Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/esurf-2022-47-RC1, 2022

Comment on esurf-2022-47

Anonymous Referee #1

Referee comment on "Steady-state forms of channel profiles shaped by debris flow and fluvial processes" by Luke A. McGuire et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2022-47-RC1, 2022

The sharp slope inflection in the area-slope relationship observed for low-order channels has long been documented in worldwide topographies, and has been explained as due to dominant debris flow erosion. However, the controls and magnitude of channel head erosion by debris flows have been addressed in a limited manner: direct field measurements of erosion are almost non-existent, and models are also lacking. McGuire and co-authors attempt to address this second issue by proposing two separate models based on several parameters, and then explore the sensitivity of these models to their different parameters. The main constraint to validating their model is the qualitative observation that the slope of the upper part of the channels increases slowly but continuously to the head of the channel.

The subject matter of this research is appropriate for the journal Earth Surface Dynamics, and the proposed approaches could represent an interesting first step in the direction of quantifying the role of debris flows in landscape evolution. Unfortunately, the way the study is designed and the paper is written does not really accomplish this goal. I would actually suggest that the authors completely reconsider the organization of their paper, including a modification (or deletion) of the process-based model to include the first-order components that are not considered and yet have a major impact on the shape of the inflection discussed in the preamble.

First, as long as the only proposed validating constraint is the observation that "the slope continues to increase or remain constant as drainage area decreases…", the whole process-based model, as it is proposed, should be rejected, even if for very high value of alfa this increase becomes so attenuated that it could be confused (in the sense of the R2 criterion used by the authors) with a uniform slope not depending on A. This left me a little confused!

A preliminary simplified analytic exploration of the empirical model (which is very briefly

and incompletely done on line 381) would have permitted the authors to identify the important ingredients for the model, by indicating quite clearly what controls the area-slope relationship along the channel heads (i.e. for A < Adf). Considering the erosion equation (eq. 4), and equation (13), the expression for "tp" (eq 14) and assuming as a first approximation that capital theta is uniform and close to 1 (shear stress >> tauy) along the head channel reaches, we can derive such relation at steady state.

S depends on uplift and drainage area according to a power relation: S proportional to A^N, with

N = -(beta/3.(gamma.c2 – b) + gamma.(1-c2))/(alfa – beta/3)

The sign of this exponent N controls the slope of the S=f(A) relationship for channel heads. For explored values of alfa >= 2, the denominator is always positive so that the main controlling parameter appears to be the numerator, and among others the sign of (gamma.c2 – b) modulated by beta. We immediately see that if this term is positive then the slope is constantly decreasing downstream. In contrast if this term is negative then the downstream slope increase observed on fig. 2 and 3 becomes possible. If c2 is fixed, the most fundamental parameters are gamma and b. In other words for gamma =0, for the empirical as well as probably for the process based model, the downstream decrease in erosive efficiency (for a uniform channel slope) is due to the channel widening (parameter b) that induces a decrease of the flow thickness h. If gamma is not zero, then there is a critical value for which the slope trend reverses (for gamma larger than b/c2 according to the authors, but in fact for gamma larger than beta.b/3/(1 + (beta /3-1).c2)).

Once these two main influences on the sign of the slope of S=f(A) have been identified, the one due to the widening of the channel and the one due to the increase in the volume of debris flowing downstream, we must ask ourselves:

- if they correspond to a reality;
- if so, if they are well taken into account by the model:

To the first question, one can notice that the parameter b is given for the fluvial domain, but that its implicit transposition to the debris flow domain, as done in this article, has no theoretical or empirical basis. Insofar as this debris flow eroded domain behaves differently from the fluvial domain, the transposition is difficult to justify. That an important trend in the model results is controlled by a parameter that is unknown and uncalibrated is quite problematic. Furthermore, as described below, the assumption of w>>h (although not made explicit in the paper) and of a rectangular channel is another problem associated with how w = f(A, M) can be injected into the equations. Regarding the increase in the volume or frequency of debris flows downstream, one need only look at Fig. 2 of Stock and Dietrich (2003) to see that the number of debris flow sources in the

contributing basin of a given point will increase downstream, more or less in proportion to the area drained. Taking this increase into account is essential to any model looking at the long-term evolution of the channel profile.

To the second question, we can notice that the process-based model does not include this essential element. This poses a double problem: firstly if we want to compare the performances of the two models, the boundary conditions must be the same (the addition of sediment downstream can be seen as a boundary condition), and more importantly if it is an essential element to the results, it must be implemented (below, I suggest to the authors a quick way to take it into account without needing to modify the core of their equations). In other words, the process-based model is unnecessarily complicated in some aspects while it does not include first order elements.

Given these two deficiencies, it seems to me that the proposed models are for the moment of little use to the community in that some important ingredients are missing and in that these models do not clearly pass a validation or refutation criterion, so that it is impossible to say whether or not these models are suitable to reproduce reality.

