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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 et al. propose a framework for computing the dimensionless relief and erosion rates of ridge-and-valley topography using a combination of valley network extraction and hilltop curvature analysis. They argue that their approach allows one to determine whether or not a landscape is in topographic steady state and to determine the mean Sc value (the maximum gradient of stability) of a landscape. Overall I think the paper will be an excellent contribution, once some issues are thoroughly considered and/or caveats provided.

We would firstly like to thank the reviewer for their positive and thorough appraisal of our work. We are pleased that the reviewer considers the manuscript and accompanying software to be worthwhile and believe that through this discussion the manuscript has been significantly improved. We have added additional discussion into the manuscript to account for the limitations highlighted by the reviewer, have enhanced the theoretical background to place the work in a better context and have added additional detail to the methodology to clarify how our data is extracted. We respond to each point made by the reviewer individually below.

(1) I am skeptical that the mean value of Sc is 0.79 in the Oregon Coast Range. Roering et al. (1999) demonstrated that many hillslopes in the OCR have gradients in the 0.8 to 1.1 range, and more importantly that the planarity of hillslopes systematically increases as gradients approach 1.2. These results are hard for me to reconcile with those of Grieve et al. In particular, I am concerned that none of the R^* values in Figure 3a of Grieve et al. appear to be larger than 0.7. This seems to suggest that Grieve et al. did not consider hillslopes steeper than approximately $0.7 * 1.2 = 0.84$. However, we know that hillslopes steeper than 0.84 are

common in OCR. Grieve et al. argue that their results differ from those of Roering et al. due to the different methods for extracting L_H. However, I don't think this adequately addresses the fact that Roering et al.'s slope data clearly show the presence of gradients approaching 1.2 and an increasing planarity of hillslopes as the gradients approach 1.2 in OCR, strongly indicating that Sc is approximately 1.2 in that area. Grieve et al. would likely argue that Sc takes on a range of values, hence the presence of some slopes with gradients above 1.0 or 1.1 does not contradict their conclusion that the average Sc value is 0.79. Maybe this is true, but I would like to see this hypothesis explored in more detail because I find the results of Roering et al. (1999) very convincing in regard to the Sc value they chose.

As detailed in Grieve et al. (2016), the critical gradient which we constrain using these techniques is necessarily an averaged value, as the fitting procedure will pass the steady state curve through the center of the cluster of data points. This results in a critical gradient where approximately 50% of the hillslopes will exceed the best fit S_c. Other attempts to constrain S_c have been made using similar best fit methods (DiBiase et al., 2010; Hurst et al., 2012, 2013) which have found similar underestimates in landscapes when contrasted with the value used by Roering et al. (1999). As noted by the reviewer we argue that our best fit S_c value is the average value of a probability distribution of critical gradients, whereas the value of Roering et al. (1999) can better be considered as an upper bound S_c and should that value be required in a study, the methods of Roering et al. (1999) would be better employed. Our method has the advantage of requiring little user supervision and no field data to constrain an average S_c value and as such can facilitate rapid, broad scale comparisons between landscapes.

We have rewritten the discussion of these points in the manuscript in order to better convey our distinction between different definitions of S_c, in particular we have explicitly made the distinction between the techniques for extracting S_c and have outlined why we observe differences between the results of the methods. The explicit comparison between the S_c values of the 2 methods for the Oregon Coast Range and Gabilan Mesa has also been removed from the discussion, to allow the intercomparison of different best fit methods, rather than the comparison between an average and a maxumum value being made. We have also added a sentence at the end of the discussion to urge the reader to consider which value of S_c is suitable for a given study.

In particular, I am concerned that none of the R^* values in Figure 3a of Grieve et al. appear to be larger than 0.7. This seems to suggest that Grieve et al. did not consider hillslopes steeper than approximately $0.7 * 1.2 = 0.84$. However, we know that hillslopes steeper than 0.84 are common in OCR. Grieve et al. argue that their results differ from those of Roering et al. due to the different methods for extracting L_H.

In the original Roering et al. (2007) work, the upper limit of R^* calculated for the Oregon Coast Range was approximately 0.8, which yields a maximum gradient of 0.96, or a difference between these two gradients of 0.12. Aside from the previously highlighted difference in methods for measuring LH and R between the two works, which result in a small increase in R and a larger increase in LH (which has a net result of decreasing R^*), two other factors will influence the observed R^* values. Firstly, the spatial averaging will act to dampen the extreme values (both high and low), which can be seen by looking at the changes in the range of data points between Figure 5B and 5C and secondly, as demonstrated by Hurst 2012, any hilltops with a gradient above 0.4 must be excluded in order to use hilltop curvature as a proxy for erosion rate.

(2) Why might the Grieve et al. approach be flawed enough to provide a misleading measure of Sc and/or an incorrect assessment of steady state? I can think of at least four possibilities. First, the Oregon Coast Range may not be sufficiently in local topographic steady state for their method to apply at the necessary level of precision required for the presence/absence of topographic steady state and the value of Sc to be reliably determined (see, e.g., Sweeney et al., How steady are steady-state landscapes? Using visible–near-infrared soil spectroscopy to quantify erosional variability, Geology, 2012). In particular, their assumption that landscape-scale erosion rates can be extracted from the hilltop curvature seems to assume a topographic steady state and/or a uniformity of erosion rates that may not apply anywhere at the scale they are working.

