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Interactive comment on "A nondimensional framework for exploring the relief structure of landscapes" by S. W. D. Grieve et al.

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In this document, reviewers comments are presented in **bold type** and our responses are in standard type.

Grieve and others present a method and software for linking erosion and the relief structure of landscapes. The manuscript is clearly written and the sensitivity analyses provide a useful guide to aid in interpreting the results of their method. Hence, I think this manuscript will be a useful and well-received contribution; the authors have produced a software that will benefit the community. Thank you.

We would like to thank the reviewer for their detailed consideration of our manuscript. We are pleased that the reviewer sees merit in the software we have produced, and agree that our sensitivity analyses should help the community to better interpret E^*R^* measurements. Following the recommendations of this review we have modified Figure 5 to correct an error in how we labeled the subplots and have added a new figure to aid with the interpretation of the Coweeta results. We have expanded our discussion in several places to allow for possible alternative explanations of our results and have also implemented all of the minor edits suggested. We believe that these changes have significantly improved the quality and clarity of our manuscript. We respond to each point made by the reviewer individually below.

One comment regards the interpretation of data that do not fall on the Roering et al. (2007) curve. In the case of the Oregon Coast Range this could be either because 1) the landscape is in a steady state and the parameterization requires adjustment or 2) the landscape is not in a steady-state and the parameterization is correct. I suspect it is quite difficult to clearly distinguish among these two possibilities, and that both should be discussed in the manuscript.

The reviewer is correct that it is challenging to distinguish between these possibilities purely from topographic data. However, there is a considerable amount of evidence for the Oregon Coast Range being in steady state (e.g., Reneau and Dietrich, 1991; Roering et al., 2007) and the selection of a lower average S_c value is supported by earlier work to constrain the critical gradient (Grieve et al., 2016). We have added a sentence to the discussion of the Oregon Coast Range results to highlight this possible alternative interpretation: *'This can be interpreted as evidence for topographic decay, however due to the preponderance of evidence supporting a steady state hypothesis for this landscape (e.g., Reneau and Dietrich, 1991; Roering et al., 2007), it is also possible that a critical gradient of* 1.2 *is too large in this location.'*

In responding to reviewer 1 we have also addressed these points more broadly, by expanding on our theory section and explicitly stating our definition of steady state. The challenges of parameterizing the model and fitting the critical gradient are now discussed in more detail in the manuscript, with the aim of drawing the reader's attention to the potential limitations inherent within this method.

Similarly, for the Coweeta site, there should be some justification for a substitution of the Sc value from the Oregon Coast Range for the value calculated from the framework,

Our selection of this value is based on the similarities in landscape morphology, sediment transport processes and the range of E^* values observed at the two sites. The aim of this selection of a critical gradient is not to suggest that this is the correct value for Coweeta, but rather that a larger critical slope than the value 0.57 of reported in Grieve et al. (2016) produces data consistent with topographic decay. We have rewritten this section to better reflect that we do not claim that 0.79 is the correct value, rather that it is within a more plausible range than the value of 0.57.

and more explanation of how the field observations indicate alluviation is reducing the critical gradient in this landscape.

When we referred to the mean gradient reducing we should have instead referred to the mean relief reducing, which has the consequence of reducing the gradient. In locations where valley alluviation is occurring, the base of the hillslope is raised vertically with regard to the elevation of the ridgeline. This reduces the relief whilst not changing the morphology of the hillslope, due to the difference in timescales of hillslope and channel response (Hurst et al., 2012). This can result in a reduction in R^* values across a landscape, leading to a lower S_c value being generated for a landscape than would be predicted were the alluviation not taking place. We have clarified this position and added a more detailed explanation of this mechanism: *'As a valley fills with sediment, the hillslope relief will be reduced more rapidly than other hillslope properties, due to the difference between rates of* *hillslope and channel response to forcing (Hurst et al., 2012). Such a reduction in relief will reduce R^*, resulting in a reduced best fit S_c value.'*

An additional comment is that results from this framework shown in Table 1 suggests there is little variation in Sc among the landscapes that were examined, despite differences in the setting of each site. I think this is a discussion point that could be elaborated upon.

We have added to the discussion to raise this point, highlighting that it is a function of the ranges of E* and R* values we observe in these landscapes, and that similar studies have arrived at the same magnitude of values for critical gradient. The following paragraph has been added to the critical gradient section: *'The similarity of the average S_c values obtained using the bootstrapping procedure across three diverse landscapes highlights the presence of a distribution of $E^* R^*$ values existing for each landscape, and the nature of an average SC measurement. Such a distribution occurs due to local variations in topography, process and material properties and similarities can be drawn between the results presented in Table 1 and other similar studies (DiBiase et al., 2010; Hurst et al., 2012).'*

The comments and requests from the first reviewer are well-posed and reasonable, and should also be weighed by the authors.

We have made significant alterations to the manuscript following the recommendations of reviewer 1, details of which can be found in our response to that review.

Minor comments: I encourage the authors to search the manuscript for "data is" and replace these instances with "data are".

We have made the requested changes.

Page 3. Line 10. "methods published" – this sentence could be rephrased, it is slightly awkward

We have rephrased this sentence to better convey our meaning: *'Such fundamental relationships provide important insight into landscape evolution, however many of these techniques are challenging to implement, due to variable or poorly defined methods, or require proprietary software to obtain data.'*

Page 5. Line 15. Be explicit here about what limits relief. It isn't a critical angle per se, but material strength.

We have rephrased this section to highlight that the critical angle is controlled by material strength.

Page 7. Line 12. I suggest inserting a comma following "(2012)"

Done.

Page 18. Lines 17-19. It would be useful to show these data in a figure, so that readers can compare results from the two Sc values.

We have produced an additional figure (Figure 6 in the new manuscript) which displays this data and have incorporated the figure into our discussion of potential topographic decay in this location.

Page 16. Line 21. Replace "This is" with "The high R* values" or similar phrasing to be more explicit.

Done.

Figure 5. The ordering and lettering of the panels does not parallel the order in the caption. Check that the main text follows the revised ordering, when referring to this figure.

The main text in the manuscript is consistent with the figure caption, and this was the intended ordering of the subplots. Consequently we have re-generated Figure 5 with the subplots in the correct order to reflect the text and the caption.

References

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