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Interactive comment on "How concave are river channels?" *by* Simon M. Mudd et al.

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We thank reviewer 2 (Liran Goren) for a number of insightful comments that will help improve the paper. Reviewer 2 is entirely correct that we should have tested the disorder metric that has been used in several recent papers. We have followed this advice and we find that it performs similarly to the "all chi" method and in some cases is the most accurate method. It is also more computationally efficient than other methods. The only real drawback is that because it uses all data it cannot express the uncertainty in the concavity, so we are now in the process of designing an algorithm to estimate its uncertainty.

The manuscript presents and compares several techniques for extracting the concavity index of fluvial basins from topographic fluvial data. The manuscript nicely states how, for different (yet, specific) models of fluvial incision, the true, process-

dependent (or process-assumed), concavity index is a crucial parameter, without which, the steepness index and information about time and space dependent uplift rates cannot be reliably retrieved. The importance of the concavity index and the motivation behind the presented analyses are therefore convincing.
The manuscript is well written, and the effort that was invested in articulating the scope of the problem and the different techniques and analyses eases the reading of even complicated concepts.

Thanks. We are glad to hear that the manuscript is clear.

Overall, the manuscript compares between two classes of techniques for extracting the concavity index, slope-area analysis and chi-z analysis. Through several insightful numerical examples the superiority of the chi-z analysis is demonstrated in particular for spatially heterogeneous and transient landscapes. The manuscript then turns to explore the concavity index of natural landscapes, where the conclusions are, as expected, more ambiguous.

Natural landscapes are indeed a vexing problem since there is no way to know the "real" concavity or m/n ratio, but hopefully our contribution at least allows others to estimate concavity and m/n reproducibly.

I have one major concern: Given that the manuscript is methodological in nature, namely, it explores the accuracy and robustness of different techniques for evaluating the concavity index, it is lacking essential reasoning for developing a new technique without exploring existing ones or even just pointing out their possible theoretical limitations. Here, I specifically refer to the development of the maximum likelihood estimator for m/n from chi analysis (which is split into two techniques), without exploring existing techniques such as the 'tributary scatter reduction' (Goren et al., 2014) and a later version of this technique developed in Hergarten et al., *2016 (both papers are cited in the manuscript). These techniques find the m/n that minimizes the scatter in elevation over chi bins. They are intuitive, computationally simple, and the scatter itself can be used to evaluate the uncertainty. Developing a new technique that appears to be computationally more demanding without comparing and contrasting it to existing techniques does not serve the goals of the manuscript and of the community that can benefit from it.*

We agree, this was an oversight. We have now implemented the disorder metric and tested it against our simulated landscapes. It does quite well! Please see Figures 1 and 2 (at the end of this document).

These are not final figures as we are still developing an algorithm to estimate the uncertainty of this method. Because the disorder metric uses an ordered list of elevations, and the ordering affects the disorder metric, we cannot remove individual points as we did in the Monte Carlo approach in the discussion paper (we will call this the bootstrap method based on advice in this review). At the moment we are trying all combinations of tributaries to enter into the disorder metric. The main challenge is that we are using different methods to quantify uncertainty so this could mislead others. We can't see a common method across all metrics to quantify uncertainty so we are going to emphasise in the text that these uncertainty metrics are only qualitative.

On the same note, I would like to draw the authors attention to a pre-print https://eartharxiv.org/5u9eg/ (recently accepted for publication in JGR-ES) that, for a different geomorphic application, compares m/n values derived from slope-area and from chi-z using the tributary scatter reduction technique. I'm a co-author on this manuscript and I apologize for this far from elegant self-promotion, but it's very relevant to the current manuscript under discussion.

The paper is very interesting and we are happy to have it brought to our atten-

tion. The findings in that study are relevant to our work and will cite it in the paper.

Another, more minor, comment, is that currently, the manuscript is missing a discussion about which and under what conditions each of the two chi-based techniques for extracting m/n is better.

We now include such a discussion.

Page 3, line 4: Within the scope of the current manuscript the adjective 'constant' for m and n is a bit misleading.

Deleted the word "constant".

Page 6, line 9: 'The chi coordinate is simply a derived function of topography'. It's a function of the distribution of the drainage area, or the topology, and not of the topography.

This sentence no longer appears since we have reorganised how we introduce chi by integrating Flint's law, as requested by the first reviewer. But thanks for pointing this out because we probably would have said it in another paper.

Page 7, lines 15-17: The technique of minimizing z scatter over chi bins that was mentioned above does not have this issue.

Well, the disorder statistic will still have this issue because longer tributaries will diverge from the trunk channel more so will add more weight to the disorder statistic. But we now implement the disorder statistic for the paper and have a discussion of its relative strengths (see above).

