

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

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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.

Overall, the manuscript compares between two classes of techniques for extracting

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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.

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.

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.

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.

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Additional comments:

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

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.

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

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

Page 8, line 13: 'must'

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?

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?

Page 12, lines 3-10: repeated text.

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?)

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.

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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 χ - z domain. But the technique tries to minimize the residual and not to straighten the profiles. How is the residual affected by area change?

Page 14, line 21: 'bust'

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

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

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

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.

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

Fig 13: From my experience in χ - 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.

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

Interactive comment on Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2018-7>, 2018.

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