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Interactive Comment

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

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We thank Reviewer #2 for their review, which provided encouraging and constructive comments, which will help improve the quality of the manuscript. We are happy that the first half of the manuscript in which we introduce and validate the method has been well received. The reviewer recommends revising the second part of the manuscript in which we utilise the method to investigate the soil to bedrock transition associated with the landscape response to changes in fluvial incision. We agree with the reviewer that this section can be more clearly focused on the new roughness-based metric, and will amend the second half to place more emphasis on this. The specific criticisms raised by Reviewer #2 are listed below, alongside a response to each.

1) "In the introduction (Page 374-375), much of the argument for developing a new

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metric of bedrock exposure is centered on the challenges and limitations of the current approaches dealing with a threshold slope cutoff. While the new method clearly does a better job than slope-based metrics in capturing exposed rock in low-sloping landscapes, the accuracy is still dependent on choice of measurement scale, cutoff values for roughness, and the ability to easily calibrate against veg-free orthophotos from a similar landscape. I would tighten the introduction to clarify the general challenges to objectively mapping bedrock exposure, and highlight how different metrics/approaches overcome these challenges, rather than pitching it as a solution to the above challenges."

The reviewer is correct in identifying that the selection of a threshold is still an issue for this method. We will revise the introduction. We will follow the reviewer's advice, and revise the introduction to highlight general challenges for calibrate remote sensing methods, and clarifying which previous limitations this contribution seeks to improve upon.

2) "Following on the first point, I think the discussion could expand on the path forward for other landscapes beyond just low-moderate relief, granitic tor-dominated hillslopes. Will this roughness metric work in steep landscapes? What about layered rocks? Perhaps it is even worth another figure to show how this tool handles a different type of landscape?"

The author is correct to note that our validation landscapes are limited to granitic settings. This was in large part due to the difficulty in locating validation sites with the necessary data and site characteristics. Validation sites required LiDAR data, high resolution orthophotographs, a gradient in bedrock exposure and limited vegetation cover. Furthermore, the bedrock needed to be sufficiently distinct in the orthophotographs to be readily classified using image processing techniques in order to produce a validation dataset. In steep landscapes, our expectation is that in many cases, the texture of the bedrock will still drive elevated surface roughness compared to a soil mantle. Complications may arise where the steep topography is prone to landslides – we dis-

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cuss this in the discussion following the validation of our method (Section 3.5) and in the general discussion and conclusions towards the end of the manuscript (Section 5) – or where bedrock is smooth. We do note that the inner canyon in the Feather River is quite steep and the algorithm appears to be performing well there. Errors introduced where bedrock is smooth may arise due to the rock surface being polished, or where the rock exhibits very low fracture densities, such as plutonic domes where jointing is dominated by surface-parallel exfoliation joints; the textural information within the topography is then indistinguishable from a smooth soil mantle. In landscapes where such features are present, it may be necessary to combine multiple topographic metrics, which we discuss explicitly in Section 5. With layered rocks, slopes parallel to the structural grain may be smooth, whereas slopes cutting the fabric will appear rougher. This may drive variable accuracy in our results. When these smooth surfaces form steep structures, a slope based metric can easily be employed to catch these false negatives; where they form low gradient surfaces, it is more problematic, although if covering a large area, should be visible in aerial imagery. To address these comments, we will add further discussion regarding variability in bedrock morphology into Section 3.5, and emphasise it again in the Section 5. Exploring the role of geological structure and rock type would make a good target for a future study. In addition, we will include two more images of landscapes on layered rock, along with high resolution orthophotographs to give the reader a qualitative idea of how well the algorithm does. These images will include the caveat that they have not been tested against image analysis: we were unable to find a site with sufficiently high contrast between bedrock and soil to carry out calibration in landscapes with sedimentary bedrock.

3) "The most glaring issue with the manuscript for me is section 4 – the application to Bald Rock Basin and Harrington Creek. The application of the new roughness based metric takes a backseat to a discussion that feels either repetitive from other work in the Feather (e.g., Hurst et al. 2012/2013) or speculative and unsupported by analysis in the current paper. I would focus on choosing a solid example of where the roughness metric gives new insight to a geomorphic problem beyond that possible from just

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looking at slope. Or, perhaps a comparison of the different bedrock exposure metrics to highlight any potential pitfalls. The Feather river example might still work well for this with the discussion of “Big Bald Rock Dome” for example."

