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Interactive comment on "Long-term Morphodynamics of a Schematic River Analysed with a Zero-dimensional, Two-reach, Two-grainsize Model" *by* Mariateresa Franzoia et al.

Anonymous Referee #1

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Summary: This manuscript develops a simplified physically based model of the evolution of a river profile with two distinct reaches and grain size fractions. A subset of the potential parameter space for the model is explored through the presentation of six morphologic parameter combinations.

General comments: The manuscript would benefit greatly by connecting with the rich literature on landscape and river evolution models and showing why the current approach adds something new to this discussion. As it currently stands it is difficult to discern what the novelty of this contribution to the literature is.

Currently, the model demonstrates that the long-term equilibrium state is one of no concavity (one slope) and thus suggests that because rivers are commonly concave

they must not be at an equilibrium state. However, I remained unconvinced that the model actually represents the mechanics or even physical representation of an actual river and rather seems to approximate the profile and set up of a sediment feed flume, which does indeed capture aspects of a river profile but leaves out important details. In that the final state of the model captures the expected behavior of a two size fraction feed flume (see discussion in Parker and Wilcock, 1995). A key missing ingredient in connecting with natural upland or mountain catchments in the current set up is the lack of a connection between the profile and the supply of sediment. In the current set up (with constant sediment supply) the river profile must always evolve to a state where it can transport the input sediment and thus needs to evolve to a state where it can always transport the coarse sediment which indicates that the final equilibrium slope always has to equal the upstream segments final slope. Such a constraint limits the types of landscapes that this model could be applied to (i.e. landscapes that approximate flumes). I strongly encourage the authors to consider how their model assumptions have dictated their final equilibrium states and that these states may not be applicable to many natural landscapes. Or I encourage the authors to consider narrowing the scope of the conclusions and discussion to reflect the conditions and limitations within their model framework.

This manuscript would benefit from the use of an english language editing service.

Specific comments:

P. 2 Ln. 7 - Flattening rather than flattering, unless the river has indeed paid you a complement.

P. 2 Ln. 5-7 - The authors would benefit from reading all and working into the introduction some of the following papers on the sorting of particles and how it links to the slope and long profile of a river (Seal et al. 1997; Paola and Seal 1995; Cui et al., 1996; Fedele and Paola, 2007; and especially the following two Paola et al., 1992a; and Paola et al. 1992b). Many of the questions posed in this introduction are addressed in these works.

P. 2 Ln. 14 - Be sure to update the citation to the Metivier et al. 2016 work (the work is now published).

P. 2 Ln. 14-20 - These lines seem a little out of place with the narrative, especially the ideas of Davis at the end of the paragraph. The end morphologic state of an alluvial river under constant flow would be one where sediment transport has ceased and the channel is everywhere at threshold (Lacy's Law, from Metivier et al., 2016, cited earlier in the text), not something with no slope (flat).

P. 5 Ln. 17 - Why are rock banks and geology used as constraints for a narrower upper segment when the model apriori assumes no such natural complexities. It would seem fine given the assumptions made earlier to just state the model set up (in figure 1) as the initial conditions rather than justifying the conditions via externalities the authors have sought to avoid.

P. 8 Ln. 2 - It is not clear why at equilibrium the bed grain sizes and slopes should be equal for both reaches. This would seem to preclude the system to be able to sort the coarse and fine sediments, which seemed to be the reason behind having a two reach two grain size model.

P. 10 Ln. 8 - It would make more sense to write '... in describing the evolution of the model' rather then the 'river'.

Figure 2. This figure takes awhile to fully comprehend and would benefit from a new figure showing the evolution of the river profile at different time points. Additionally it would help to change the x-axis label from t* to log(t*).

P. 11 Ln. 27-29 - Don't the longterm implications of the model show that there is no concavity (i_U = i_D)? How does this model relate to natural rivers that are concave? It is stated later in the conclusions that an implication would be that because rivers are concave they must not be at their equilibrium state, but this seems to be a bold

conclusion based on the many simplifications present. Please consider an alternative possibility that the longterm implications of the equations are not indicative of an actual river, but may represent a sediment feed flume where under constant discharge capable of transporting all size classes (as in the model here) the resulting end state will have a single slope (to a first order) and one bed surface composition (see discussion in Parker and Wilcock, 1993). Consider also the work of Guerit et al., (2014), particularly figure 9 where their 1D river (alluvial fan) which would very closely approximate a 1 reach 1 size fraction version of this model still displays concavity at its final state and the physics of the argument suggest that the concavity is related to sediment transport.

P. 11 Ln. 31 - Figure 2 does not seem to support the assertion that Tfill is the most important morphometric parameter determining the longterm river evolution. In Figure 2 the various runs do not converge at Tfill=1 (log(Tfill) =0) or display any change in behavior (no inflection points or maximal/minimal values). From Figure 2 it seems that the various runs converge on their final equilibrium behavior near Tfill =10. Some discussion should be added here as to why it takes 10 times the time needed to fill the space to achieve the equilibrium values. If Tfill is a better predictor of other important parameters, those parameters should probably be added to an additional figure that supports the assertion of its importance.

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