In this paper we employ the same definition of steady state as was used in Grieve et al. (2016), whereby we consider a hillslope which retains a constant topographic form in relation to its baselevel, the channel at the base of the hillslope, to be in steady state. Such a formulation, defined by Mudd and Furbish (2004), and employed on a range of transient landscapes by Hurst et al. (2012, 2013) within the context of E^*R^* analysis, allows for the extraction of erosion rates across diverse tectonic and erosional regimes using hilltop curvature.

The clustering of the values in E^*R^* space when contrasted to transient landscapes such as Cascade Ridge (Figure 4) give us confidence that the Oregon Coast Range is in approximate steady state. We consider the variability which we observe in the E^*R^* data in such a landscape to highlight the catchment scale variability which has been predicted in models (Reinhardt and Ellis, 2015) and elegantly demonstrated with field data by Sweeney et al. (2012).

We have updated the theory section to include this explicit definition of steady state with the aim of increasing the clarity of our later discussions surrounding landscapes which are in steady state, such as the Oregon Coast Range.

Second, they are applying a 1D model (equation (5)) to 2D reality. This may seem like a quibble, but the fact that there is nothing like a convergent hillslope in their model seems relevant in assessing its ability to definitively allow us to make conclusions regarding relatively subtle aspects of landscape evolution.

The challenge of applying 1D models to 2D reality is one which we should have explicitly addressed in this manuscript. The data generation and topographic analysis is designed to ensure that we are best able to apply these 1D models to our 2D data by following the methodology of Hurst et al. (2012), who first encountered this challenge. The valley extraction algorithm we employ, which is designed after Pelletier (2013) and Passalacqua et al. (2010) identifies areas of high convergence on hillslopes as valleys, which, when used as the end points for the flow routing algorithm employed to generate L_H and R measurements effectively exclude hillslope traces which cross convergent topography. We have added to the theory section of the paper to address this factor explicitly, presenting a brief review of methods of applying 1D models to 2D topography, and directing the reader to consider our results within the context of the challenges inherent in analyzing real topographic data.

Third, they assume that colluvial transport flux is independent of soil thickness. If sediment flux is an increasing function of soil thickness (as has been shown by many studies) and soil thickness is lower than the landscape average near divides (also common, since divides are divergent yet hillslopes includes convergent areas where soils tend to be thicker), then the approach of Grieve et al. may systematically overestimate the true value of E* since the value of K will be an underestimate for the landscape as a whole. More broadly, equation (5) is of uncertain applicability if sediment flux is a function of soil thickness, bringing to my mind the question of how confident we can be in the results of this method with regard to the presence/absence of steady state.

The reviewer correctly highlights that there is evidence for depth dependent sediment transport on many hillslopes (e.g., Braun et al., 2001). Roering (2008) performed a non-dimensionalization of sediment transport from depth dependent creep and demonstrated an increased sensitivity of hilltop curvature to erosion rates under this transport regime. Which could be used to account for an increase in E^* values in a landscape. Unfortunately it is not currently possible to perform large scale experiments using real topography incorporating a depth-slope product flux law as we do not have soil thickness information at the spatial scale required.

Grieve et al. (2016) used topography to falsify predictions of the linear and nonlinear sediment flux models, but was unable to make any predictions regarding other sediment flux models due to either a lack of analytical solutions to the models, or as is the case for the depth-slope product, a lack of soil thickness information. The resulting work demonstrated that topography in four fieldsites was consistent with a nonlinear sediment flux law and this is the basis with which we employ such a model in this study, which shares the same fieldsites.

In light of this clear limitation to our results we have extended the Theoretical Background section to discuss the existence of other flux laws, and the basis of our selection of the nonlinear model over any of the other choices. We do not consider it possible to quantify the uncertainty in our results as we have no constraint on soil thickness, but trust that by drawing readers to this limitation near the start of the manuscript we can avoid any misunderstanding of the applicability of both our results and the technique as a whole.

A fourth possibility is that some fluvial erosion occurs on hillslopes in addition to colluvial erosion. As a result, their model (which includes erosion by colluvial processes only) might underestimate the true erosion rate. I think all of these possibilities should be considered in the analysis or at least acknowledged as possibilities in the revision.

This is a fundamental observation which impacts upon many facets of topographic analysis. In the case of this study we have taken great care to employ a valley extraction scheme based on the work of Pelletier (2013) and Passalacqua et al. (2010) which performs well across a range of landscapes in defining the transition point between channel and hillslope. However, no such algorithm is perfect when applied to real topographic data and as such we have expanded our methods section to more explicitly address this particular limitation of our topographic analysis.

Minor: The paper has a few typos. For example, "couple" should be "coupled" on p. 14, line 19. The publication year of Grieve et al. (2015) should be (2016). All of the references have strange random numbers included after them. These should be removed.

We have corrected these typos and a small number of other mistakes which were noticed during the typesetting process. The strange random numbers included after the references appear to have been added during the typesetting process, but we are investigating how to ensure that they do not appear in the final manuscript.

I wish to thank the authors for a stimulating paper and wish them the best as they continue on.

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