Page 7, lines 22: Could it be that 'bootstrapping' is a more accurate description than 'Monte-Carlo'?

Yes, you are correct. We have changed the name.

Page 8, line 13: 'must'

Fixed.

Page 10, line 19: The geometry of the K patches should be described. From the fig, they appear to be square-shaped. Wouldn't it make more sense for the patches to be a function of the topography of even the drainage network itself?

We now say "These are rectangular in shape with K values that taper to the baseline K over ten pixels. We acknowledge this pattern is not very realistic but the aim is not to recreate real landscapes but rather to confuse the algorithms for quantifying concavity and test if they can still detect modelled concavity even if we violate some of the assumptions implicit in the concavity algorithms."

Page 12, line 3: 'reference concavities between 0.4 and 0.5 should give an accurate representation of the relative steepness'. Do you mean that in general or just for the Loess Plateau? If generally, then it calls for a justification. How does it relate to your natural basalt-sandstone experiment in Oregon?

Added phrase "in this area of the Loess Plateau" to make it clear we are only referring to this study site.

Page 12, lines 3-10: repeated text.

Fixed.

Page 13, line 2: A short discussion of how the lithology is expected to affect m/n is probably needed here. (Possibly via the relation between channel width and specific stream power/drainage area?)

We don't actually know this but we can put some speculation here. For example the different incision rules for plucking and fluting have different predicted slope exponents so we can attempt to connect that to concavity. As far as we are aware, however, nobody has attempted a systematic investigation of how lithology affects concavity. In fact, this is something we would like to do once we (and reviewers) are confident in our methods.

Page 13, lines 15-16: Could be worth mentioning that the Gulf of Evia overall represents a natural experiment where U varies both temporally and spatially.

We liked this description so have used almost exactly this phrase in the paper. Thanks.

Page 14, lines 15-18: How exactly does drainage area change affect the derived m/n? If all the tributaries are losing area, then they should all be plotted as convex in the chi-z domain. But the technique tries to minimize the residual and not to straighten the profiles. How is the residual affected by area change?

The methods do not account for drainage area change so we will state this in the text to avoid confusion and also mention how drainage area change may lead to convexities or concavities in the chi plots that will cloud interpretation of concavity.

Page 14, line 21: 'bust'

Fixed.

Page 15, lines 13-16: This appears to be a key sentence, but its relation to the results and discussion is not straightforward.

We will try to be more clear in the revision. What we are trying to say is that if you get concavities from many small basins you may be able to build up some sense of regional variations in concavity, and these might be linked (empirically) to things like climate, tectonics, and lithology. We are attempting to foreshadow what we intend to do with these algorithms in the future, and what we hope other workers might use our algorithms to do.

Fig 7: maybe it's worthwhile explaining what are the squared low relief patches in the variable K panels.

We will add this to the caption.

Fig 9: The captions of panel C are not clear. The two chi-based methods have different m/n maxs.

We will clarify this.

Fig 11: I assume that the dashed line represents faults. Maybe add a legend. Also, it might be worth differentiating (by color) between basins that drain across relay ramps and those that drain across faults.

Good advice. We will do this.

Fig 12: Same comment: differentiate between basins that drain across relay ramps and those that drain across faults.

Good advice. We will do this.

Fig 13: From my experience in chi-z analysis, such a scatter and concave tributaries are indicative that the chosen m/n is too high. Can you show the same basin with different m/n. This might hint that the scatter minimization technique and your new MLE technique give different results.

Our plotting functions as a matter of course print out the chi-elevation profiles of every basin for every m/n ratio (and also produce mpeg files showing how the chi-z plots change as m/n ratio increases) so it is fairly easy for users to check if the algorithm is producing results that raise alarm bells. Figure 3 shows other m/n ratios for this basin.

Wang 2017b probably deserves more credit for comparing the chi-z to slope-area predictions.

Yes, we agree. We will add more text highlighting this contribution.

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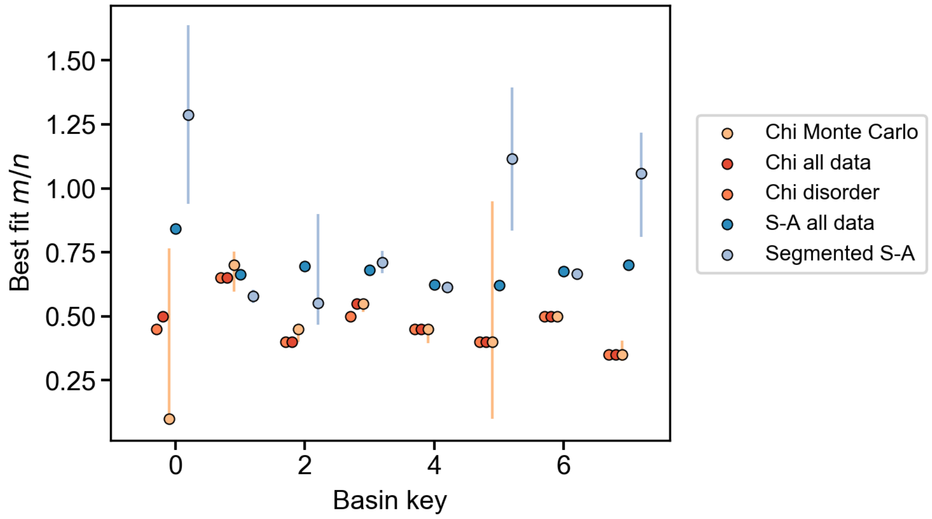

