Dear Guilia,

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we have now prepared a revised version taking into account the suggestions made by the two reviewers. In order to get a better impression on figure size and labeling, we have already used the ESURF layout instead of the ESURFD layout.

Comments of Jaroslaw Jasiewicz:

Good paper but require to be less engineering-oriented. I suggest to replace or supplement formulas (not very clearly derived) with additional figures as paper is addressed to geoscientists not geophysics. The goal of the paper should be more clear. The experimental part of the paper is interesting and well presented.

During the last years I got the impression that several publications in quantitative or tectonic geomorphology are on a rather advanced mathematical level. We therefore wrote the equations with some kind of minimum steps in between. The step to Eq. 8 was probably the most difficult part, so we extended the derivation and explantation here (lines 94-100). Furthermore we added a short hin on how to modify the χ transform (lines 198-199). The suggestion to point out the scope more clearly is considered beloe (first comment by Wolfgang).

Comments of Wolfgang Schwanghart:

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

One may argue whether all these comments fall into the category "major comments". However, we hope that the extensions of the paper derived from three of these comments have improved the quality of the manuscript.

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.*

   Isn't it straightforward to extend constitutive relationships to the widest possible range of parameters? In our opinion, the "debris flow regime" is just a name for a domain where the erosion rate still depends on the catchment size, but the dependence is effectively weaker than predicted by Flint's law. The good fit of χ_b (as well as the results of Stock et al 2003) even suggests that the total erosion rate is the sum of the fluvial erosion rate and another (hillslope) component being essentially independent of the catchment size. We have now pointed out a bit more in detail what it is good for. Beyond making χ maps in general (what we already mentioned), unraveling the contest of drainage basins is a good example as the effect is strongest close to the drainage divide (lines 155-167). The additional parameter is discussed below (at point 3).

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.*

   As far as I found, the χ method is rather robust against the inappropriate representation of the drainage network on coarse DEMs; much more robust than local channel slopes. I recently tested it with the "old" SRTM3 data set finding no significant difference to SRTM1. It seems that the relationship between mean channel slope and catchment size follows the original stream-power law even slightly better at small catchment sizes on the coarse DEM. I therefore think that the effect of limited DEM resolution is even opposite to our correction, so that the deviation must be related to a different regime of erosion at small catchment sizes and not to the DEM resolution. We have prepared a new version of Fig. 1 also displaying SRTM3 data illustrating the effect (lines 138-140 and 144-149).

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.

Yes, the perhaps spurious improvement by each additional parameter was the reason to consider the one-parametric approaches χ_a and χ_b. Comparing those approaches with the original χ_θ should not be biased by the number of parameters. We intentionally avoided a quantification of the improvement by the two-parametric approaches (and only stated that it may be not very much) for two reasons:

- Separating training from validation would require an additional model how the parameters a and b depend on precipitation, lithology, etc. Otherwise we must assume that these values are constant, and there is no reason why a or b should be constant, while θ varies from catchment to catchment.

- Formal information criteria require a penalty for the number of parameters. Here the problem is that the number of observations is very high (typically about 10000 per catchment, much higher than the number of parameters), but these observations cannot be seen as statistically independent.

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 with and without area offset.*

   Good idea! We have prepared extensions of Figs. 4 and 5 showing the respective main rivers up to the drainage divide in their original profile as well as in the considered χ representations.

All the best,

Stefan

Stefan Hergarten

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