Reviewed manuscript: Modeling memory in gravel-bed rivers: A flow history-dependent relation for evolving thresholds of motion

Manuscript Authors: C. Masteller and colleagues

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Summary and Contribution:

The authors present an empirical model which describes the discrete time evolution of the critical dimensionless Shields number (similarity-based threshold for sediment transport; note that I use "number", "condition" and "stress" below in an interchangeable way) using a unique field data set of streamflow discharge and sediment transport with the latter measured using an impact plate system installed within the Erlenbach located in the Swiss Prealps. The data are unique because sediment transport is quantified directly over annual hydrographs (along with observations of streamflow) with the impact plate system rather than using wading-type measurements combined with rating curves or transport functions for times lacking observations through measurement. As a result, the authors data permit them to identify streamflow conditions associated with the onset of bedload transport, removing substantial uncertainty in estimation of the critical dimensionless Shields number. The empirical model builds from prior work (Johnson, J.P.L., *Earth Surface Dynamics*, 2016) with revisions to the original model focused on describing the discrete time dependent behavior of the critical dimensionless Shields number in terms of so called "strengthening" and "weakening" processes. The model faithfully (and impressively) captures annual trends to the critical dimensionless Shields stress based on the available records during years when "strengthening" dominates and/or "weakening" events do not occur, with more variable performance for years when "weakening" dominates. To my understanding the authors model is the first of its kind in the geomorphic literature, and I congratulate them on their excellent work.

Based on several readings of the manuscript, I believe some effort is needed to address the comments and suggestions I raise below. I do not have any serious criticisms of the authors work, however, there are some conceptual points (bigger picture comments) that would benefit from thought and discussion in the manuscript. I think the manuscript is well written, and the figures nicely developed such that they add important illustrative dimensions and information to the narrative of the text. I appreciate the authors inclusion of discussion elements which add some critical review of the proposed empirical model making it clear that they have thought about their approach and results from a variety of viewpoints. Some specific examples of the points raised below include consideration of bedload transport in rivers as a "nonlocal" process which in part helps set the context of the measurement system as well as the fact that the authors calculation approach mixes time and space considerations, and additional discussion focused on the nuances around the interdependence of (in-channel) sediment supply and the critical dimensionless Shields condition. I also have a few questions around the parameter *Beta* that would benefit from clarification, most importantly, I cannot recover a (0,1) behavior of *Beta* with the form of Eq. 2 presented in the manuscript (I get negative values).

I want to thank the journal editors and authors for the opportunity to review the submitted manuscript, and for your patience as I completed my review. I hope that my comments are helpful to the authors.

Bigger Picture Comments (in no particular order)

1. **Transport as a nonlocal process:** There has been important work completed that illustrates how bedload transport in rivers is a non-local process, i.e. that the transport (activity in the case of the authors work) of particles close to the bed surface as measured at any particular point along a river profile *x* and at some time *t* is a function of transport processes upstream of *x* (within some finite distance set by the *pdf* of particle travel distances) and for some finite time interval prior and leading up to *t* (within some finite time interval set by the *pdf* of particle travel times; see Furbish et al., *GBR*, 2017 for a clear discussion; see Foufoula-Georgiou and Stark, *JGR*, 2010 for a broader discussion). The implications of bedload transport as a nonlocal process has relevance to the authors work in a number of ways: (1) it provides context for interpreting the time series of particle impacts at the Erlenbach measurement site [and hence time dependency of the dimensionless Shields stress and the critical dimensionless Shields stress] by offering a conceptually useful way to understand the authors "noisy transport data" as more of an expected outcome based on the explicit dependence of transport on time and space; (2) it offers a broader perspective to conclusions reached regarding deterministic variability of threshold evolution; (3) it elaborates the context for the authors concept of "memory" in that nonlocality provides a more concrete way to frame the authors proposal that memory "integrates the effects of the past history of both flow conditions and channel bed conditions" (lines 253-254, page 12 of the manuscript; although I also recognize that the authors idea of memory involves much longer time scales); and (4) it helps to better frame the authors calibration and application of Eq. 1 to the Erlenbach data because the authors approach mixes space and time effects, and a nonlocal perspective naturally reflects these two aspects of transport processes. In summary, the authors relate time variations of the critical dimensionless Shields number to conditions upstream of the point of observation. A nonlocal perspective of bedload transport can help the authors, to some degree, make these points in more concrete ways.

