Earth Surf. Dynam. Discuss., 1, C526–C531, 2014
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Interactive Comment

Interactive comment on "Constraining the Stream Power Law: a novel approach combining a Landscape Evolution Model and an inversion method" *by* T. Croissant and J. Braun

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Received and published: 14 January 2014

We are grateful to Dr. Gareth Roberts for his detailed and constructive review that will definitely help in improving our paper. Following is a detailed response to his specific comments.

Specific Comment n°1:
First, it is encouraging to see that when K and U are known, the values of n and m can be reliably retrieved for a synthetic landscape. However, when K is also free predicted n and m are incorrect. Why are the best-fitting values of n and m smaller

than they should be? As well as showing misfit scatter plots and PDFs, you should show comparisons between observed and theoretical river profiles. These additional plots help the reader to see how well you are fitting data. Apart from Figure 2e & 2f, the authors do not show calculated landscapes. Show observed vs. theoretical river profiles, and difference maps (i.e. reference topo - best fitting model topo) for each test. By plotting misfit as a function of n and m along minimum misfit valleys the reader could evaluate if the values of n and m are well constrained.

The small values of predicted m and n parameters come from the fact that the misfit definition contains an intrinsic minimun corresponding to n=0 and m=0, as already stated in the text (see in Fig.3 in supplementary material where the difference between observed and the m=n=0 theoretical topography is shown to be nil). As both parameters tend toward zero, K has to increase to maintain the same erosive power for the landscape to remain at equilibrium and doing so, tends toward the value of U.We have added a short statement in the text of the manuscript to better explain this point.
Even for small value of m and n, the river profile in supplementary material Fig.1 is very close to the target (or observed) river profile. This demonstrates that the major criterion to reproduce a river profile is contained in the river concavity and not in the exact value taken by the parameter m or n (with the misfit definition used in this study). We also agree with the reviewer that showing theoretical vs. observed (or target) river profiles and map of differences is indeed necessary to visualize the quality of the method, the figures have been added to the supplementary material (Fig.1 and 3). Moreover, plot of misfit value against m and n along a misfit valley (i.e. misfit < 30) are shown in supplementary material Fig.2. The clear reduction of the misfit value for observed values of m and n attests the quality of this approach when U and K are known. This figure has also been added to the supplementary material (Fig.2).

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Specific Comment n°2:

Secondly, what do the results of inversions where synthetic reference topography was generated with $n \neq 1$ look like? Can you reliably retrieve $n \neq 1$?

We agree that to study the behavior of the inversion procedure with a non linear erosion law is interesting. To do so we run some extra model experiments with a reference topography generated with $m = 1$, $n = 2$ (ensuring a concavity of 0.5), K = 10^{-7} and $U = 0.0005 m/yr$. Results of the inversion when n and m are free show a misfit reduction for the reference value of m and n. The PDF associated with these two parameters demonstrates the strength of the method to reliably retrieve the parameters values ($n = 2 \pm 0.015$ and $m = 1 \pm 0.003$) even for non linear erosion law. figure has been added to the supplementary material section (Fig. 4) and a short sentence has been added to the text to refer to it.
The case where m, n and K are free (not shown here) presents the same type of behavior than the case in the paper.

Specific Comment n°3:

Thirdly, you should include results from inverting real and/or synthetic transient landscapes in this manuscript. Can you more reliably retrieve erosional parameter values in transient landscapes?

We agree that the study of the transient behavior of rivers should be explored with this novel approach and has been stated as a perspective in the conclusion. This study is currently under development and will be the subject of another publication.

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Technical corrections:

âĂć Pg. 899, line 25–pg. 900, line 2. I think that the phrasing here is a bit odd. Presumably you didn't run models with K < 10^{-6} because of the computational burden of running with small time steps? If so, I would state that.

We agree that the phrasing may be ambiguous. To extend the range of values for K increases the computational cost of the inversion because it requires more forward models to explore the parameter space. In the synthetic cases, K is already known, we then chose to let it vary in a small range of two orders of magnitude (centered on the K_{ref} used to generate the reference model) in order to understand the behavior of the misfit function. In the natural case (application to the Whataroa catchment), K is unknown so the range is extended to 9 orders of magnitude to better explore the parameter space. We have added a short sentence to explain this in the text. In Fig.2a (original manuscript) we can see that there is an abrupt change in the misfit value (between the high misfit value domain (in blue) and the rest) due to the range in which we allow K to vary.

âĂć Pg. 906, lines 19–27. What Roberts & White (2010) and Roberts et al. (2012) did is not accurately reported. In their studies erosional constants are not 'fixed at arbitrary values'. Rather erosion rate was calibrated against independent constraint. The values of m and n are constrained by the minimum residual misfit between theoretical and observed river profiles (e.g. Figure 11, Roberts et al., 2012), which is a similar approach to what was described in this manuscript. K is constrained using independent constraints (i.e. local incision estimate, known uplift histories). In their studies, calculated uplift was not assumed to be related to dynamic support. The shapes of river profiles do not directly tell us about the mechanism of uplift, but provide

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useful clues about the temporal and spatial evolution of uplift.

We agree that the turn of phrase is not accurate and have modified this part in the revised manuscript.

âĂć Spelling and grammatical mistakes (e.g. pg. 892, lines 2 (tectonic), 20 (provide); pg. 893, line 17 (commonly); pg. 894, lines 2 (define SPM), 3 (increase); pg. 901, lines 3 (New Zealand), 10 (dryer); pg. 903, lines 10 (et), 13 (constrained); pg. 904, line 28 (constrain); pg. 907, line 2 (containing).

We thank the reviewer for his corrections. All grammatical errors have been corrected in the revised manuscript.

âĂć Figure 2d, e, f. Add extra elevation label to colour bar for scale.

The figure has been modified according to the reviewer suggestion in the revised manuscript (Fig.2).

âĂć Figure 5. More detail would help the reader to locate themselves. Show drainage divide and location of Alpine fault. Show the location of pixels (rivers) used to calculate misfit. Linking this figure to plots of observed and theoretical river profiles would help the reader to evaluate the goodness of fit.

We agree that this figure should be more detailed. The color scale has been changed, the location of the main divide and Alpine fault have been added as well as the catch-

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ment location in the south island of New Zealand (see Fig. 5 in revised manuscript). The location of pixel used to calculate the misfit and the associated river profiles are shown in supplementary material Fig. 5.

Please also note the supplement to this comment:
http://www.earth-surf-dynam-discuss.net/1/C526/2014/esurfd-1-C526-2014-supplement.pdf

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Interactive comment on Earth Surf. Dynam. Discuss., 1, 891, 2013.

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