Earth Surface Dynamics Discussions

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Comment on esurf-2021-83

Anonymous Referee #1

Referee comment on "Investigation of stochastic-threshold incision models across a climatic and morphological gradient" by Clément Desormeaux et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-83-RC1, 2021

In this manuscript Desormeaux and others create and capitalize on a well-designed suite of data from fluvial landscapes in France (erosion rates, discharge data, precipitation data, variable topography, and geology), to test models of river incision. The authors test models spanning a wide range of complexity. Interestingly, they conclude that a relatively simple model of stream power that incorporates discharge matches observations better than those that incorporate other parameters thought to be more specific to processes of incision. This finding is quite interesting and will be a nice contribution to the vibrant and timely discussion regarding how rivers actually incise into bedrock.

The article is very well written and organized. The figures are good quality (though some of the labels are too small, these need to be reconsidered or some of them will need to be larger than they currently are in the pdf). The manuscript is good shape and is nearly publishable as is. However, I think there are a few things that could be included to increase the clarity and impact of the final paper. I outline these issues below. Well done! I look forward to seeing this in print soon.

Major(ish) comments:

1) Explaining why R-SPM is better than the other models.

As this is one of the main conclusions, I think there needs to be a bit more explanation for this result. Interestingly, this result is consistent with a few other well-cited and recent publications that use similar models:

Finlayson, David P., David R. Montgomery, and Bernard Hallet. "Spatial coincidence of

rapid inferred erosion with young metamorphic massifs in the Himalayas." Geology 30.3 (2002): 219-222.

Ferrier, Ken L., Kimberly L. Huppert, and J. Taylor Perron. "Climatic control of bedrock river incision." Nature 496.7444 (2013): 206-209.

Adams, B. A., et al. "Climate controls on erosion in tectonically active landscapes." *Science advances* 6.42 (2020): eaaz3166.

These publications suggest that erosion rates, fluvial relief, and mean annual precipitation can fit observations of natural landscapes through simple versions of the SPM similar to what the authors call R-SPM.

To understand why R-SPM might work better in this study, it would be good to know if there are linear or non-linear relationships between ksn and E as a function of precipitation or discharge. Figure 6B demonstrates that there is not a simple linear relationship between ksn and E for all data. As they state, this would not be expected for a landscape with variable climate. However, could the data represent an envelope of linear relationships set by changing R in equation 14? I would suggest that this is what the chi squared value in Fig 7B shows. It seems that the data are best fit with n ~ 1, constant coefficients and exponents, and spatially variable R. Unfortunately, Fig 6 does not contain any climate data from the sampled basins, and so this is difficult to assess this visually. I would argue that coloring the data by k, R, or MAP would be a more helpful way of understanding the point of the paper than by region. I would recommend such a figure be included. As it is, there is no way of comparing the modeled curves to the observed data (points).

A constant K where n~1 is an assumption of the Finlayson paper, a finding of the Ferrier paper, and similar to the Adams paper (though they find n ~ 2). Similar parameters work with the dataset here because of the nearly linear relationship between ksn and E with the variability in the relationship scaled by R. Many other studies have shown relationships between ksn and E can be nearly linear. Because of this, adding a non-linearity, like a threshold, is highly unlikely to improve any regression-based fits of the data (e.g. Fig 7C and 7E), even if R is variable. For example, the reason that DiBiase and Whipple (2011) were able to improve their model with a threshold term, is that their ksn – E relationship was non-linear (something like n > 1 for a pure regression). Similarly, incorporating D50 may not improve the model either, if D50 is a nearly linear function of ksn, which is linearly related to E. If this where the case, then adding an imperfect estimate of D50 might just add scatter to the data (i.e. Fig 7D, chi squared worse than R-SPM), or add nothing for a closer estimate (i.e. Fig 7F, chi squared same as R-SPM).

Again, these are not faults of the paper. I think the authors have done a robust analysis and done well to explain how and why they add complexity. However, I think taking into consideration the simplest interpretation from the outset and acknowledging the

correlation of variables helps to explain the outcome and ways of pushing these ideas forward. I would encourage the authors to include some discussion of these points in their manuscript.

2) Explaining the relationships between E, ksn, and MAP.

Are the author's sample erosion rates controlled by climate? I think the discussion starting on line 366 suggests they think they are not that they are not, but that the highest rates are coincident with steep topography, which is likely coincident with the highest rock uplift rates. I would tend to agree, but maybe that is not what they are saying. It seems that they have selected basins that are in topographic steady-state, which might mean that erosion rates are a reflection of rock uplift rates. If this is the case, then the significant influence of climate is setting the fluvial relief needed to erode at that rate. This would suggest that climate does not set the erosion rate unless climate changes over time and creates a transient signal. Whether they agree with me or not, I think there are several places throughout the manuscript that could benefit from some clarification on this issue. Another example starts on line 395. What do the authors mean by precipitation controlling denudation? Seems like this would require tectonic feedbacks. Or is the implication that these rivers are in a transient state? If they are in a transient state is this the best region to be testing these models? Are the modern topographies coupled to the calculated erosion rates? I think some clarifying points around these ideas would help future readers.