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Interactive comment on "How does grid-resolution modulate the topographic expression of geomorphic processes?" *by* Stuart W. D. Grieve et al.

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Throughout this document the reviewer's comments are in **bold type** and our responses are in standard type.

This manuscript seeks to quantify how well topographic information of varying resolution can be used to extract quantitative information for landscape evolution interpretation and simulation. The authors have crafted a clear goal in this respect and chosen three well-trod soil-mantled landscapes to explore how successive degradation of DEM resolution affects estimates of curvature, slope length, relief, and channel network extent. While the geomorphic community

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Discussion paper

trumpets lidar's superior capabilities for these endeavors, the authors are correct in noting that the extent of lidar coverage is limiting (although we should still advocate for global coverage!). This manuscript succeeds because it nicely presents empirical findings and because it incorporates a nifty theoretical explanation for why we lose the ability to resolve features of a given wavelength with changing grid spacing. Below, I describe a small handful of suggestions that should be easily incorporated if so chosen.

We thank the reviewer for their detailed and positive consideration of our work. We are delighted that the reviewer considers the manuscript to be successful in exploring the influence of grid resolution on geomorphic analysis. We believe that through the discussion points highlighted below, the manuscript has been significantly enhanced, and will consequently be of greater value to a wider audience. At the recommendation of the reviewer we have expanded the introduction to highlight the challenges inherent in comparing different data products, with the aim of giving readers more confidence in the use of lower resolution data products, where appropriate. We have clarified the terminology used throughout the manuscript and highlighted minor points which enhance the interpretation of our curvature results. The spectral analysis section has been expanded to provide a clearer explanation of the equations used, in order to better highlight the relevance of this section to the remainder of the manuscript.

Major comments: 1) Are DEMs of the same grid spacing necessarily equal? The issue of varying DEM grid spacing is the fundamental message of this manuscript and the authors argue for the utility of the results obtained here for interpreting TanDEM-X and SRTM data, but these datasets are derived differently from lidar data and it's not clear to me that the exercise laid out here will always be relevant in this respect. In other words, can the authors show that degrading lidar data to 12meters is essentially the same as obtaining a

TanDEM-X dataset? Or similarly for a 30-m SRTM dataset? Because we hold up lidar as the gold standard, it's appropriate as the reference but it seems that additional factors will likely affect how well radar-based techniques can resolve surface features. It would be highly convincing, for example, if the authors could show that a degraded lidar dataset is consistent with TanDEM-X or SRTM data in a simple way that closes the loop and truly opens the door for widespread use of the principles established in this very fine paper. Perhaps the answer is already known and a simple 'yes, this works' can be garnered from the literature.

This direct comparison between data products was something we considered during the planning of this research. We decided to avoid directly testing particular data products as we wanted to show the effect of changes in data resolution, rather than provide a quality estimate for a range of data sources. We agree that such a comparison would be a compelling result to add to this work, however, a full evaluation of the similarity between data products would be beyond the scope of this contribution. The errors between LiDAR data and other data products are region specific, with errors appearing to be connected to landscape morphology and it would be challenging to make a broad conclusion that any SRTM or TanDEM-X data product is globally equivalent to down-sampled LiDAR without local scale testing. We have added a brief discussion of these challenges, highlighting the need for further evaluation of these datasets to Section 2.1.

2) Language: the manuscript cycles through the terms 'resolution' and 'grid spacing' in various forms and there are a few places where the meaning can become confusing (e.g., line 497). The word 'resolution' is sometimes traded for 'spacing', inviting ambiguity and my suggestion is to simplify wherever possible.

We have gone through the manuscript and clarified our terminology, to only refer to

increasing or decreasing (grid) resolution, rather than grid sizes or spacing.

3) Broadly convex hilltops can be resolved with coarse data. The authors nicely show that curvature at the Gabilan Mesa site is retained as grid spacing increases, which is a very nice result. After seeing more and more of the GM lidar data, it does appear that many sections of that study area exhibit remnant surfaces that are not yet adjusted to regional baselevel lowering, which is a potential bias that could merit mention. That said, I think the result here is nonetheless robust.

We have added a statement into Section 3.1 to highlight this complexity within Gabilan Mesa.

perhaps more importantly, though, I wonder how this result would be useful for a researcher working in a far-flung field area without a priori knowledge of whether their study site exhibits broadly convex hilltops. How would they be able to determine whether they're working with an Oregon Coast Range or Gabilan Mesa like study area? How will they know to trust their curvature values or not?

This will always remain a challenge inherent in desk based research, and if the only data available to a researcher is low resolution topographic data, it would be challenging to be entirely confident in measurements of hilltop curvature. We have added a statement to the discussion (Section 5.1.2) to emphasize this point, although in most cases it would be possible to gain a qualitative understanding of the landscape morphology through other non-topographic datasets.

4) Theoretical explanation for grid spacing dependency on feature resolution. Section 5.1.1. does a very nice job of laying out a spectral analysis-based explanation for this problem. It nicely complements the empirical analysis here but it is terse and challenging to follow. My concern is that readers without a background in signal processing will not be able to follow it. The wonderful outcome in lines 640s to 660s merits much fanfare and is well-stated, but getting there could be much more straightforward. On line 629-634, exactly how does the gain equation show the associations stated, for example? There is no curvature value in this equation (strictly speaking), so the connection b/w gain and the traditional metrics could be more clear. In fact, my sense is that this section would be more effective as a substantial part of the manuscript methodology rather than a theoretical afterthought stuck in the discussion. This analysis is central to the message here and the authors might consider putting it upfront because it bears on all of the empirical results. That said, it would require some hand holding to be effective in that role. In particular, the goal needs to be clearly stated as well as simply stated explanations about wavenumber response function, continuous vs. discontinuous waveforms, and gain. The explanation for fidelity is nicely stated but I fear most readers will have to do substantial work to get there...which would be rewarded if they do so, however! A major rehaul is not the suggestion, but rather a more integrated role for this section. In particular, the very different curvature and slope fidelity curves are highly compelling (fig 12). Because the loss of slope fidelity explains previously published papers, this also strikes me as something that's more meaty than discussion subsection material.

We thank the reviewer for their positive comments about this section. Prior to submission, we debated placing this section in the methods section rather than within the results section. We felt that this led to a more disjointed narrative than is present in the current structure of the manuscript. In other words, we tried what the

reviewer suggests and didn't really like the result: the manuscript flows better if the numerical problem is highlighted and then we use the analytical solutions to show why this problem exists. This ensures that the paper's focus is on the practical problem of determining what one can actually say about a low resolution DEM rather than throwing the reader into a cold bath of spectral analysis before any results are reported.

We will follow the reviewer's advice on expanding this section so it becomes (we hope) clearer to readers where the equations have come from and what they mean.

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