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Earth Surface Dynamics Discussions

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 #2

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In this submission, Franzoia et al. present a mathematical framework within which it may be possible to analyse the evolution of experimental and natural alluvial rivers. They invoke a suite a simple, analytically tractable hard assumptions about a river system to perform the analysis, of which the principle are: 1. The system is chute-like, 2. The system is driven by constant, time-invariant inputs of water & sediment; 3. Mechanical abrasion of the transported sediment is negligible; 4. A local uniform flow assumption, i.e., that the bed slope is the energy slope of the flow, and 5. That the system is fully alluvial, i.e., the Exner equation always applies. The derived set of analytical expressions is then used to assert that the system described tends to a planar profile, and that this response occurs over a well-defined timescale related to

the sediment volume flux and the initial (and boundary) conditions. The manuscript concludes with a sensitivity analysis performed as a set of six numerical case studies examining the effects of modifying various of the driving parameters and boundary conditions. They suggest that all six share a common response pattern of a rapid initial transient response, a drawn out, quasi-equilibrium period during which the profile is concave and fines downstream, then a slow approach to the linear condition and zero fining.

I struggled with the assumptions underlying this manuscript, and so cannot recommend it for publication in its current form. Because the approach is purely analytical, the authors are forced to make strong assumptions about process and form in order to render the equation system analytically tractable. Each of these in isolation could be regarded as questionable when applied to a real river system; together, they feel like an unacceptable stretch if describing a real river (see below). These concerns would be lessened if the framework were explicitly applied to an experimental setup (only), where at least the validity of the various assumptions in the experiment could be more easily tested or ensured by the method. However, this leads to the main, overarching weakness of the text as presented: it remains totally ungrounded in physical reality, be that the field or experiment. Without this element in the work presented, a reviewer can critique only the hard assumptions in an abstract sense, and on this basis it has proven challenging for the authors to defend this framework as it stands.

With that in mind, I would specifically question the following:

1. Early in the introduction, the authors dismiss out of hand the role of abrasive fining in alluvial systems (p2 lns4-7). This statement is – bizarrely – purely asserted, and lacks any citation to support what is definitely a controversial interpretation of the literature on this. My reading is that the relative contributions of abrasion and size-selective sorting remains very much an open question – see e.g. various work by Attal, Lavé and coauthors, including a piece in last week's (27/4) Nature by Dingle and colleagues. This assumption may be justifiable for an experiment, but the paper seeks explicitly to

link the framework presented to field examples of rivers.

2. P2, lns 12-16: The comments about necessary disequilibrium in concave natural rivers. This is a very hard assumption, and is model driven (i.e., it is a hypothesis emerging from assumptions made in deriving this and similar frameworks; it does not follow immediately from physical reality). It, too, is uncited. I appreciate that this is the conceptual framework that these authors wish to push as important both in this work and the sequence which precedes it, but here it is being presented as an obvious and unarguable observation, rather than the somewhat circular assertion it appears to be. This impression is aggravated by the absolutist language around the claim (e.g., “cannot” at ln 13, “perforce” at ln 12 in the abstract). This language needs at the least to be shifted out of the abstract and intro and into the Discussion. A wider discussion of whether this is a plausible hypothesis for real rivers is probably appropriate, if the manuscript retains its focus on real rivers.

As a related issue, I note that today (3/5) a manuscript appeared in the Int. J Sed Res by two of the same authors (10.1016/j.ijsrc.2017.04.002) which seems extremely similar in its approach to this manuscript (sharing the bulk of its equations), but the conclusions seem quite different. In particular, in that work, the authors indicate that the similar 0-D, two reach, two grainsize model tends towards a stable, downstream fining, concave profile. Subtle differences are present (e.g. a quick scan reveals differences in handling of channel width, grain size distributions, and hiding functions), but given the shared authorship, difference in resulting conclusions, and very similar approach, I think a discussion of where and why differences appear between the manuscripts would be appropriate.

3. The influence of stationary water and sediment inputs. This I think gets at the heart of the field vs. experimental issue underlying the paper. Many experiments are run under these conditions, and so arguably the presented model would be justified narrowly for such experiments. However, this is manifestly not a valid assumption for real rivers, and indeed, has been invoked within a variety of papers (including a clutch cited in the

introduction) as key in stabilizing and driving field-measured concavities. The lack of tributary input is also troubling in this model for the same reason. This is very much a hard assumption; it's not possible to say a priori under the analysis presented here what effect relaxing it would have. However, we can hypothesise based on previous studies, and indeed first principles, that time variant discharges must have an effect. In particular, they must alter assumptions around time invariant hiding functions and other parameters (e.g., Equ 3), and surely affect the rationale presented at the top of p 5, where the authors explicitly argue for a fully equilibrium system at a “seasonal or shorter” timescale.

4. A related issue is the invoking of the LUF assumption. I agree that this is an elegant and useful minimal complexity approximation of the SWEs, but it remains just that – an approximation. Under the assumption of constant discharges it seems fully applicable (as the system would seek hydrologic steady state, as described), but if we were to relax that assumption, it's not clear to me that this would remain appropriate. This condition is known to suppress instabilities in fluvial systems (see e.g. work by Smith & Bretherton), and is perhaps the driving assumption behind the tendency of these models to seek a perfectly linear profile. But, as I say, who knows until the assumptions are relaxed and/or they are shown to be meaningful in reality.

A number of more minor issues came up during my read through. These essentially fall into three broad classes:

1. For a manuscript with so much detailed mathematics, the authors could be considerably more generous in talking the reader through the derivations and how various observations made arise from the math. For example, hiding-exposure coefficients could be a vital part of this story, but they are not explained in any detail, nor is their formulation as a constant term (? – if not, what is the justification of its disappearance into constants in later equations?) really justified. Another example might be the key result that the profile tends to a constant gradient (e.g. line 1 p 8), which appears to be made by observation from a suite of 8 differential equations. Another might be the

complex statement of how V_inf is defined at ln 10 p 8, which is extremely opaque – until one recalls the much clearer statement of what V_inf actually "is", drawn from a previous paper, presented pages back in the introduction. There are many examples. Please be less terse. 2. The English used can sometimes be convoluted to the point of inhibiting understanding. Most frustratingly, this seems to be worst in the abstract (e.g., "perforce", "giving reason of", but it is pervasive also. Much of the language also seems unnecessarily jargon-y (though technically accurate), especially sitting alongside the dense mathematics (e.g., "granulometry" where "grain size" would serve; "barycentre"; "quasi-equilibrium"). Plain English and well defined technical terms would help a lot here. 3. From my background, many of the terms deployed in the derivation are not using the Greek or Latin symbolology I would usually expect (e.g., slope is I not S; concavity is X not theta; sediment transport rate is P not Q_s, etc etc). If the authors have good reason for these formulations then fair enough, but if they are not following a standard scheme then they should at least consider moving to the more geomorphically typical symbology for clarity for the reader. Also, as a side note, I see that some of the symbol definitions are imprecise (e.g., P_bar is "averaged sediment transport", when this is more precisely a time- and reach-averaged sediment discharge). Please check them, as this can be very frustrating when trying to follow dense mathematics.

In summary, the key issue I have with this manuscript is that it contains a number of hard assumptions that cannot be uncontroversially justified when applied to either natural or experimental rivers. This could to a large degree be remedied by adding material to the manuscript explicitly making these comparisons, either experimental or natural, but I don't think the manuscript can stand without this. I imagine this would be readily possible in the case of a comparison to experiments, but not straightforward for real landscapes.

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