Interactive comment on ”Investigation of stochastic-threshold incision models across a climatic and morphological gradien” by Clement Desormeau et al.

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Dear Dr Clubb,

Here you will find our responses (as an attached supplement) to yours and reviewer’s comments on the manuscript entitled ” Investigation of stochastic-threshold incision models across a climatic and morphological gradient” (esurf-2021-83), on behalf of my self and co-authors.

We thank you and the reviewer for the positive and constructive comments on our manuscript. Please find below a summary of the main changes and as an attached supplement, our detailed responses (highlighted in blue).

- As requested, we have clarified the links between the climatic parameters observed, the denudation rates and morphometric parameters. For this purpose, we produced an additional figure that highlights aspects concerning the relationship between ksn and denudation rates with variation in runoff and discharge variability (in the supplementary materiel). The figure showing the relationship between runoff and denudation rates have been moved in the main text and we have added a panel concerning the relationship between runoff and relief with variation in denudation rates that highlights the orographic effect over the studied area.
- We have responded to the general points about divide migration, long-term representatives of discharge variability and D50 predictions by developing several paragraphs in the corresponding parts of main text, notably in the discussion.
- We have provided additional developments in the discussion on the topic of the relatively better performance of R-SPM when compared with complex models (ST and SVT-SPM) on the basis of reviewer #1 comments the suggested references.
- Finally, we have also discussed the possibilities of local deviations from steady state with respect to the assumption of the stream power model, and the implications for our dataset.

We provide an edited version of the article including all these modifications and we have complied with all the minor comments and suggestions relative to improving the figures (font and figures sizes, labels). We also produced a document highlighting all the difference between the initial manuscript submitted in October and the current version with all the majors and minors revisions (added highlighted in blue and removed crossed-out in red).

Best regards,

Clément Desormeaux

1 Editor : Fiona Clubb

Dear Clément et al.,

I'd like to thank you for submitting your manuscript to ESurf. I think the careful field data collection to calibrate the modelling is a real strength of your paper. I would encourage you to carefully read and respond to the reviewer comments, as well as my additional comments below, while preparing a revised manuscript. Note that there is a suggestion to incorporate additional literature: the addition of this literature is, in my opinion, well- reasoned and would improve the manuscript. However, it is up to your discretion whether or not you choose to include it.

Best wishes, Fiona

1.1 General points

- Migration and capture: Has there been evidence of divide migration or capture events in the Massif Central? This would affect the erosion rates, discharges and channel widths observed in the dataset. There's not necessarily additional analysis to do for this but it would be worth discussing in any case.

  Thank you for this comment. A key criteria for the selection of the basins included in our dataset was the absence of major knickpoints and a regular concave river profile. However, we also observe a contrast in denudation rates across the main divide and some local evidence for river capture and divide migration, in the Cévennes and Ardèche mountains, which can have an effect on some basins erosion rates and other parameters. There is most likely a long-term dynamics of progressive divide migration, but no large-scale capture events in the areas we selected for our analysis. We have added a paragraph discussing these topics in the first part of the discussion (middle of section 6.1).

- Discharge variability - Quaternary: Is the current discharge variability of the region representative of long-term variability over the Quaternary?

  Right, climatic parameters have certainly changed in absolute value, in tune with glacial/interglacial cycles. However, as the topographic margin is a long-standing feature controlling the orographic patterns, we expect that the relative contrast between the margin and the interior of the Massif, which is the 1st order signal here, is present over that time scale. A paragraph has been added to the presentation of the hydro-climatic data to highlight this point and some additional literature has also been added in the same part (section 4.1.2).

- D50 - predicting: It's interesting in the denudation modelling that the free scaling parameters ends up predicting a lower D50 compared to the field measurements. I wondered whether this reflects the fact that the grain sizes are surface measurements through bedload counts, and therefore might reflect armouring of the bed and not be representative of the true D50.

  This is an interesting point. In fact, all the field measurements come from surface bedload counts. It can be a limitation of the data, and we now mention this possibility in the discussion (middle of section 6.2). However, Fig. 10 shows that the results of the models with a free $D_{50} - k_{sn}$ scaling fall into a transitional erosion regime and very short recurrence times which do not correspond to the reality of the hydrological regime in our study area.

- Denudation rates - runoff relationship: The fact that denudation rate increases so clearly with basin runoff and yet does not correlate well with ksn is an interesting result, and I think could be made more of in the paper. I would suggest moving the denudation rate-runoff relationship in the supplement to the main text to highlight your discussion that in this landscape (non-tectonically active), denudation rate is more clearly controlled by precipitation than by slope.

