



EGUsphere, referee comment RC2  
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## **Comment on egusphere-2022-605**

Anonymous Referee #2

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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, EGU sphere, <https://doi.org/10.5194/egusphere-2022-605-RC2>, 2022

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In this contribution, the authors take on the issue of grid scale dependence in coupled channel-hillslope landscape evolution models. Using a recently published mathematical formulation along with a new definition of what constitutes a channel, they construct an LEM that seems to circumvent some of the potential issues with past LEM implementations: chiefly the issues of 1) needing to define arbitrarily channels versus hillslopes (some previous approaches do this, though many do not) and 2) grid scale dependence in models that couple river and hillslope evolution.

I find the manuscript to be useful to the community given that we are always looking for modeling approaches that allow us to circumvent known issues with our current techniques. I think the authors have been honest about where the utility of their advance begins and ends (i.e., maybe not useful for running simulations on real DEMs), and that they do a nice job of exploring the behavior of the approach they propose. My chief concern about the manuscript in its current form is that it is not well enough integrated into the LEM literature. Readers are not shown clearly and specifically the shortcomings of other approaches, and it is therefore hard to be sure as a reader exactly in what situations this new approach is a major advance. I would like to see this paper published in ESurf after some modifications—to the writing rather than the science—that 1) clarify the position of the current work in relation to the large body of LEM literature, and 2) clarify the utility of the current approach given its intricate relationship to the D8 grid and its current lack of applicability to non-model-generated grids. To be clear, I think this is a useful contribution, but I think the authors could increase their impact by expanding on these points.

For example, the paragraph beginning on line 34 states what I believe is a well-known limitation of Eq. 2, for example Kwang and Parker 2017 talk about this. I would like to see a citation to either that work, other relevant work, or a combination of the two that demonstrates to readers that this is a known issue. Similarly, in the following paragraph (line 40) there is no citation to papers describing the problem of grid scale dependence. Readers are left to wonder whether this is a problem inherent to the SPIM/diffusion model in every case, or whether a model could in theory be scaled correctly in order to avoid it.

Following this discussion, in line 44, it would be good to be more specific than to say that a given approach is “not free of problems.” What are the problems? Are they problems that the new method being introduced will solve, presumably?

Another example is in the paragraph starting in line 45. We miss references to the work of Garry Willgoose (e.g., 1991 papers), who if I am not mistaken was one of the earlier workers to explicitly separate channel and hillslope process representations dynamically. I like the idea behind this new paper (that we can define channels as nodes with one downslope neighbor), but I think the impact will be greater if the authors are more thorough about placing their work in the context of past work. I congratulate the authors on an interesting paper.

Please find line-by-line comments below.

31: I am not sure that the Howard 1994 citation is well-captured by this statement. It might be better to rephrase or to choose a reference like maybe Braun and Willett 2013 where sediment truly is not considered.

33: It would be worth pointing the reader to some of the foundational papers on modern sediment-flux-dependent river incision models, e.g. Sklar and Dietrich, 1998; Whipple and Tucker, 2002; Gasparini et al., 2007; Turowski et al., 2007, as well as the long history of models (some of which are cited later in this paper like Davy and Lague 2009) that have computed the sediment mass balance in addition to calculating river incision. We don't want readers to get the impression that we don't have options beyond detachment-limited treatments.

37: The use of “canyon-like” is unclear here. Do you mean landscapes in which channels become very steep at low drainage area (e.g., Kwang and Parker, 2017)? I think a new phrase is needed for clarity, or the phrase could simply be deleted and the sentence would stand as-is.

41-42: I am not sure all readers will understand intuitively why this happens. Another sentence or two describing why adding hillslope processes causes a spatial resolution dependence would be useful for setting up the problem.

44: Given that the problems associated with coupled channel-hillslope models make a major motivation for this work, I ask the authors to please summarize the problems discussed in Hergarten et al. (2020a) that they reference here.

50: Again here, given that this paper is a separate contribution, it would be good to

restate for readers what actually is the approach proposed by Hergarten et al (2020a).

55: Is it possible to be more descriptive/clear than "weird?" I know that in some cases the issue with this approach is that slope-area data no longer reveal a smooth hillslope-channel transition that is observed in many real landscapes, for example. Are there other specific issues that could be brought up here? Are there citations that could be added that illustrate these issues?

200: It is not clear why these small-scale persistent changes in the topography occur—could a sentence be added to explain more clearly?

202: Here it sounds like "canyon-like" just means "steep hillslopes." I recommend re-phrasing for clarity.

209: This last sentence could use a little more detail to be clearer.

214-215: This feels like a stupid question, but: Why does the erosion rate decrease? Is this only for the case of the chosen fluvial versus hillslope parameter values, or is this universal?

218: Similar question for erosion rate increase. I am having trouble understanding, and I fear readers will too, what dynamics are occurring here. A few more details would help.

Section 5: I find this section very interesting. Do the authors expect the same result when  $m/n \neq 0.5$ ? There are some applications in which 0.5 is a bit of a special value (Kwang and Parker, 2017) so it might be worth checking another ratio.

272: Like many of the literature references throughout, this one is quite vague. Could the authors add an extra sentence clarifying what salient points of that paper are relevant? I for example am aware of Hergarten 2021 but have not read it in any detail, so am a bit lost here.

328: Again it would be good to see multiple references here to demonstrate the extent to which this practice is "established." Certainly this is an assumption in much topographic analysis of real DEMs, but in my understanding of the literature it has not (at least recently) been a favored approach for LEMs. If I am wrong, then that's ok and the addition of several citations will settle the question.

Section 7 in general: I find this discussion somewhat difficult to follow, largely because I do not fully understand why the landscape is given to reorganization even under a largely steady state condition. Some more detailed explanation of the processes causing that behavior would clarify this section.

Conclusions: These in general represent the content of the paper well, but I would also like to see a brief addition (this could also be before the conclusions if the authors prefer) making the case for why and how future workers should take advantage of the advances provided by this paper given the limitations (which are already well-stated by the authors). What can we do with this new knowledge?

Thank you for the chance to review this interesting work!