

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

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We thank reviewer 1 (Roman DiBiase) for his thorough review and for highlighting a different way of casting the paper that does not rely on stream power. We will still make some mention of stream power because it serves as the basis for numerical simulations, and also plays a role in the assumption of collinearity (see below), but we take the advice that introducing the concept of concavity can be done without this restrictive assumption. These comments have very much helped make the context of the paper more general, and we would like to thank the reviewer for these suggestions which we feel have substantially improved the paper.

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

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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:

Thank you for your supportive comments. As we describe below, we agree with the suggested revision (see below) and will carry it out in the revised manuscript.

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.

Yes, we agree this is a much better approach. We have modified the text accordingly and now take the approach outlined in the supplementary materials of the Whipple et al paper. The introduction now separates concavity from stream power. However we cannot escape stream power entirely. In chi space, if we only rely on concavity then the prediction is the chi profiles are linear. But any disturbance in

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erosion rates will lead to piecewise linear segments (Royden and Perron (2013) showed this very nicely). In the Mudd et al (2014) paper, we attempted to address this using a segmentation algorithm. However, this algorithm is sensitive to parameter values as well as being computationally expensive. By running many hundreds of landscapes through our code since then, we found that tests of collinearity appeared to perform better than assuming any linearity to segments along the profile. Perron and Royden (2013) suggested that if erosion signals propagated upward in elevation at the same rate, then the elevations at the same chi distance along the channel will be the same. In other words, the tributaries and trunk channel will be collinear. We therefore decided to run this test in idealised situations, where we could exactly constrain the concavity via m/n in the stream power incision model. The problem with the collinearity test is that it cannot be derived from Flint's law: it rests upon assumptions about how erosion signals will move through the channel network. Stream power is one theory that predicts this behaviour. Therefore, the collinearity test has to be connected to an incision law and not directly to Flint's law, meaning that we cannot divide our analysis from stream power completely. This is somewhat unsatisfying, as we are well aware of the many assumptions that go into the stream power model (e.g. Lague (2014)). We have completely rewritten the introduction to address this point and make clear the assumptions involved.

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).

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We will highlight the papers mentioned here and ensure that the connection between steepness and erosion rates are clear, regardless of any assumed incision law.

I think the paper would be stronger if, like the title says, the main analysis focuses on finding the intrinsic concavity index θ , 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 !

Duly noted. We will change the focus to the actual concavity rather than the m/n ratio so the title is now more indicative of the paper contents.

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

The Wobus paper actually recommends smoothing the DEM: it was written in the dark ages of DEM quality. However we will note that modern workers don't do this.

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

We will clarify this in the text: it uses all the tributaries and the main stem in a given basin.

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

We now call it two (there is the all points and what we were calling the 'monte carlo points' methods). Liran Goren suggested we call the second method a bootstrap method and we will use this terminology. We also introduce a disorder metric suggested by Liran and will devise a version of this to account for uncertainty.

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

Done

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 $\exp(a)\exp(b)$.

Thanks for spotting that. We inserted this mistake as a rhetorical device and it doesn't affect the results. We will expunge this equation from the manuscript.

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?

See discussion early in the response. We acknowledge the problems with SPIM and the fact that the collinearity method relies on assumptions about process. However a local linearity test requires some segmentation process (which is what some of the authors of this paper tried in 2014 and we find that method is extremely noisy and uncertain). We have tried to highlight the drawbacks of collinearity but we feel its advantages outweigh its disadvantages. We now say this in the conclusion, and explain why we feel this way.

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

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We will now say “For all but the final method the analyses use all tributaries in the basins.”

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

Fixed.

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.

We will modify the text so as not to mislead readers.

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?

In practice, workers generally fit small sections of the channel network with a concavity because the knickpoints distort the overall concavity. This is typically done using visual inspection. The tutorials and code associated with the Wobus et al. (2006) paper, for example, include functions to let users manually choose intervals over which to select concavity. One of our main goals is to ensure reproducibility, so we attempted to use a segment finding algorithm (mentioned in the paper). This sometimes works, and sometimes doesn't. So we find it very difficult to select appropriate segments for concavity using S-A data with reproducible techniques—this is true for both numerical simulations as well as in real landscapes. I suppose we are setting up a straw man for the numerical simulations, but this straw man situation is the one every geomorphologist finds themselves in when they are doing S-A analysis.

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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)

Indeed. We will add a sentence here highlighting this. Although, many of these other processes (i.e., debris flows) tend to reduce concavity and not increase it.

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).

We will note this in the revision.

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)

We introduced this example simply to show that things could get complicated so we have tried to avoid too much interpretation (since we are not really in a position to do so). However we will add a sentence to make the point highlighted by the reviewer. The other reviewer has also asked for some adjustments here so there will be some changes to this section.

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!

We are optimistic that a richer set of metrics will be used in the future as topographic and other data improves.

Page 14, Line 21: Typo “bust”

Fixed.

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

See discussion above. We completely agree with the reviewer about having metrics that are agnostic toward channel incision laws but the collinearity test derives from the SPIM so can't be described as purely geometric. We acknowledge this is a weakness of the test.

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.

We will rewrite this somewhat to point this out and also introduce the results from the new disorder test (which in some cases does better).

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).

We will clarify this in the caption. In the text around the reference to the figure we will refer to the very nice Gasparini and Whipple (2014) paper.

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

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Clarified. We now say “The data is taken from only the trunk channel.”

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

We will expand on the caption.

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

Yes we do. Thanks for finding that. It will be fixed.

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

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