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

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This paper presents a new method for constraining the intrinsic concavity of river channels, in order to more accurately interpret spatiotemporal patterns of climate and tectonics from landscapes that deviate from the simpler case of steady state, uniform rock uplift, rock strength, and climate. The new metric compares the chi-elevation plots of tributary and mainstem channels in an objective manner, and is integrated into LSDTopoTools, an open source topographic analysis environment developed by the authors. This paper then evaluates the model as compared to existing approaches, using examples from real and synthetic landscapes.

Overall, this is a nicely-written paper with great figures and the code seems like a very useful addition to an arsenal of topographic analysis scripts that have evolved in recent years (e.g., LSDTopoTools and TopoToolbox). I think this paper fits well at ESurf, and I

only have one major issue that I think needs to be resolved before publication:

Major comment: On Page 4, Line 25, the authors recognize a strength of the existing slope-area method of determining channel concavity is that it "requires no assumptions whatsoever about the underlying form of the equations describing channel incision". Thus, I was surprised to find that the chi analysis underpinning the new method was (unnecessarily) framed in terms of the stream power model! Although the Perron and Royden 2012 paper also frames chi in terms of stream power, I would instead recast equations 7-9 in terms of the more general empirical relationship of Flint's law (equation 1), which makes no assumptions about process – ks and theta are simply geometrical properties of river channels. We did this in Whipple et al. 2017 Geology (doi:10.1130/G38490.1), but did not expand too much on the reasoning.

Note also that the relationship between channel steepness and erosion rate/uplift rate (Page 3, Line 21-29) is again not necessarily tied to stream power, but relates to an empirical relationship between relief and erosion rate (equation 1 of DiBiase and Whipple, 2011, doi:10.1029/2011JF002095; also discussed in Whipple and Meade 2006, doi:10.1016/j.epsl.2005.12.022). Connecting this exponent and the concavity index to m's and n's in stream power gets problematic because things vary depending on the specific form of the incision law (for example, adding a threshold changes the steepness-E relationship without changing m or n).

I think the paper would be stronger if, like the title says, the main analysis focuses on finding the intrinsic concavity index theta, rather than the model-dependent ratio m/n. Note that this of course does not preclude the comparison with stream power model landscapes shown in section 3 and interpretation/comparison with expected m/n!

——— Line comments: ———

Page 5, Line 16: I think only the profile is smoothed, rather than the full DEM.

Page 5, Line 23: Is method (i) using a single channel, the entire channel network?

Whole DEM?

Page 7, Line 1: This is just one new method, correct?

Page 7, Line 9-10: Not totally necessary, but might be helpful to emphasize the MLE = 1 for r = 0.

Page 7, Line 11: There seems to be a mistake in the math here where it was assumed that exp(ab) = exp(a)exp(b) rather than exp(a)ˆb.

Page 8, Line 5-9: Not just hanging tributaries, but any complexities influencing concavity that are not captured by simple stream power framework (e.g., spatial patterns in sediment cover/grain size). Perhaps it makes sense to include areas upstream of these hanging tributaries in the statistical analysis? Maybe collinearity is too stringent, and similar steepness is instead more useful?

Page 9, Line 34: “i) by regression of all χ-elevation data” Make clear whether this is just one channel or the whole tributary network at once

Page 12, Line 3-10: TypoâĂŤthis text is directly repeated from above.

Page 12, Line 32: Note that Duvall et al. (2004) argue that the high concavities in the Santa Ynez Mtns are due to strong rocks in the headwaters and weak rocks below, which is different than the "spatially varying m/n as a function of lithology" shown in Fig. 10.

Page 13, Line 19-20: I agree - but then why is it appropriate to use this for the numerical experiment on landscape transience, which also includes knickpoints?

Page 13, Line 23: I think more importantly, other processes become important in the transient! (e.g., DiBiase et al, 2015, doi:10.1130/B31113.1)

Page 14, Line 7: Spatial gradients in tectonics are far more important than temporal variations in disrupting interpretations of chi at divides. if spatially uniform U/K, then chi still good indicator of divide instability during temporally varying U (or K).

Page 14, Line 9-18: I don’t quite agree here. The fact that this is a relay system means that spatially variably uplift likely dominates, complicating a simple interpretation of chi across divides (see Whipple et al., 2017 JGR, doi:10.1002/2016JF003973)

Page 14, Line 19-22: “. . .river profiles. . .are not alone sufficient to interpret the history of landscape evolution, but must be considered alongside other observational data and in the context of a process-based understanding of landscape evolution . . .” I strongly agree!

Page 14, Line 21: Typo “bust”

Page 14, Line 32: Be careful tying the paper to stream power! (see main comment above)

Page 15, Line 4-6: I think would be good to point out that the second method does not handle well spatially variable rock uplift rate.

Figure 1: More detail is needed in caption to explain this sketch. Is it a single trunk channel? An entire stream network? There is also some good discussion of these challenges of interpreting concavity in Gasparini and Whipple, 2014 (doi:10.1130/L322.1).

Figure 2: Again, is this a single channel? Whole tributary network?

Figure 3: This caption could use more description. Hard to follow without careful reading of main text.

Figure 11: Do you mean “UTM Zone 34N”?

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