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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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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.

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.

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. 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. Third, they assume that colluvial transport flux is independent of soil thickness. If sediment flux is an in-

creasing 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. 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.

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.

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

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