2. **Equation 1 and *Beta*:** In section two the authors discuss their model for the time evolution of the critical dimensionless Shields condition. I think this section will benefit from more direct discussion of the development part of the model. As it stands the section explains the components of the model and why specific elements, etc. are justified, but their discussion does not provide much detail on the model development side. For example, if someone else attempted to recreate the authors model working from the existing text and supplemental information alone, do they have enough information to guide their thinking and decision making to eventually lead them to the form of the model presented in Eq. 1? Basically, what were the key steps or decisions made that led the authors to the present model formulation. To be clear, I am not suggesting that the authors list out every decision made along the way to the formulation of Eq. 1, but rather they provide enough information to better understand the key steps in getting there. Perhaps this can be addressed by prefacing the presentation of Eq. 1 by stating explicit hypotheses or specific ideas that underpin the authors thinking (this is briefly done at the end of section one but I am suggesting it is done in relation to presenting the proposed empirical model). Among other reasons, providing more clarity around model development will help future readers as they attempt to apply and test the model to any particular circumstance. With this in mind, it may be helpful to write out the discretized form of Eq. 1, or specify the time marching components of Eq. 1 with notation so it is clear how the initial and time dependent values are specified in the calculation procedure.

   The present text lists *Beta* (Eq. 2) as a ratio of the differences between the max/min critical dimensionless Shields stress and the time dependent critical dimensionless Shields stress, respectively. Based on my reading of the manuscript I have assumed that the authors used max and min values of 0.36 and 0.036, respectively, for all associated calculations (Fig. 1 caption). However, I am not sure if this is the case. If this is true and using the form of *Beta* given in Eq. 2, it seems that *Beta* should be negative at times when the critical dimensionless stress > the min dimensionless critical Shields condition (0.036?), and < the max dimensionless critical Shields condition (0.36?)--for example if the critical dimensionless Shields number has a value of 0.05. However, the authors state that *Beta* ranges from a value of 0-1 (lines 99-100). What am I missing? What were the values of the max and min dimensionless critical Shields stress used in the calculations of Eqs. 1 and 2? Did they change in time? Why is the authors form of *Beta* different from the form given by Johnson, 2016 (Johnson, J.P.L., Earth Surface Dynamics, 2016)? Were the max and min values calculated with the power law forms given after Eq. 2? If so, what was the value(s) of *S* used (based on the min/max values given in the Fig. 1 caption, the power law forms of the min/max suggest *S* ~ 0.11)? Also, based on the form of Eq. 2, it is difficult for me to imagine how *Beta* takes a value of 1 because the max and min values of the critical dimensionless Shields number by definition will never be equal. Clarification around *Beta* will be helpful.

Last, the supplemental information provides important information related to calibration of Eq. 1. based in part on the work of Paphitis and Collins, 2005 (Paphitis and Collins, *Sedimentology*, 2005). It is important that this information is presented in the main text because it is key to calibration of the *Gamma* exponent of Eq. 1 term 1, and second because the referenced work was conducted experimentally using sand sized particles, which diverges from the Erlenbach field conditions.

3. **Critical Shields condition:** Conceptually, I stumble over how to disentangle the critical dimensionless Shields condition, the dimensionless Shields condition and the sediment supply. The critical dimensionless Shields condition and the dimensionless Shields condition are derived quantities that are calculated based on specific information (e.g. the authors estimation of the dimensionless Shields number and the critical dimensionless Shields number relies on an empirical rating curve relating streamflow discharge to the local average dimensional shear stress [Yager, E.M., PhD thesis, 2006]). Meaning, it is not possible to directly measure a critical dimensionless stress or Shields condition. The authors record of particle impacts removes a substantial degree of uncertainty related to when transport begins in the monitored section of the Erlenbach. However, the principal metrics are still subject to calculation. On the other hand, sediment supply delivered to some position *x* and for some time interval $t+\Delta t$ is a tangible thing, something that given adequate technology, etc. can be measured and quasi-directly quantified (or at least approximately so) because it is a physical response. In rivers, for example, the diligent and careful use of sediment baskets and similar passive measurement apparatuses situated in the streambed can provide a reasonable record of supply magnitude, grain size composition and the rough time interval over which a basket fills (e.g. Hassan and Church, *Water Resources Research*, 2001). At several locations in the manuscript the authors state or use other studies to suggest that sediment supply influences threshold evolution (lines 43-44), and in turn that supply depends on thresholds (lines 58-60). I understand conceptually how thresholds and sediment supply are inter-related, noting that "sediment supply" can mean a couple different things—i.e. in-channel storage, hillslope derived, and so forth. It would be helpful if the authors developed an expanded discussion of sediment supply vs. thresholds in which they more carefully step through the nuances of how these are inter-related and inter-dependent, and what is explicitly meant by sediment supply in the context of the manuscript. The last paragraph of section six could be

suitable to expand the discussion, although it would be more impactful to have this presented in section one as the reader will have a clearer picture when reading the remainder of the manuscript.

