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Earth Surface Dynamics Discussions

Interactive comment on "Topographic roughness as a signature of the emergence of bedrock in eroding landscapes" *by* D. T. Milodowski et al.

Anonymous Referee #3

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In this manuscript, Molodowski and others propose that surface roughness, calculated from high-resolution LiDAR data, can be used to identify bedrock outcrops and quantify the extent of soil cover. The manuscript presents first a test of this metric in two landscapes in the Colorado Front Range and southern California; the authors use a supervised classification to identify bare rock regions from orthophotos and compare these to roughness metrics calculated from LiDAR data. In the second half of the manuscript, the authors then apply their method to separate systems in the Sierra Nevada and Idaho where soil cover is expected to vary due to a transient hillslope response to changing incision.

The methods are sound and the authors present a reasonable test of their metric. The manuscript is well written and mostly well structured, though there is a disconnect be-

tween the validation that makes up the first half of the manuscript and the application that takes up the second. I have a few major comments regarding structure and content, and believe these could be easily addressed in a revised manuscript.

1. Validation sites were chosen partially based on the need for minimal vegetation cover. However, the application sites do not meet this same requirement. My issue is not the vegetative cover alone; the authors required sparse cover for their validation so they could use orthophotos to test their roughness proxy. Presumably, this requirement need not apply if the methods and assumptions hold across different landscapes. However, the different ground cover does suggest a different geomorphic system than given in mostly grassland validation sites. The BRC location is covered by mixed conifer forest, and though the authors don't discuss the vegetation of the HC site, it is likely similar. Considering that surface roughness should mark a process transition on the landscape, should we really expect this transition is the same topographically in systems with fundamentally different processes? We know that topographic variability at small scales reflects process – For example, small scale variations in topography may reflect tree throw; e.g., Roering et al., 2010. Wouldn't this variability be distinct in landscapes undergoing different processes. Furthermore, the application is shadowed significantly by the fact that only one of the test sites appeared to validate the method. The correlation between bedrock cover and roughness was poor when vegetation cover was high in their test site at Rayleigh Peak. It seems a bit of a jump to then apply the method in a very different forested system?

2. Importantly, the authors fail to address why roughness should correspond to this process transition. Besides the observation that soil mantled landscapes are generally smooth, we have limited discussion of what specifically creates roughness on a landscape. If we are starting from a process discussion, then we should at least talk about the processes that generates roughness or why the transition from diffusive to detachment limited conditions would generate outcrops with significant roughness.

3. The abstract makes a fundamental assumption – that the transition from soil mantle

to bedrock mantle marks a process transition from diffusive to non-diffusive transport, but, this ignores other important process transitions and variability that also occur in landscapes with significant topographic roughness and bedrock cover. 'Soil' may instead represent points of sediment/colluvial storage in such landscapes, where lateral transport complicates simple production-erosion controls on soil cover.

Line 9: "The resultant increase in surface roughness" is an assumption, not a known. I suggest deleting or rewording this sentence.

Page 388, Line 14: Detrital 10Be is measured on the sand fraction, not the silt fraction.

Page 390, Line 15: Since application sites are so different than test sites in terms of vegetation cover, doesn't this require a more robust validation?

Page 391, Line 13: Detrital 10Be is measured on the sand fraction, not the silt fraction.

Page 393, Line 0-18: This discussion is very interesting, but does not fit into any of the set up or motivation established in the Abstract, Introduction and Methods sections. This section feels disjointed from the rest of the manuscript, and I'm now not sure of the authors' intention in the second half of the manuscript.

Page 395, Line 25: Norton et al also quantified soil cover in the Swiss Alps, and found significant cover in rapidly eroding basins.

Interactive comment on Earth Surf. Dynam. Discuss., 3, 371, 2015.