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Earth Surface Dynamics Discussions

Interactive comment on "Short communication: Rivers as lines within the landscape" *by* John J. Armitage

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Response to review by Liran Goren:

First I would like to thank Liran Goren for taking the time to review my submission. I found here comments thought provoking and very valuable. Liran Goren points out three shortcomings of my manuscript, some of which I have to agree with her on, and that I will respond to below:

(1) First, given the fixed cell size, the differences between the cell-to-cell steepest descent routing algorithms and the node- to-node steepest descent routing algorithms might not be accurately presented. Isn't it possible to cast the cell-to-cell as a node-to-node over the complementary hexagonal graph, whose edges connect the centers

of the triangles of the original grid? In this case, the differences between the two implementations lie in the grid spacing, l vs. l_s, and in the possibility to route water in 6 directions over the triangular grid with respect to 3 directions in the complementary hexagonal grid. If the number of routing directions is the critical issue, then the difference between the simulations is not the outcome of river representation (i.e., rivers with finite width with respect to rivers as lines), but of the grid shape. Specifically, using cell-to-cell hexagonal grid should be similar to node-to-node triangular grid.

If this claim is true, then the comparison between these two implementations could be invalid, and as a consequence, it cannot be used to test the theoretical claim for the advantage of representing rivers as lines, which is central to the manuscript.

I think Liran Goren is correct, but also not. First I do not agree that the comparison is invalid. My intention was to compare the standard way of routing water from cell to cell, the steepest slope of descent. This routing algorithm is used in many very popular landscape models. By changing the way I considered flow to be routed, along the cell edges, and by distributing that flow I believe the model results are no longer grid resolution dependent.

But, I was wrong that the reason for this independence is due to the "rivers as lines", but as pointed out by Andrew Wickart, it is more likely due to the multiple flow paths. By treating flow paths as lines I get 7 flow directions, and therefore yes if my cells had 7 sides then perhaps grid resolution would also not be a problem. For this reason I agree with Liran Goren that my "theoretical claim for the advantage of representing rivers as lines" is not why the model is resolution independent. Rather it is because I can route flow down many directions.

This is in itself an interesting finding. I propose that I could lengthen the introduction and methods to discuss distributed flow routing. I could then demonstrate how if I route flow as lines and distribute this flow the model is not resolution dependent. This would involve a significant modification of the structure of the manuscript, and therefore I would like to know the Editor's opinion on this before I make changes.

2) Another issue is the observed unsteadiness of the topography only with the distributed flow routing algorithm. Goren et al., 2014 (Earth Surf. Process. Landforms) showed that a similar unsteadiness emerges in the DAC LEM that uses the steepest descent algorithm, but importantly, allows for the drainage area and the discharge in each grid node to vary continuously though time due to shifting water divides. The possibility for a continuous change in the drainage area leads to the emergence of the drainage area feedback, by which an increase in the drainage area, leads to faster channel downcutting that propagates downstream and then upstream back to the original node, promoting further drainage area increase. Since the downcutting signal propagates throughout all the tributaries, it affects all the neighboring basins, leading to ongoing "ringing" in the landscape elevation and erosion rate, namely, to unsteadiness. Goren et al., 2014 further developed the argument that this behavior is similar to the one observed in the distributed flow routing algorithm of Pelletier 2004, whereby small changes in elevation affect the local discharge (equivalent to area), which leads to further changes in elevation. This means that distributed flow routing is just one possible implementation for representing the 'area feedback' that is responsible for unsteadiness in numerical, experimental, and possibly natural landscapes.

I have scanned through the Goren et al., 2014 paper and while the DAC model can generate unsteady landscape, the mechanism for this is due to drainage capture and not a distributive flow routing algorithm. Therefore, is the point being made here that unsteadiness is a non-unique indicator of the appropriateness of using a distributed flow routing algorithm? If so, I would counter that the distributive flow routing also leads to a grid resolution independence. These two measures, I believe, make it superior to steepest slope of descent.

3) A third issue is the observation of resolution independency with the distributed flow algorithm. This, as well, has been documented in Goren et al. 2014 (e.g., fig 10) for the DAC LEM that implements the steepest descent algorithm. It is therefore possible that

the resolution independency is the outcome of the LEM ability to represent the area feedback, while the distributed flow algorithm is just one possible implementation for it.

In Figure 10 of Goren et al. (2014) the location of the drainage divide is plotted against model resolution for a comparison between CASCADE and DAC. The models are forced by a uniform uplift and constant tectonic horizontal velocity. This is understandably because the focus of this work is drainage capture. However, I am uncertain if this comparison of resolution dependence is relevant if I am looking at topographic wavelength or sediment flux out of the model domain against time. To answer that question I would need to run DAC for the same configuration as I implemented in Figure 1 of the manuscript. I however do not have access to DAC.

4) Finally, the use of a diffusion equation (eq. 1) rather than an advection equation to represent incision along fluvial channels at the scale of a mountain range needs to be justified, since such an equation cannot produce knickpoints, which are a dominant feature in mountainous rivers.

I am interested in the resolution dependence of my landscape evolution model. However, it is worthy to note that CHILD expresses a similar resolution dependence when it was used in detachment limited mode to study the sediment flux response to periodic tectonic change (see the supplementary material to Li, et al., Some signals are not the same as they appear: How do erosional landscapes transform tectonic history into sediment flux records?. Geology ; 46, 407-410). Both detachment-limited and transport-limited models are heuristic and arguably wrong, yet to paraphrase a favourite quote of mine, "all models are wrong, but some are useful".

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