  The figure presenting the denudation rates runoff relationship has been moved in the main text and we have also added a panel dedicated to the relationship between runoff and relief that allows to better understanding the orographic effect (see below). In line with similar comments from reviewer #1, we have expanded the discussion of this point and the importance of precipitation/runoff variation on denudation rates in non tectonically active landscape (middle of section 6.1).

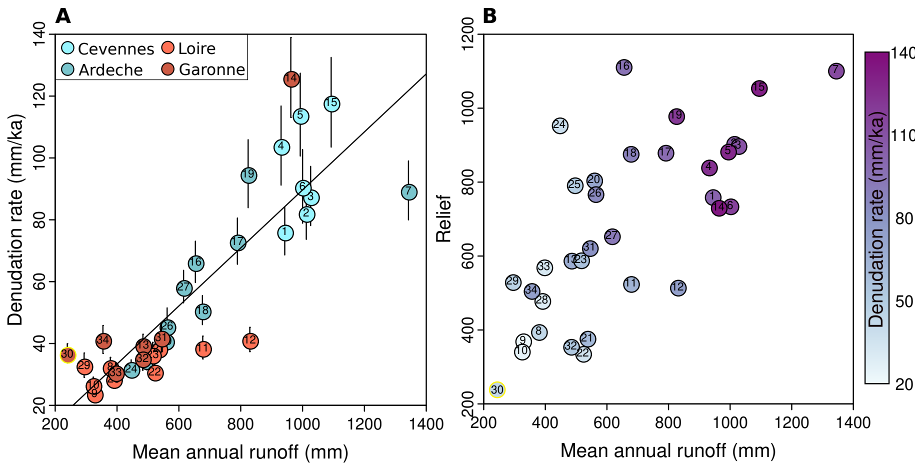

Figure 1: (A) Denudation rates against basins mean annual runoff calculated using the calibration of Fig 4. Symbols are colored according to the 4 different subregions and black solid line is a linear fit to the data. (B) Basins relief against mean annual runoff. Symbols are colored according to denudation rates of the basins.

1.2 Specific points

- Power law discharge variability: When calculating discharge distributions, how was the high discharge part of the distribution determined to fit the power law? I think a figure illustrating the method here would be helpful.

  We have added another sentence in the method concerning the choice of the definition of the lower bound for fitting the power law behavior. We have also modified the corresponding figure in the main text to indicate that limit for each of the plotted stations (subsection 4.1.1).

- Figures points : Can you annotate the plots (Figure 3) with 'Most variable' to 'Least variable' to make it clearer? Many figures have labels and font sizes that are too small e.g. Figure 1, 5, 6, 8. Please gothrough all figures and check the font and figure sizes for readability.

  Most and least variable have been added on figure 3. Figures 1, 5, 6, 8 have been expanded.

2 Reviewer #1

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.

2.1 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.

We have expanded our discussion following the points raised by the reviewer and made reference to the corresponding publications (end of section 6.1 and middle of section 6.2). This is indeed a puzzling and surprising results or our result as, due to the strong gradient in hydrological variability across the margin, we had initially expected a more pronounced influence of such process, at least relatively between the East and West part of the studied area. We note the importance of the inclusion of spatial variations in runoff (A-SPM vs R-SPM) in order to explain the distribution of denudation rates was also a key finding of Campforts et al. (2020) study in the Ecuadorian Andes.

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 $\sim$ 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).

We have included a new figure for the comparison of denudation rate and channel steepness with two panels in the supplementary material, as suggested by the reviewer. We do not observe any clear trend that could be associated with range of similar values in either runoff of discharge variability. This results again highlight the complexity of our dataset, where almost all parameters (runoff variability, lithology, grain size) present significant spatial variability, and the main structure dominating the dataset appears to be the strong EW gradient across the margin for most processes and associated parameters. These observations and ideas have been added to discussion, in particular to emphasize the point that spatially-averaged values at wavelengths $\sim$100 km or ad hoc choices for these parameters, which are often used in regional-scale discussions of denudation rates, are likely to blur important first-order spatial patterns (end of section 6.1).

A constant K where n$\sim$1 is an assumption of the Finlayson paper, a finding of the Ferrier paper, and similar to the Adams paper (though they find n $\sim$ 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).

This is a very interesting point. The critical assessment of the added value of more complex model frameworks was already an idea we tried to put forward in the previous version of the discussion. We have expanded the discussion in several points in that direction to highlight the contrasts with the reference study of DiBiase and Whipple (2011), and notably, as suggested by the reviewer, the impact of additional complexity associated with accounting for spatial variations in grain size (sections 6.1 and 6.2). The reviewer is right to question the added value of injecting additional data in this case, but we think that the test of the D_{50} scaling has two merits : (1) to observe that even if the SVT-SPM (observed scaling) is not performing better than the R-SPM, it is much better than the ST-SPM and raises the importance of spatial variations for the threshold and (2) to highlight the very high impact of using free parameters to set the incision threshold.

