Earth Surf. Dynam. Discuss., 3, C299–C301, 2015
www.earth-surf-dynam-discuss.net/3/C299/2015/

Earth Surface Dynamics Discussions Open Access

Interactive comment on "Tectonic geomorphology at small catchment sizes – extensions of the stream-power approach and the χ method" *by* S. Hergarten et al.

W. Schwanghart (Referee)

w.schwanghart@geo.uni-potsdam.de

Received and published: 8 September 2015

Hergarten et al. develop and explore an extension of the chi-transformation to small catchment sizes by introducing an additional parameter to the stream power equation. As such it is a direct translation of the method of Stock and Dietrich (2003) that extends the stream power equation to headwater areas dominated by debris flows. In addition, the authors introduce an alternative optimization scheme to find a mn-ratio (and offsets to the catchment area) that linearizes the chi-elevation relation. These methodological developments are novel. The manuscript is well written and fits well within the scope of Earth Surface Dynamics and the special-issue theme. However, I have some major

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

comments that the authors should address before their manuscript is acceptable for final publication in ESURF.

1. Why should we extend a method (the stream power equation) to a geomorphological process domain (one dominated by debris flows) for which it was not tailored? I can envision the value of the approach for creating maps of chi-values (Willett et al. 2014) to better illustrate and quantify the contest of the drainage basins and possible directions of drainage basin capture. However, this comes at the cost of introducing a second parameter whose representativeness of the geomorphological processes in the vicinity of the divide is questionable.

2. The limitation of the chi-method to small catchment sizes is not exclusively set by the transition from a fluvial to a debris flow domain, but may also be due to the resolution of the DEM. A good illustration of the limitations of DEMs with 30-m resolution (at least to derive planform stream patterns) is shown by Stock and Dietrich (2003, Fig. 3). To which extent will the introduction of a second parameter serve balancing the decreasing representativeness of the DEM and to which extent does it actually model the debris flow domain? Here, comparison of the approach using datasets with different spatial resolutions would enable clarification.

3. Additional parameters in a model will always increase the goodness-of-fit statistics if training data is used for model evaluation. This is not necessarily true for the predictive performance. The authors might want to consider assessing the different models using a training and validation set, or alternatively use metrics that penalise goodness-of-fit statistics for additional parameters (e.g. Akaike or Bayesian Information Criterion). This will provide a more objective evaluation whether inclusion of the additional parameter is justified or not.

4. I like the visual presentation of the results. However, I think that the presentation could benefit from adding another figure similar as Figure 1 that compares an actual river profile of a single river reach extending close to the divide with the chiplots derived

ESurfD

3, C299–C301, 2015

with and without area offset.

References

Stock, J., Dietrich, W.E., 2003. Valley incision by debris flows: Evidence of a topographic signature. Water Resources Research 39. doi:10.1029/2001WR001057

Willett, S.D., McCoy, S.W., Perron, J.T., Goren, L., Chen, C.-Y., 2014. Dynamic Reorganization of River Basins. Science 343, 1248765. doi:10.1126/science.1248765

Interactive comment on Earth Surf. Dynam. Discuss., 3, 689, 2015.