Earth Surf. Dynam. Discuss.,
doi:10.5194/esurf-2017-4-RC1, 2017


Earth Surface Dynamics Discussions
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EGU



# *Interactive comment on* "Validation of digital elevation models (DEMs) and comparison of geomorphic metrics on the southern Central Andean Plateau" *by* Benjamin Purinton and Bodo Bookhagen

**S.W.D. Grieve (Referee)**

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Received and published: 7 February 2017

I would firstly like to thank the authors for this excellently written contribution, it was very interesting to read and provides a much needed analysis of the utility of modern satellite derived elevation products for geomorphic analysis. The figures and tables are clear and well constructed and the additional information provided in the supplement greatly clarifies much of the technical work undertaken in this analysis. This manuscript compiles a collection of optical and radar derived DEM products from both open access and commercial sources and evaluates their accuracy against a



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Discussion paper

large collection of dGPS points. From this accuracy assessment a subset of the DEM products are selected upon which to perform a series of geomorphic measurements to test the applicability of these products for modern geomorphic analysis. The geomorphic evaluation of the DEMs applies both hillslope and channel metrics and demonstrates the potential and limitations of using spaceborne platforms to acquire elevation data for geomorphic analysis. An additional piece of analysis which is very valuable is the 2D Fourier frequency analysis, used to identify high frequency noise in the optical datasets, which has previously been identified as having a geomorphic origin, but in this instance appears to be solely derived from instrument error. This paper has the potential to become a valuable contribution to the discipline not only from the wide relevancy of the analysis and results to the surface processes community, but also due to its scientific rigor and clarity.

**General comments**

Overall I have no major concerns surrounding the analysis or the presentation of the analysis and consider the manuscript to be close to being ready for publication. However, I offer the following comments:

I found the abstract very dense, in particular because of the use of a large number of acronyms. While I recognize that the results for individual data products need to be specified, it may help the reader get to grips with the aims of the paper, if in addition to grouping the datasets in the 4th line of the abstract by resolution, they were also grouped into radar and optical sources, as occurs later in the manuscript.

In the discussion surrounding the measurement of hillslope length, the challenges of

interpreting hillslope length from slope area plots are highlighted clearly and although I prefer the use of a flow path method to discern hillslope length I appreciate the utility of the slope area method in this study and believe it provides some very interesting results. However, one additional issue I would like to see highlighted is the assumption that grid resolution is equivalent to the unit contour width and can be used to convert a drainage area into a characteristic hillslope length. In some cases this may be appropriate, but these two parameters are distinct and to my knowledge no work has been done to attempt to correlate these parameters. I do not expect your analysis to change, as varying a constant in the calculation of hillslope length will not change the trends of your results, however, adding a sentence to highlight this assumption within the methodology on Page 12 would enhance the clarity of this section.

Throughout the analysis, curvature and slope is calculated using a 9 cell window, which suggests that as the grid resolution is varied between data products, these derivatives of elevation will be calculated across differing length scales, potentially capturing the signals of processes operating at distinct spatial scales. I would be interested to see a small discussion of this difference between this paper's approach and other approaches to measuring curvature and slope from kernels of a variable radius.

In the line by line comments below I have identified some confusion between the terminology of grid resolution increasing or grid size decreasing. Please check the manuscript for any other instances of this.

A lot of reference is made to supplemental figures S4 to S9, I appreciate that these

are large figures, but it may be more valuable to present this information in the main manuscript to ensure readers who don't always read supplements will still see the interesting results from these datasets. However, I will leave this up to the authors and the AE to decide whether this will result in too many figures in the main manuscript.

**Line by line comments**

In addition to the issues mentioned above, I have some more general minor line by line comments:

Page 2, Line 23 - There have been developments in the production of adaptive resolution DEMs (e.g. Liu et al., 2014), it would make this section more complete to direct an interested reader to some of these papers.

Page 2, Line 31 - Grid resolution is increasing, grid size is decreasing.

Page 3, Line 26 - With this list of lidar applications it would be better if the references were placed alongside their examples, rather than a long list of uses followed by a long list of references.

Page 32, Line 7 - The global compilation of m/n values and other properties by Harel et al. (2016) would be a good reference to add in here to place these results in their full context.

Page 35, Line 27 - Is there a reason for the selection of a 1 km radius for the estimation of relief?

Page 39, Line 4 - This is the only paper title in the reference list which is in

block capitals.

Page 39, Line 8 - Check the formatting of the author's name for this paper.

**Figures and Tables**

Figure 10 - As the points obscure each other, would it be possible to introduce some transparency to the diamonds to more clearly show both datasets?

**Supplement**

The supplement is an excellent addition to the main text and provides detailed information on the data and the methodologies employed. Everything is very clear, aside from the description of the use of "standard GIS tools", it would be helpful to indicate which program you used to help future authors reproduce your work.

I have also gone through the provided Matlab code and although I have not run it as I do not have access to the right licenses, from a close reading of the code it appears to implement the analysis which is described in the paper. I would also like to thank the authors for sharing their code.

– Stuart Grieve

**References**

M-A Harel, SM Mudd, and M Attal. Global analysis of the stream power law parameters based on worldwide 10 be denudation rates. Geomorphology, 268:184–196, 2016.

Zhaoqin Liu, Man Peng, and Kaichang Di. A continuative variable resolution digital elevation model for ground-based photogrammetry. Computers Geosciences, 62:71–79, 2014.

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