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.

As described in our responss above, the discussion has been expanded in several places to build on the ideas proposed by the reviewer.

2.2 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.

As noted above, in our response to the editor's comment, we carefully selected our basins to avoid large-scale manifestations of transience such as major knickpoints or capture of large headwater areas. We also discuss in more details some of the steady-state assumptions and associated implications when comparing our data with model predictions. Due to the strong EW gradient for most parameters in our dataset, co-varying with the major morphological changes across the margin, it is difficult to explicitly define the causal link, in particular with respect to the influence of precipitation. As we discuss above, on of the clearest result of our study is the marked performance improvement of the R-SPM when compared to A-SPM. As the R-SPM incorporates both the influence of fluvial relief/gradients and spatially variable climate (vs only the former for A-SPM), it supports the influence of precipitation as a strong modulator of incision efficiency, as suggested for example by Adams et al. (2020). The discussion has been edited at the two places highlighted by the reviewer, to clarify the type of interactions between denudation and precipitation (section 6.1).

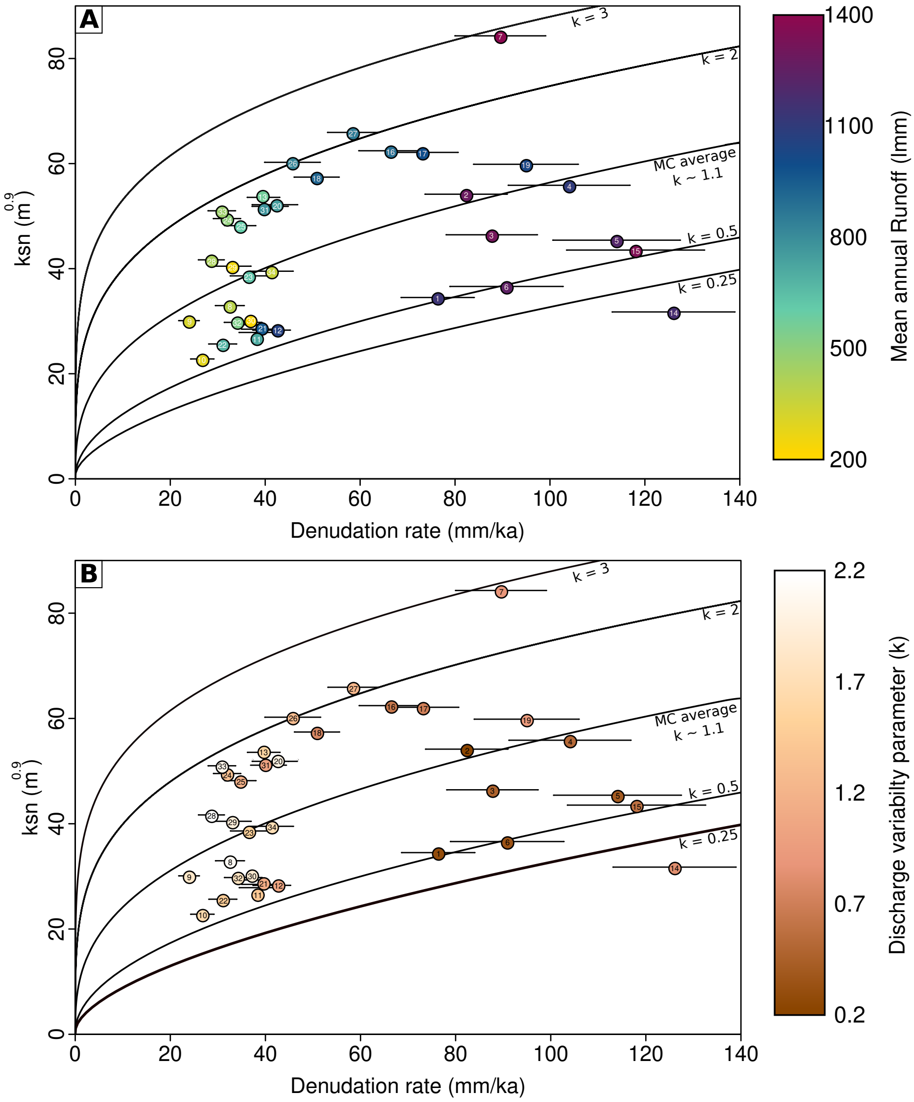

Figure 2: Comparison between normalized channel steepness index and denudation rates. (A) Symbols are colored according to mean annual basins runoff calculated from linear relationship between MAP and $\bar{R}$ (Fig. 4). (B) Symbols are colored according to basins discharge variability value calculated from thin plate spline surface (Fig. 1B).