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Comment on esurf-2022-14

Jean Braun (Referee)

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-RC3, 2022

Review of “Theoretical and numerical considerations of rivers in a tectonically inactive foreland” by Stefan Hergarten

Dear Stefan,

I have read your manuscript with great interest. It contains some very interesting results. In particular, I appreciated your division of channels in the foreland into carriers and redistributers. These are indeed very useful concepts to understand most of the dynamics of the quasi-steady sedimentary system you study and that you labeled a “tectonically inactive foreland”. The analysis of the concavity of channels is very interesting and novel, the dual role played by the redistributers as well as the source-to-sink description of the system are all facilitated by the new nomenclature.

Although I support the publication of this material in ESURF, I express below some concerns that I have about the presentation of your results, their robustness and their applicability.

- The objectives of the manuscript are relatively well explained and certainly interesting for those of us that like to play with equations. I find, however, that you could improve your introduction by relating better your objectives (last paragraph of the introduction) to questions that are asked by sedimentologists, geomorphologists. Similarly, I believe your paper would gain much in its impact if you were to come back to these questions (and how your work has contributed to their resolution) in your discussion.
- Although the basic evolution equation has been presented elsewhere, I believe it is important for the comprehension and the flow of the manuscript to at least present the

basic PDE that you are solving. Even though you focus on the quasi steady-state solution, you must solve an evolution equation, most likely expressed in terms of the vertical elevation of the topography as the main unknown. It is also important to give the form of this equation in the case where Kd tends to infinity, because this is the form that you have used for most of the results presented here (in the basin). Am I right in assuming that it then takes the form of a non-linear diffusion equation? For both the mathematically-oriented readers of your manuscript and the sedimentologist who might be interested in interpreting your results, it seems important to me that these equations (the full form and its asymptotic form when Kd tends to infinity) be presented.

- Although I fully support the need to use dimensionless variables when presenting model results, I do not agree with your approach to quote absolute values for basic parameters such as K or grid size and derive other length scales and time (or rate) scales out of it. I believe it would be much more useful to explain with some simple relationships how the dimensionalisation should be done, i.e., how one could apply your dimensionless results to a problem of known size and rate.
- In the model description, you mention that the algorithm you use is implicit and thus unconditionally stable. You do not, however, assess its accuracy, which we know must depend on the time stepping (and grid spacing). Can you please provide us with an estimate of this accuracy. I am concerned (see point 6 below) that the solution might be dependent on the step size. If your results are applicable to natural systems, which are characterized by finite avulsion rates, the model should be characterized by a characteristic time for channel geometry to change. I believe that you need to check whether the time step you are using is smaller than such a characteristic time for many of the conclusions you draw to be correct.
- You note that the foreland is made of two parts (a fan and what I will call an alluvial plain connecting the fan to the ocean). You also note that the behavior of the system is rather contrasted in these two sections. So what controls the size of the fan becomes an important factor in describing the system's behavior. I recently demonstrated with a 1D version of a model identical to yours that it is the size of the mountain catchment area that controls the size of the fan (regardless of the value of Kf). This implies that the setup you have used (with a very small mountain) leads to a relatively peculiar situation that might not be representative of many forelands. May I suggest that you test the robustness of your finding against the size of the fan (by changing the size of the mountain area). It might lead to very similar results with a simple shift of some of your curves (as shown in Figure 10). But it might not. Furthermore, some of the numbers you quote in your "source-to-sink" section may be quite different for a different relative size of the fan.
- Let me now come to my main concern: I found the part concerning the time scale for drainage reorganization very interesting. However, I do not know how to interpret these results to understand how real (natural) systems behave. I am particularly concerned about how the spatial and temporal resolutions of your model experiments might influence your results. I think this needs to be investigated for your results to have the impact they deserve. As channels have no width, there is a possibility that you might not be able to extract an avulsion time scale out of the basic equations, in which case many of the results you present (for the time evolution of the system in its quasi steady-state) might be difficult to use to interpret natural systems.
- Another point of concern is your use of a single direction flow routing algorithm, which you should try to be better justify in your method description in a low slope system/environment controlled by continuously evolving states of deposition and erosion. Such natural systems are often characterized by non-dentritic channel networks with flow splitting occurring as often as flow merging.

I also have some minor comments on the presentation of your results:

- Line 182: I am not sure about the use of the term “black and white scenario” nor to what it corresponds to.
- Figure 9 is not clear; you have two sets of arrows leaving the foreland into the ocean; I believe one must be associated to carriers and the other to redistributors. This should be indicated somewhere (in the caption?). Also I am not sure to what corresponds the set of vertical arrows in the foreland? In a steady-state solution, on average the flux in and out of the foreland must be nil.
- Please make sure that figure 13 that uses the classification of “regions” in the foreland as described/shown in figure 1 has a reference to it in the caption.

I hope you will find these comments and suggestions useful and hope to see a revised version of your manuscript.

Regards

Jean

Jean Braun