Moreover, the architecture of the paper should be modified. It seems to me that a clearer and more rational approach from the point of view of the construction of a physical model would be schematically the following:

1. What are we trying to demonstrate or test? A spatially variable model of instantaneous erosion? An erosion model representative of the long term geometry at equilibrium (if this notion means anything on slopes affected by landslides)?

2. What are the constraining observables to validate or invalidate the models? In the submitted study, if I understand correctly the only constraining observable, presented just in a qualitative way is the fact that in general, past the fluvial/debris flows transition the slope continues to increase going towards the source. This constraint being unique, it is essential to be clear on this constraint. Is this observation general? Or is it just observed for 3 drainages in the San Gabriel Mtns (fig. 1)? It would be helpful to offer a mini-synthesis of observations made on this topic in the literature. And to add quantitative criteria (e.g. the ratio between the slope at the source (Sch) and the slope at the transition (S(A=Adf)), or another criterion quantifying whether the slope remains stable or continues to increase above the transition A=Adf). On this point of quantification it is essential to know if the slope continues to increase as suggested by the authors. If so, as said before, this systematically disqualifies the process-based model that predicts an increase in the downstream slope between Ach and Adf regardless of the values of alfa and beta.

3. To build the model, one needs to keep the essential elements (as for a Taylor expansion, do we keep all the details at order 1 (there is no point in keeping terms of order 2 if all the terms of order 1 are not kept). In the absence of a theoretical framework allowing to make this choice, one can at least define, given the points 1 and 2, what are the elements of the model that it is essential to keep. I understand that it can be complex to introduce into the equation (6) an aggregation term (M increasing downstream) of the sediments (and of its momentum) during downstream transport, but it is on the other hand extremely easy to conceive just a multiplicative term in the frequency of passage of the debris flows at a given point, which increases according to the drained area (and to the number of upstream talwegs likely to generate debris flows departures) _ this is equivalent to introduce a "kdf" that would depend linearly (or not) on A.

4. Propose in particular for a simplified model like the empirical model here a first simplified analytical resolution to predict the main trends. In the present study, given this analysis highlighting the role of gamma as a parameter conditioning the increase or reduction of A downstream, the phenomenon carried by the gamma parameter cannot be neglected or dismissed. It must be taken into account. At this stage, the authors in their study should have resumed their model, added this aggregation to the process-based model, and proposed a new model (i.e. discard the old model which can be considered as a first draft) and only talk to us about this last model.

5. Verify or deepen these first conclusions using the numerical simulation. For the "empirical" model, the simulation will allow us to take into account the capital theta term and to have an analysis based on an unapproximated solution.

6. Possibly propose a more advanced model if the empirical model does not allow to account for the observables.

Other issues :

- I found in several equations some problems with the dimensions that are not respected; because I did not check everything in detail, I encourage the authors to recheck all the equations. There is also a vagueness about the volume of debris flow and how it is introduced for the process-based model. To be corrected.
- Why are the ranges of exploration of the parameters external to the model (M0, kdf, ke ...) not the same for the two models? If we want to compare the performances and predictions of the two models, it seems to me obvious to explore the same ranges of values.
- If we want to model the landscape, then it is required to be conservative with respect to the sediments. This is indirectly addressed in the discussion through the coupling between U and kdf , but it must be done more rigorously (especially since it is simple to

do). For instance (lines 339, 356), the choice of the relations between U and kdf is totally arbitrary, and is not even the same between the two models (this choice is not trivial because it will condition the subset of orange points and the slope of the relation Sdf=f(U) for this). For example, for the process based model, increasing the uplift rate by a factor 10 leads, given the chosen coefficients within the inequality, to vary kdf by on average a factor 2. In theory, and excluding a small proportion of material exported by other processes (wet ravelling, subfluvial process?), an increase of erosion rate by a factor 10 should lead to an increase of the debris flow frequency by a factor 10 (assuming that their volume remains constant).

- It seems to me that in order to reproduce a long term geometry, introducing a temporal distribution of debris flows could be necessary. Indeed, the authors rely for the fluvial part on the relation proposed by Lague (2014). It seems to me that one of the main conclusions of this study is that the exponents of the law E=f(S,A) depend strongly on the distribution of floods because the instantaneous incision law includes a threshold (tauC) below which erosion is zero. For debris flows, since there is also a threshold for the motion onset or efficiency of erosion (tau_y), one can anticipate that the resultant of the mean law will be sensitive to the combination of a threshold and an event distribution with small events traveling little distance because h and tau will be small), and that this may impact the position of Adf, as well as the shape of the transition between the two domains which will be more gradual. In other words, it is again a matter of trying to be consistent: as the problem (and in particular the transition zone) depends on the law of river incision downstream and debris flow upstream, it is important to include the same level of detail in the models on both sides.
- I would suggest adding a schematic graph describing the process-based model, and (in appendix?) one or more results of the propagation of a debris flow downstream as simulated by the process-based model.