Fig. 1. Best fit m/n ratios for variable uplift scenario (from Figure 7 in the discussion paper).

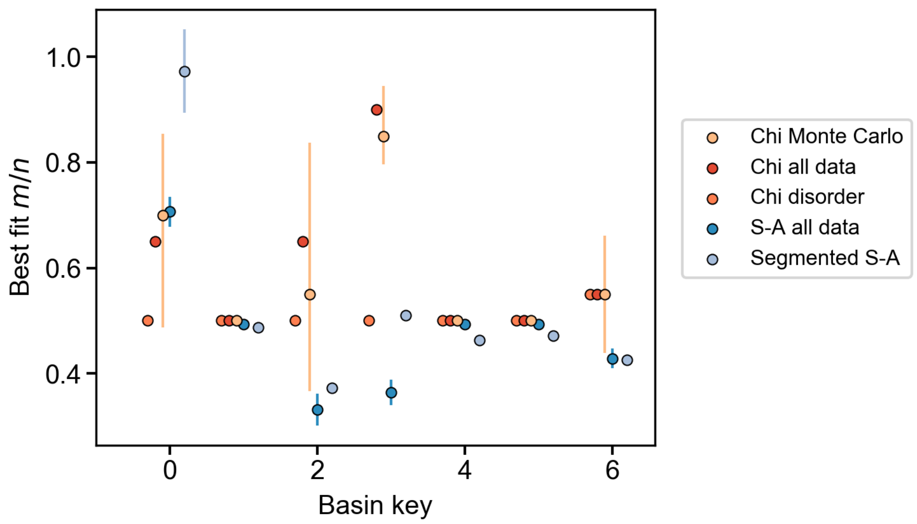

Fig. 2. Best fit m/n ratios for variable erodibility scenario (from Figure 7 in the discussion paper).

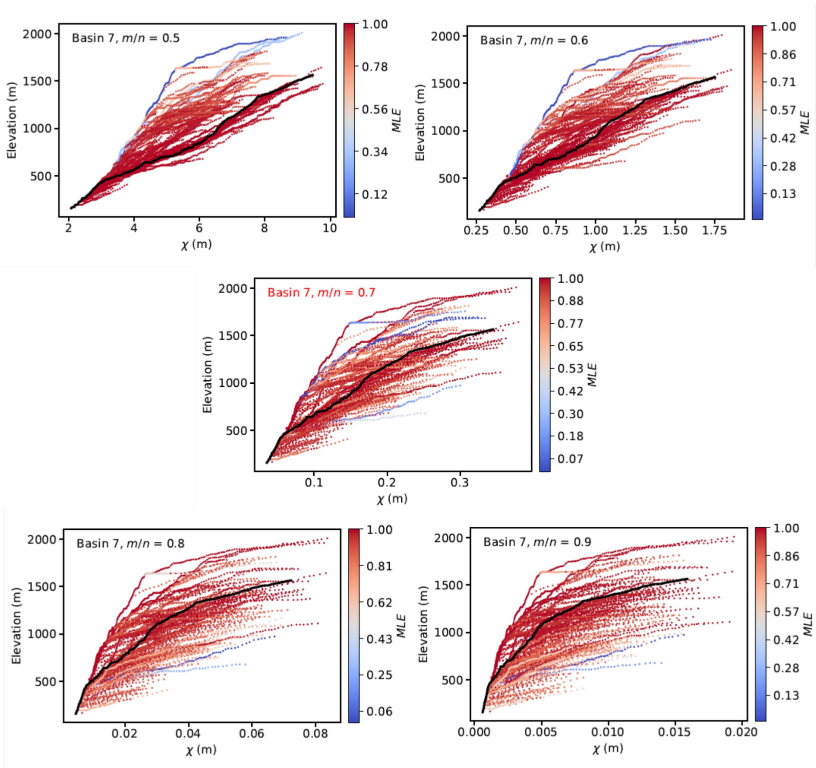

Fig. 3. Chi-elevation plots of Evia basin 7 (from Figure 13 in the discussion paper).