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Comment on egusphere-2022-605

Anonymous Referee #4

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-RC4, 2022

This paper proposes an alternative solution to identify the channel – hillslope domain in dynamic landscape evolution models. The topic is of interest to the community. In general, the authors can give somewhat more depth to this story by pointing out issues they generally declare and by supporting their statements with literature and examples. Also, the results and findings would benefit from a clearer description at several points.

Title: I do not find the title to be adequate. This paper is not about self-organization of channels or hillslopes but rather presents a new LEM, that is essentially a full-scale fluvial model where hillslopes are represented as a drainage area independent process. There is no backup of any of the findings by field observations and the authors declare themselves that more research is needed to underpin this work and potential consequences. Hence, I would suggest a more technical title like: "A new approach to delineating channels in Landscape Evolution Models."

Line 40. What do you mean with 'a scaling problem'? Please specify. Model components like SPACE (Shobe et al., 2017) have been used in combination with diffusion (Shobe et al., 2017). In theory, all processes should act everywhere on a landscape. Why would diffusion as a process not act over channels and vice versa for fluvial incision? Naturally, at small discharges (drainage area) diffusion would be dominant over fluvial processes. I have been asking myself this question at several points throughout the manuscript and find it critical to address this point. Referring to other work does not suffice since this assumption is at the heart of this story.

Line 63 Add SPACE (Shobe et al., 2017)

Line 70 explain Kd and Kt

Line 125 here diffusion is applied to the entire domain. Just curious how the afore mentioned scaling issues are altering the results here. Aha, it is mentioned in the next sentence I see. Still wondering what those scaling issues are. Also, is the D value dimensionless? How does this compare to actual diffusion values ? (e.g. m2/yr see e.g. (Godard & Tucker, 2021))

Line 150: energetically favorable means less energy, right? Maybe specify to help the readers a bit here.

Line 155. “In turn, we need a model for hillslopes that does not favor dendritic networks energetically” Not sure I understand why not, please explain better.

Line 171: This might be true for the shared stream power model, but in the Carretier solution, a threshold slope is still used to calculate transport lengths. Please specify what you mean exactly.

Line 209. The river is shorter, where? Explain better.

Line 210: belongs

Line 211: 5000. How do we see that on the figure? Catchment A only goes up to 400 (dimensionless?)

Figure 3: Explain in the subscript what Ah is. Makes the figure readable on itself.

Figure 3-6: Are all these findings for non-dimensional values/axes? Please specify.

Line 236: “Owing to the dominance of parallel flow patterns at hillslopes: That is interesting. So, at A<Ah, flow patterns do not organize in ‘energetically favorable’ patterns? Would be good to elaborate a bit on this.

244: Again, it has never been explained clearly what the ‘scaling issues’ and ‘such problems’ are. This is critical to support the value of this work. It does not suffice to point to previous work.

Line 270: Would the authors expect differently when m/n is not 0.5?

Line 272: I find these kinds of sentences of very little added value. I have no clue what is meant here unless I go read this paper. Either explain what is meant or drop the sentence.

Line 278: This paragraph needs some more context to be of added value for the paper. Is the focus on slope breaks, or rather on the orientation of streams? I was expecting to read how this model adjusts the SA plot one expects to see based on observations where a transition from a hillslope domain into a debris-flow dominated into a alluvial channel domain occurs (Montgomery & Foufoulaâ□□Georgiou, 1993). Please elaborate on that. Do we not see any hillslope domain because the model is actually a fluvial incision model where hillslope erosion does not depend on A? Curious to know.

Paragraph 8. As the authors seem to suggest this conceptual model seems to be disconnected from reality. Hence, it should be made clear what exactly the added value of this approach is. Why would one favor this method rather than just assuming continues processes of diffusion and incision (the latter maybe with an incision threshold)? If I would be to use a LEM; I am not convinced I would consider this approach in the way it is described now. Please summarize the benefits of this versus other approaches (other than the Ac method). It would also be good to connect this work to field observations. Yes, it does not work well in its current state, but are there ways to improve this? On a similar note: the authors show different simulations with various values of A_h. Are those values chosen arbitrarily? Can they be set using data or by using DEM-derived topographic metrics?

Line 384. ‘Serious problems’. That sounds a bit suspicious. Explain what the problems are and why they are assumed to be not seriously affecting model behavior.

Line 389: What does it mean, works quite well?

Refs:

Godard, V., & Tucker, G. E. (2021). Influence of Climate-Forcing Frequency on Hillslope Response. Geophysical Research Letters, 48(18), 1–11. https://doi.org/10.1029/2021GL094305

Montgomery, D. R., & Foufoulaâ□□Georgiou, E. (1993). Channel network source representation using digital elevation models. Water Resources Research, 29(12), 3925–3934. https://doi.org/10.1029/93WR02463

Shobe, C. M., Tucker, G. E., & Barnhart, K. R. (2017). The SPACE 1.0 model: a Landlab component for 2-D calculation of sediment transport, bedrock erosion, and landscape evolution. Geoscientific Model Development, 10(12), 4577–4604. https://doi.org/10.5194/gmd-10-4577-2017