Earth Surf. Dynam. Discuss., 3, C421–C425, 2015
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Earth Surface Dynamics Discussions Open Access EGU

Interactive comment on "Modelling sediment clasts transport during landscape evolution" *by* S. Carretier et al.

Anonymous Referee #2

Received and published: 18 November 2015

Summary Presently there is a disconnect between linking particle scale dynamics at the process scale to long term average fluxes used in models of landscape evolution. To start to bridge this gap the authors develop and adapt a set of rules for river and hill-slope transport laws to introduce particles of an arbitrary size to a cell based reduced complexity landscape evolution model. These rules are then tested against analytical predictions with simple scenarios. Such an approach holds promise to begin to understand how long term particle displacement may be linked to landscape scale transport laws.

General comments:

In general I am supportive of the reduced complexity approach that the authors employ. These comments while seemingly critical are intended to broaden the applicability of the manuscript as I am generally in favor of publication as the approach is one that should be adopted more often.

The introduction is perhaps a little too short. Tackling both landscape evolution models and particle transport in about a page of text is by no means an easy task and sells some of the novelty of the paper short. I suggest to focus the introduction a bit more on why it is challenging to couple both particles and landscape evolution models, and why each approach separately has not been able to rectify this gap. You may be able to avoid discussing the particle scale physics or individual particles at all as the paper treats particle transport in a continuum sense (i.e. there is no discussion or use of stochastic particle step length or rest time distributions).

The initial model set up in sections 3 to 3.2 is well laid out and easy to follow, however more information is needed to understand how the authors treat the addition of clasts to the system. In that it is not clear how the clasts are actually moved, in line 6 on page 14 there is a mention of a numerical random function to used to move the clasts. It is not clear to me where in the model framework that fits in, nor is it present in any of the given equations.

It is worrying (or perhaps just perplexing) that the variance and the mean are not equally susceptible to the choice of dt and dx. In particular the authors should explore and rigorously comment on why the smallest spatial step (dx) results in the poorest agreement between the expected and the model results for all model runs. In the conclusion the author's state that decreasing the cell size decreases the over estimation, but the smallest cell size has the greatest mismatch with the expected trends, unless I am really misreading the graphs.

It is interesting to see the modeled topography evolve with the addition of sediment clasts in figure 6 and section 4.4, but it would be useful and a great contribution for the authors to analyze the resulting clast and topography statistics to show that the model results are capable of producing realistic topography. Such as whether the slopes of

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the catchment satisfy well known slope area scaling relationships, or how the hydraulic geometry of the channels grows with area (width is specified from the outset so it satisfies this relationship by definition). Did the clasts stop moving because they reached a low enough slope or because they became buried (i.e. why has the trailing edge not transported itself further or why has the leading edge transported itself so far). I understand that it is not the authors intentions to analyze the final modeled topography or clast statistics in figure 6, but in that it represents the strongest case for their approach it would represent a substantial contribution to their ideas. Currently the gap between figure 6 and figures 3, 4, and 5 is rather vast (adding a channel to the run in figure 5 would help in reducing this gap). In that I am left wondering if the model actually produces statistically realistic topography and therefore has potential to realize the perspectives in section 5. Figure 6 is in many ways the realization of the manuscripts title, while figures 3, 4, and 5 represent more proofs of concept that clasts can be tracked using the rules developed for hillslopes and river networks.

The following thoughts, comments and critiques are given in the order that they appear in the manuscript.

pg.3 ln 26 - It might be best to reword this sentence as neither Hassan et al., (1991) or Haschenburger (2011) use magnetic minerals as tracers, they have implanted magnets into natural cobbles. As an example the work of Houbrechts et al., (2011) uses magnetic iron slag as a tracer (their work may be relevant considering the longer timescales of the study). It may also be useful to consider the recent work using RFID tracers (there are numerous studies over the last decade), the work of Bradley and Tucker (2012) comes to mind as they develop the transport distance distribution for a large population of tagged clasts.

pg.4 ln 11 - reduced-complexity rather than complexity-reduced.

pg. 9 ln 19 - In this line are "Eqs. (1) and (2)

pg. 10 ln 1 - "a brutal", is this supposed to be abrupt?

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pg. 10 equation 9 - in the case of setting the river width to Qˆ.5 is the width then allowed to change due to the bank/lateral erosion in section 3.1.4 or are the banks only allowed to erode when the river width is set to the cell size? If width is set to the cell size and it is allowed to evolve does it reproduce the Qˆ.5 scaling?

pg. 11 eq 10 - Qs is not defined in this paragraph or previously.

pg. 12 ln 8 - suggest shallower or smaller rather then lower. Lower as used here connotes below which does not seem to be what the paragraph is saying.

Figure 2 A & B - It is very difficult to see the black dots under the red. Most readers will completely miss that they are even present.

Figure 5 A - final panel has "ans" instead of yr.

Figure 5 B - "analitical" should be analytical.

Figure 6 - Northern and southern boundaries are referenced in the text, however there is no indication within the figure where these directions are.

pg. 18 ln 18 - The debate on decreasing grain size as one travels down river has mostly concluded at this point. Size selective deposition is responsible for the majority of the downstream fining process in all but the steepest catchments where steep slopes and large particles allow for energetic collisions. See Fedele and Paola (2007), Paola and Seal (1995) and more recently Miller et al. (2014) for a catchment specific view.

Additional references cited in review: Bradley, D. N., and G. E. Tucker (2012), Measuring gravel transport and dispersion in a mountain river using passive radio tracers, Earth Surf. Process. Landf., 37(10), 1034–1045, doi:10.1002/esp.3223.

Fedele, J. J., and C. Paola (2007), Similarity solutions for fluvial sediment fining by selective deposition, J. Geophys. Res.-Earth Surf., 112(F2), F02038, doi:10.1029/2005JF000409.

Houbrechts, G., Y. Levecq, V. Vanderheyden, and F. Petit (2011), Long-term bedload mobility in gravel-bed rivers using iron slag as a tracer, Geomorphology, 126(1–2), 233–244, doi:10.1016/j.geomorph.2010.11.006.

Miller, K. L., T. Szabó, D. J. Jerolmack, and G. Domokos (2014), Quantifying the significance of abrasion and selective transport for downstream fluvial grain size evolution, J. Geophys. Res. Earth Surf., 119(11), 2014JF003156, doi:10.1002/2014JF003156.

Paola, C., and R. Seal (1995), Grain-Size Patchiness as a Cause of Selective Deposition and Downstream Fining, Water Resour. Res., 31(5), 1395–1407, doi:10.1029/94WR02975.

Interactive comment on Earth Surf. Dynam. Discuss., 3, 1221, 2015.