In Section 4, our aim was to utilise the roughness metric to investigate the soil-bedrock transition associated with increasing rates of erosion. The soil production functions that are widely used in the literature predict an abrupt transition from soil to rock at a soil production “speed limit”: few modelling papers include patchy emergence of bedrock (two exceptions are the Strudley et al and Gabet and Mudd papers cited in the text) but the only paper to quantify patchy emergence of bedrock in the face of an erosion gradient that we are aware of is the DiBiase et al. paper. We quantify this patchy emergence with our algorithm that can detect outcrop in both gentle and steep landscapes, and we show patchy emergence of bedrock in two field sites where it has previously not been quantified. Other than a master’s thesis, no work has been published at the Harrington creek site, and the Hurst et al papers did not address bedrock emergence. Documenting the emergence of bedrock questions prevailing approaches of modelling soil production, so we feel these are important results. In addition, we feel these results are not repeating the work of Hurst et al, but rather are a new contribution. Based on the reviewer’s comments, we have clearly not articulated the importance and novelty of the geomorphic analysis, and perhaps spent too much time on the geomorphic context (which is perhaps why the reviewer considered the section a repetition of earlier results), In the revised manuscript, we will follow the advice of the reviewer, and make some substantial revisions to this section. Specifically, we will focus the discussion on the changing bedrock exposure across the transition, whereas the changes in slope etc. which follow the observations of Hurst et al. (2012, 2013) will be condensed and moved towards the discussion of the site characteristics. We hope that this brings the use of the algorithm to the fore, as suggested by the reviewer.

Line comments:

P374 Line 23-24: “typically obtained” – perhaps better to say “for example”. Also, isn’t the new method subject to the same challenges and limitations w.r.t. calibration and vegetation cover?

Following on from first point highlighted by the reviewer, we clarify the limitations common to all remote sensed metrics that require some degree of calibration, and those aspects that are tackled with this method.

P384 Line 9-11: I would be cautious here. Both validation landscapes are actually quite similar (low/moderate relief with granitic bedrock outcropping as sparse tors). The nature of bedrock exposure morphology is quite diverse, and so it is unlikely single metric will capture the full spectrum of behavior (i.e., granite tors, dip/anti-dip slopes in layered rock, glacially sculpted landscapes, desert landscapes e.g., Navajo Sandstone in Utah). This point touched on briefly by the authors, but perhaps deserves more explicit discussion, given the emphasis on the broad application of this metric/approach.

This corresponds closely to the broader point raised by the reviewer in points 1 and 2 above. We will ensure that more emphasis is given to this aspect in Section 3.5: Implications for use of topographic roughness in other settings.

P388 Line 14: Detrital silts? The Ferrier 2012 paper focused on point measurements of 10Be concentrations in soils.

Sorry, that was a rather glaring mistake. The offending sentence will be rewritten: "Point measurements of regolith production rates, based on cosmogenic Beˆ10 concentrations, suggest erosion rates integrated over 10ˆ3-10ˆ4 years of up to 0.12 mmyrˆ-1 (Ferrier et al., 2012)."

P391 Line 13: Again, silts? Check this to be sure, but I suspect it is sand or coarser.

Will change "silts" to "sediments".

P393 Line 22-25: "Booth et al. (2009) exploited. . ." This should be introduced earlier in the paper.

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This will now also form part of the discussion in Section 3.5: Implications for use of topographic roughness in other settings

P394 Line 14-17: This can be expanded - compare slope vs roughness metrics?

We will include a few sentences expanding on particular settings where it would be beneficial to utilise both a slope-based metric and roughness-based metric in cohort. Moreover we would encourage readers to consider combining metrics in landscapes where this is likely to provide significant improvements.

P395 Line 12-15: What are the implications of this?

Higher point cloud resolutions will permit, better discrimination of vegetation and ground surfaces, thus a more accurate ground DEM. As a result, classification of small wavelength features will be significantly improved. We will try to make sure this point is clear in the new manuscript.

References

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