Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC2
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Comment on esurf-2022-14

Anonymous Referee #2

Referee comment on "Theoretical and numerical considerations of rivers in a tectonically inactive foreland" by Stefan Hergarten, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2022-14-RC2, 2022

This manuscript presents a numerical model and derives a conceptual framework to quantitatively explore sediment transport dynamics and drainage network evolution over large spatial and temporal scales in a foreland setting.

An emerging concept of the study is the division of foreland rivers into carriers and redistributors with well-defined geomorphic functions, long profiles, and sediment transport dynamics. The identification of the two groups of rivers is appealing and has the potential to serve other studies that explore sediment routing in foreland basins.

The manuscript presents only numerical results. This is a valid choice – numerical insights could be highly beneficial even when presented independently from a field, experimental, or previously presented numerical research questions or observation. However, I believe that the current manuscript could greatly benefit from some connection(s) to field observations, illustrating that the new concept of carriers and redistributors is meaningful.

Aside from a few odd phrasing choices (pointed out in RC1), the manuscript is overall well-written.

There are two major issues that I recommend considering, which could potentially increase the manuscript's impact and usefulness.

First, the model is not described in sufficient detail. While the model was presented in a previous study, readers should be able to get the main message of how the model works without needing to consult the previous manuscript. This is particularly true since much of the inferred sediment and drainage dynamics appear to be specific to the model. Equation 1 has two unknowns: E and Q, and additional equations are needed to close the system. Both unknowns can be formulated as functions of local elevation, but this is not currently specified. Boundary conditions are also not fully specified. I.e., how does the model deal with Q at the highest node of each network? Choices pertaining to drainage dynamics are not presented and discussed. I.e., how the model deals with local slope reversals due to sediment deposition? Does the model assume steepest descent to induce drainage change? What is assumed about flow routing? Are lakes allowed? These choices likely directly influence model results, but readers are currently left in the dark.

The model is developed with non-dimensional quantities, and a specific dimensional interpretation is proposed. This is a common practice that works in many cases. However, in the current manuscript, I found that the repeated dual interpretation (dimensionless and dimensional) is confusing. One way to overcome this issue is to formally present the scale factors once (it could also be interesting to see a non-dimensional analysis of equation 1) and from that point on to present the results only with either dimensional or non-dimensional form (the former is probably easier to read). On the same issue: why does it make sense to use two length scales? Why can't the analysis work with a single length scale?

Some of the steady-state and long profile analyses presented in section 6 could be moved to the model description, providing much-needed intuition of model expected behavior.

The second major issue is that I struggled to balance sediment mass and topography across sections 7-9. I assume that the model conserves mass (including sediments deposited in the ocean). However, I could not balance it myself (I didn't try to balance it formally, but just in terms of sources, sinks, processes, and orders of magnitude). Figures 9 and 14 are confusing to me. How come sediments are incorporated from the foreland, but there is no arrow showing sediments being damped in the foreland? How come the percent sum of sediments deposited in the ocean is 100 while both the figure and the text refer to the total sediments as more than 100% (e.g., the 91% of the carriers is out of the 133% coming from both the mountain and the foreland).

Similarly, I'm unsure how to reconcile figures 10, 11, and the sediments transported to the ocean. Carriers' deposition rate is larger by an order of magnitude with respect to the erosion rate of the mountain and by two orders of magnitude with respect to the erosion rate of redistributors. How can that be? Are the carriers two orders of magnitude smaller in area than redistributors? How is it possible that with such a high sedimentation rate, the foreland is not growing in topography but reaches a statistical steady state? How can this be reconciled with figure 9?

Perhaps a formal statement (with equations) of mass/volume conservation could help out here, clarifying which component out of the mass balance each figure analyzes?

Line comments

Line 1 - First sentence of the abstract reads a bit detached.

Line 2 – 'Fundamental properties'. At this stage, only fundamental numerical properties'.

Line 100 – Not sure there is a need to mention simulations that are not presented here.

Line 121 – Perhaps ‘representation’ instead of ‘coordinates’.

Perhaps a more informative title to section 2.

Figure 3 – Maybe variance would be a better description than relief.

Figure 5 – Maybe also show some profiles?