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Comment on egusphere-2022-605

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Referee comment on "Self-organization of channels and hillslopes in models of fluvial landform evolution and its potential for solving scaling issues" by Stefan Hergarten and Alexa Pietrek, EGUsphere, https://doi.org/10.5194/egusphere-2022-605-RC1, 2022

The detailed exposition of the modeling scheme and the results is generally clear. Most comments concern basic questions about the model implantation and definition of channels.

Line 32: If the comment about lack of sediment transport refers to the Howard (1994) model, this attribution is incorrect because sediment transport is considered as are alluvial channels. Even at locations where the channel is bedrock, the sediment is routed downstream and influences the gradient of any alluvial channel segment downstream.

Lines 37, 202: The description of simulations such as Fig. 2 as having "canyon"-like morphology seems inappropriate. It seems that "canyon-like" seems to be conflated with the appearance of a strongly elaborated drainage network. The common usage of "canyon" implies a valley sharply incised into a relatively smooth upland – often implying cliff-like slopes bordering the valley. I suggest using a different term and defining its meaning.

The issue of defining the channel network and, correspondingly, drainage density is not adequately discussed. In natural drainage networks one way of defining drainage density is the presence of actual channels with well-defined banks. This approach is, of course, not useful for landform evolution modeling at basin scale, at least at the level of process generalization in current LEMs. It also suffers in a more general sense that the drainage network so defined is time-dependent, because the channel network can expand and contract with flood events, land use change, and short-term climate changes that do not strongly affect drainage basin morphology as a whole. As the authors indicate, defining the channel system by a critical drainage area, A_c, is arbitrary. There are two ways to define A_c, and each has limitations. The first is straightforward imposing definition of the channel network to initiate at the critical area no matter what the modeled or actual landform processes are or what the landform morphology reveals. The second is to impose a process threshold in LEMs at a critical contributing area. That seems to be what the authors imply based upon discussion and simulations in section 7. There is some logic to this if the threshold is clearly defined as occurring at a critical process threshold, such as a critical fluvial shear stress for channel incision or a critical threshold for hillslope stability. Several studies have explored stochastic forcing of a critical fluvial shear stress (as a result of storm intensity) and its effect on drainage basin morphology. Both uses of A_c are relevant only to simulation modeling and not for determining drainage density in natural networks. The authors implement a more general scheme based upon the slope relationships between adjacent cells which is applicable to LEMs but probably error-prone for analyzing natural drainage basins. This seems to be a reasonable, but not unique approach for analysis of simulation models, although as discussed, it strongly dependent on the flow routing scheme and the use of square simulation cells. A possibly more general and less "noisy" method is to define a critical topographic concavity to define channel heads. Howard (1994), for example, used the gradient divergence divided by the basin-wide average gradient.

Many LEMs utilize both fluvial and slope processes within each cell, with the emergent landscape depending upon the relevant process balance in each cell. The authors criticize this approach without providing specific justification. In actual simulations there is generally a very abrupt downgradient break between cells in which slope processes dominate and fluvial processes are unimportant and the inverse. In fact, in natural landscapes both fluvial processes and mass wasting processes occur on slopes, and individual locations can temporally transition between being dominated by either process, justifying this approach. The relative dominance of fluvial versus mass wasting process determines drainage density (Howard, 1997, EPSL, 22, 211-227; Tucker & Bras, 1998, WRR, 34, 2751-2764) and inferentially channel network definition.

The introduction should be more explicit about the implied issue in LEMs about cell-size dependency of LEM simulations. If processes are scaled correctly, there should not be cell size dependence unless the cell size is too small to adequately represent slope processes and morphology.

Lines 61-96: The exposition here is clear. The use of combined sediment transport and bedrock erosion is fine, although in many natural channel systems transition from bedrock reaches to alluvial reaches is abrupt.

Line 105: The choice of only one downstream cell to define channels is fine for steady state incision, but what happens when channels aggrade and there are multiple downstream potential flow paths (e.g., alluvial fans)? I see that this is addressed as a limitation in Lines 108-110.

Section 2 general comment: The erosion model in this part of the paper only considers fluvial processes. In the absence of slope processes is not the entire network channelized by definition?

The use of A_h to emulate slope processes in what is essentially a fluvial-only model seems arbitrary. The justification seems to be primarily to allow computationally-efficient modeling by eliminating the complexities of explicit modeling of mass wasting. This lessens any general implications of scale independence beyond their specific LEM.

In general the paper is clearly written and illustrated. The conclusions of the modeling seem limited to a specific implementation of their LEM in terms of process modeling, channel network definition, flow routing and grid characteristics.