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Interactive comment on "Graphically interpreting how incision thresholds influence topographic and scaling properties of modeled landscapes" *by* Nikos Theodoratos and James W. Kirchner

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The paper under review expands upon previous work by the authors on performing dimensional analysis of landscape evolution models. The authors expand upon new techniques of interpreting curvature-steepness index space as a way of characterizing diffusive landscapes, similar to S-A in bedrock fluvial landscapes. They then re-define a Péclet number (competition between advection and diffusion) for landscape evolution models which take into account incision thresholds, as well as examining the influence of varying incision thresholds both between and within model domains. The paper is interesting and well-written. I found the point that, if an incision threshold is included in

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Discussion paper

the calculation of the Péclet number, the degree of landscape dissection is dependent on uplift rate very interesting and think perhaps more could be made of this in the paper. This could be a nice hypothesis to test in real landscapes where a relationship between drainage density and uplift rate has been observed.

I think the paper is suitable for publication in ESurf after a number of points (listed below) are addressed. My main issue is that more justification should be provided for the physical basis of representing the incision threshold as purely a function of area and slope, rather than as a minimum value of shear stress or stream power. It would also be good to better situate the paper in context of the wider literature, as well as clarifying the novelty of this paper compared to the authors' previous work.

Specific comments:

The introduction could better set out the novelty of the work that is being presented here. Lines 15-19 (page 2) mention the work of Theodoratos et al. (2018) and Theodoratos and Kirchner (2020), which introduce the concept of curvature-steepness space and dimensionally analyse a LEM with an incision threshold, respectively. This sounds very similar to the summary of the manuscript in the abstract, and therefore leaves the reader wondering what the novelty of this paper is compared to the previous ones.

Following on from this, it would be useful to expand the introduction with a more thorough literature review: many studies have already examined the influence of incision thresholds on erosion (e.g. Snyder et al., 2003; DiBiase and Whipple, 2011; Lague, 2014; Scherler et al., 2017; Venditti et al., 2019, etc...). This work would be better set into context with a more comprehensive review of previous studies.

Page 20, Line 30: The caveat of m=0.5 and n=1 is an important one considering that many studies have found that this is not likely to be the case in the majority of real landscapes (e.g. Lague, 2014; Harel et al. 2016). Although this caveat is mentioned here, it would be useful to expand on how changing m and n would affect the graphical interpretation of LEMs. Would it be at all possible to use curvature-steepness index space if n is not equal to 1? Or are these tools only useful if n=1?

Page 3, Line 1, "Because a negative incision rate would not be meaningful. . ." doesn't a negative incision rate represent deposition? In terms of real landscapes this is meaningful.

Equation (2) sets the incision threshold θ as merely a function of area and slope, such that no incision will occur at low values of (Aˆ0.5*S). What is this representing in terms of physical process? Previous approaches use incision thresholds to represent discharge variations, climatic controls on discharge, thresholds for particle motion/detachment, etc. The paper does explicitly mention this point (Page 3 Lines 5-14), and states that using this simplified formulation is more practical. However, in my opinion setting the incision threshold to be dynamic would add a lot to the paper and provide more physical basis for the parameterization.

Related to this, I found it difficult to see how the variation of the incision threshold metric (just as a function of area and slope) across the model domain would relate to the strength of incision thresholds in real landscapes. From Figure 7, it appears that this variation is just representing the distribution of area and slopes that you would expect in a landscape consisting of hillslopes and valleys. How is this related to the physical processes that would cause thresholds for incision in fluvial systems?

Page 4, Section 2.3 could be explained a bit more for readers not familiar with the previous paper. For example, how the dimensionless grouping of Kθ/U was obtained.

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