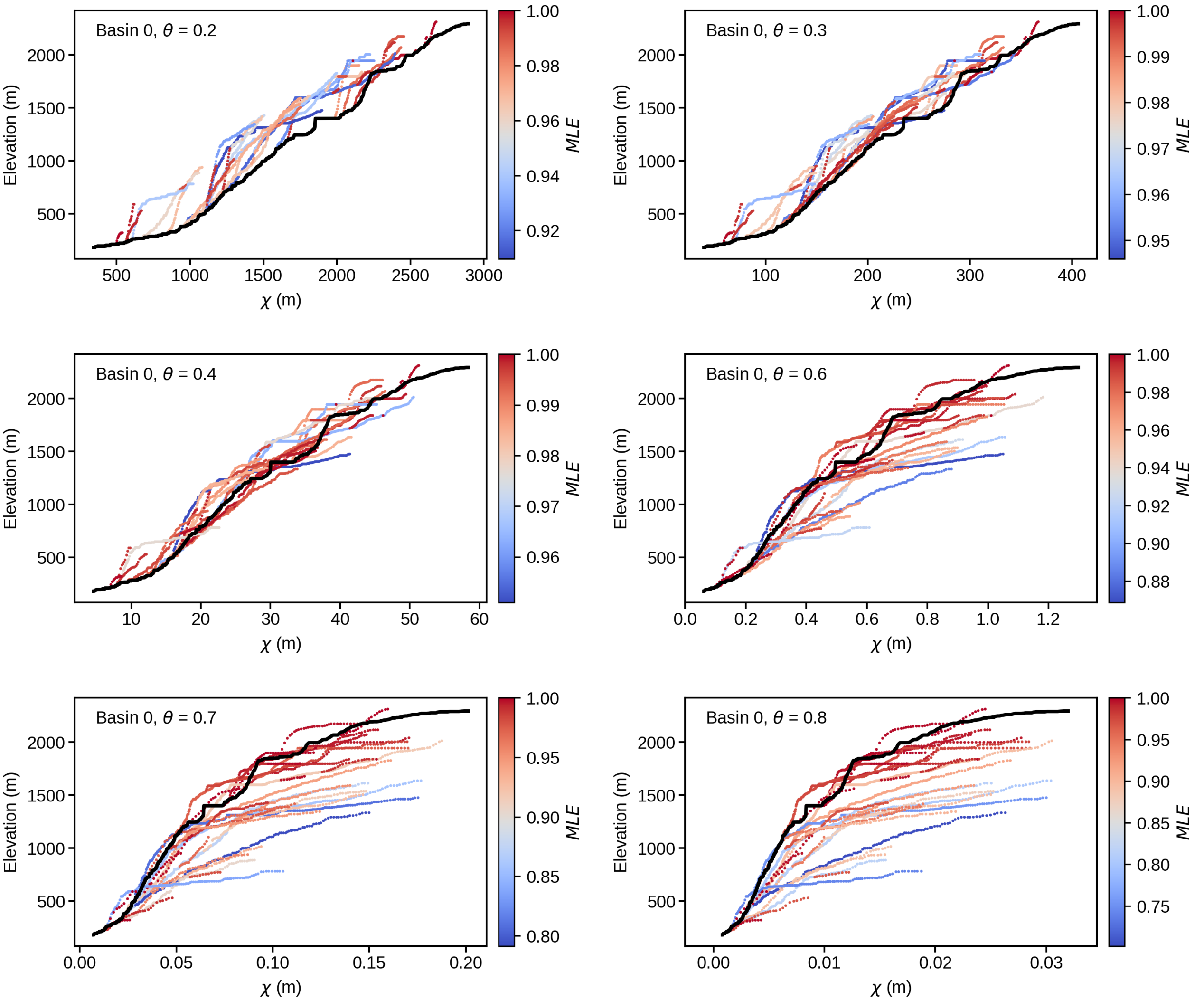

Figure 1: χ plots for the Middle Fork American River showing the change in curvature with the selected value of θ from 0.2 to 0.8. This may be incorrectly identified as a signature of tilting. MLE = most likely estimator, colours represent similarity of each tributary compared to the main stem.