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# *Interactive comment on* "Growing topography due to contrasting rock types in a tectonically dead landscape" *by* Daniel Peifer et al.

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The preprint presents some great field, laboratory, and computational work, interprets the results in a reasonable way and provides insightful conclusions. While my current opinion of it is excellent, the first impression was not. It took me a lot of re-reading to figure out that it makes sense. Let me explain in order to help make the final article more attractive for casual readers as well.

The claim in line 240 "denudation rates are negatively correlated with normalised channel steepness" is surprising when one looks at Equation (5b) which implies that fluvial erosion and steepness are positively correlated. Is denudation negatively correlated with fluvial erosion? Is there a mistake? Steepness is not a quantity that can be mea-



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Discussion paper

sured in nature, it is derived and requires a choice of concavity. Could a poor choice lead to this unexpected result?

In fact, everything is fine. The context and Figure 4B tell that catchment-averaged normalised channel steepness is being discussed. Figures 4E (the link between denudation rate and rock type) and 5C (the link between the catchment-averaged local relief and rock type) provide results that come directly from measurements and are easier to interpret:

- hard rocks denude more slowly;
- as a consequence, relief on them is higher.

Channel steepness on hard rocks is thus higher as well (for a reasonable concavity). Therefore, there is a negative correlation between the steepness and the denudation rate when speaking of catchment averages.

Assuming I'm a typical reader, a typical reader would understand this point with less effort if the results from figures 4E and 5C (which directly describe nature and are in agreement with one's expectations) were mentioned first and emphasised more.

I believe the units for K depend on the exponent $m$ (see equation 4) and are not fixed for a given concavity. In this case, the claim in line 219 that the unit follows from the reference concavity is not exactly right. The results referred to around the line 272 with $n = 2$ may be given with a wrong unit, assuming the reference concavity was the same. Conversely, different concavity indices could result in the same unit for K, so not every K with the same unit has the same meaning (in contrast with what line 317 implies). All of it has no consequence for the conclusions of the article.

There are also a few little things I'd like to mention. The terms 'steepness' and 'steepness index' seem to mean the same thing, similarly for 'concavity' and 'concavity index'. Consistent use of one version would eliminate any doubt. The DOI of Perne et al., 2017

appears to be wrong. In the caption of the Figure S2, the description of subplots (A, C, E) should refer to (A, B, C). Regarding the lines 341 and 342, referring to persistence seems unnecessary for the relief to be growing (relief growth is not associated with a particular timescale so no particular averaging period is necessary). The persistence implies that relief has on average been growing throughout the averaging time scale.

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