4. **Bedload transport and threshold conditions:** There are discussion elements which frame bedload transport around threshold conditions, inter-connections between these conditions and gravel-bed river geometry (e.g. lines 22-23; lines 222-224) and the nature of threshold conditions during floods or transporting events (e.g. lines 22-23, lines 38-39, lines58-61). In several cases there is important literature missing from the discussion. For example, ideas around bedload transport, transporting floods and gravel-bed river geometry have been the subject of significant field-based data collection efforts, some of which provide results not as definitive as that suggested in the present form of the manuscript. Whiting et al., 1999 (Whiting et al., *GSAB*, 1999) present a comprehensive dataset based on hundreds of bedload measurements across more than 10 headwater rivers which suggests that gravel-bed river geometry is maintained in the Idaho batholith (a snowmelt dominated system) at flows less then bankfull (~0.80 bankfull), with the common 1- to 2-orders of magnitude variability in the sediment-flow rating curves. There are many other examples in the literature as well (that I know the authors are aware) which suggest that bankfull flow is not necessarily the most important flow in maintaining channel form (geometry). I raise this point because the subject is the matter of significant debate in fluvial geomorphology, and presenting a more balanced picture of what the literature suggests seems appropriate.

<u>Minor and Editorial Comments</u>

Lines 16-17 (comment also relates to lines 237-238). The sequence, timing and magnitude of significant precipitation events and heatwaves (both of which cause floods) are stochastic. Because “weakening” events are directly linked with floods, the authors conclusion that weakening events are more stochastic than strengthening ones is clear. Use of the phrase “...suggests that flood-induced bed weakening is more stochastic and less predictable then strengthening.” is a little confusing. Are there additional mechanisms not related to flood events (this would include mass movements, etc.) that could cause weakening? I guess I am unsure whether this is a surprise or unexpected from the authors point of view? I think the authors specifying “more stochastic and less predictable” is what is causing my confusion. Also, what does “more stochastic” mean?

Lines 22-23. What do “close” and “floods” mean? Can the authors be more specific?

Lines 23-26. Blom et al., 2017 conclude that the influence of climate change (and hence extreme floods) to river geometry in the zone downstream of the hydrograph boundary layer and upstream of the terminal backwater zone may be negligible (page 19 of Blom et al., 2017). How does this fit into the concept of mapping “climate onto fluvial processes”?

Lines 27-29. I had to read this sentence a few times to understand it. Can the authors re-phrase for clarity?

Lines 57. Minor point: Do all sediment pulses cause disequilibrium? Sediment pulses are commonly of short relative timescales. Ideas around disequilibrium can be associated with longer relative timescales. Pulses can disrupt local bed elevations, grain size populations and hence local transport rates—this fits at least two ideas of disequilibrium. But pulses can also fall under the concept of dynamic equilibrium. I am wondering if/how disequilibrium as used in this sentence differs or is similar to the use of the same word in the following sentence?

Lines 58-70. I think the work of Moog and Whiting, 1998 (the authors include this work in their references) is relevant to the discussion here. The key from Moog and Whiting relates to hysteresis and their data which suggest that prior to the occurrence of the estimated transporting flow each season, there was higher bedload transport for a given flow than afterward. This trend was attributed to limitation or exhaustion of in-channel sediment supply as the snowmelt hydrograph progressed. This comment in part relates to my "bigger picture" comment above related to sediment supply and the Shields condition.

Lines 165-166. How do you know that certain data are "outliers"? Does the concept of an outlier make sense in an inherently "noisy" system? Even time series of flux or transport in the most simple of experiments is, for example, noisy (for example, see Fig. 6 of Ancey et al., *Physical Review E*, 2006).

Lines 179-186 – Figure 2. Nice figure with lots of great information. What do the black dots in the lower panels represent? I read carefully and could not find mention of what they represent.

Lines 219. "...higher *relative* transport capacities"?

Lines 228. There is a missing word towards the end of the line.

Lines 257-258. The sequence, timing and magnitude of future floods is a stochastic phenomenon dependent on future climate conditions, etc. I believe it is generally held that future conditions can be "projected" (not predicted) when there is a dependence on climate-related phenomena because of the probabilistic nature of the problem. Perhaps I misunderstand the intent of the sentence?

Lines 258-262. I encourage the authors to provide a more comprehensive discussion of how their model of the critical dimensionless Shields stress can be applied in other circumstances, with particular details related to what data, at a minimum, are necessary to locally calibrate their Eq. 1. I think data additional to a high-resolution discharge time series is necessary.

Lines 263-265. I don't understand how "transport disequilibrium" influences transport rates? My understanding of the idea is that transport disequilibrium depends on how observed transport compares to calculated transport (Rickenmann, D., *Water Resources Research*, 2020).

References not in the manuscript:

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Foufoula-Georgiou, E., and C. Stark (2010), Introduction to special section on Stochastic Transport and Emergent Scaling on Earth's Surface: Rethinking geomorphic transport—Stochastic theories, broad scales of motion and nonlocality, *J. Geophys. Res.*, 115, F00A01, doi:10.1029/2010JF001661.

Furbish, D. J., Fathel, S. L., and Schmeeckle, M. L.: Particle Motions and Bedload Theory: The Entrainment Forms of the Flux and the Exner Equation, in: Gravel-bed rivers, Wiley-Blackwell, 97–120, https://doi.org/10.1002/9781118971437.ch4, 2017.

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