Figure 1 caption : add « (eq. 17) » to link the equation to the text. Indicate the projection system (UTM zone ??). I tried to look at the location of these points but it does not correspond to a drainage basin that can be unambiguously identified)

Figure 1: add the P value for ach fit

Line 68: would it be possible to indicate another reference, i.e. other than this PhD thesis that cannot be easily accessed ?

Line 111: those values seem to me quite arbitrary. Why this choice? In addition, the ratio ms/ns=0.6 seems a bit high compared to classical curvature parameter values of 0.4-0.5.

Line 114: I would suggest to provide a number to this equation, and to discuss more at length the choice of the parameters, and in the discussion the implications of this choice.

Line 126 and Eq (3): where this equation coming from? It needs to be explained and

justified. This relation is not detailed at all and refers to a thesis that is not readily available online and to a work that has not been peer reviewed. It is impossible for me to judge its relevance in these conditions, and it seems essential to me to publish beforehand or to include in this paper the developments proposed in this PhD thesis.

Line 127: for reasons given above (and to justify/discuss the sensitivity to uplift rate), it would be more appropriate to display Fdf explicitly instead of hiding it in kdf.

Line 142 (eq. 4): this equation is not homogenous. Either some terms are missing (like the frequency of debris flows), or the kdf units (as given in table B2 to B6) is incorrect

Line 153: "in a rectangular channel" This is a major hypothesis. As much a river channel constrained by its banks or in a canyon can possibly present a rectangular section, as much an ephemeral channel head presents, for what I saw in the nature, a rather widened or prismatic shape. This choice was made for simplicity I assume, but it would be necessary to discuss the adequacy of this assumption and in the discussion whether having a wider channel would change the results.

Line 156 (eq. 6): this equation contains several errors.

The 3rd term on the left hand side is not homogeneous. I assume it is rather gz.h^2

I assume that gx (1rst term on the right hand side) has to be replaced by the projection of the weight onto the channel sloping direction (otherwise, gx=0); and similarly, gz has to be replaced by the projection of the weight onto the direction normal to the sloping channel bed.

Line 187: "introducing this effect … is beyond the scope of this study". This sentence is quite paradoxical: why do you decide not to incorporate this effect in the process based model, and to do so in the empirical model? If you want to compare the performance of the two models, then you need to consider equivalent boundary conditions and hypothesis on flow volumes.

Again I presume that incorporating this effect in the eq. (6) is uneasy. However, one can easily play with the frequency of debris flows to introduce this dependence (linear or by a power relation with an exponent gamma' between 0 and 1) to the drainage area. In that case, you should do it similarly for the two models (i.e. not consider the relation M=M0.A^gamma for the empirical model, and saying/demonstrating that you will capture the two effects with only one process)

Line 194-198: this part was not clear to me until I realized that there is no distribution of the volumes of debris flow but always the same one running through the channel. Did I understand correctly?

Line 206: this equation seems to me oversimplified: first one should use the hydraulic radius instead of h, except if one can demonstrate that w>>h; second for steep slopes, S should be replaced by sin(theta) with theta the slope angle. The more exact equation should be first written and then the potential simplification justified

Line 210: "we specify debris flow volume at each grid cell". Do you mean "passing through each grid cell "?

Line 211: how A is expressed ? in m2 ?

Line 225 (eq 13): where is the 10^-6 factor in front of A. Are A units now km2?

Line 305: "some parameter combinations …": some ? No, ALL parameter combinations according to fig.2 for which Smax is always strictly larger than Sch.

Line 308: "… is inconsistent with observations that indicate slope continues to increase or remain constant as drainage area decreases…" . In other words, given above remarks, the whole process-based model should be rejected … as long as it does not include a downstream increase of the volume or of the frequency of debris flows.

Lines 331, 332: those conclusions are a bit obvious (except on the relation between U and Adf). No need of doing numerical simulations for this.

Line 364: replace kdf by gamma within the inequality. Again the choice of a coefficient 0.5 seems quite arbitrary. In addition, and in contrast with the DF frequency, it remains unclear to me why this gamma parameter should be modified with U.

Line 376: "data and numerical experiments presented here are not capable of differentiating… although cases where alfa < 3 and beta > 2 generally perform poorly". The authors are quite honest and objective in this sentence. They should start from this sentence, instead of introducing the section insisting on the fact that McCoy's (2012) model with alfa=6 and beta =1 perform well. It seems to me that present study does not permit to reject this model, but neither does it validate it.

Line 407-408: This is obvious: from the moment when, by definition of equation (17), Sdf is defined where the slope becomes constant and deviates from the fluvial relation S=f(A), it goes without saying that Sdf becomes disconnected from any parameterization of the fluvial law (except if ms is close to 0).

Line 418: is Penserini et al. (2017) the only paper that tested the relation between channel head slope and uplift rate? Have the authors checked the literature in the whole US, Italy, Taiwan, Himalaya-Tibet, etc ?

Line 423: ok for the regression in fig. 10b but in contrast the difference in fig. 7b does not seem major between orange and blue points.

Lines 445-446: these lines are redundant with lines 418. The whole discussion should be condensed on these points.

Line 457: "the channel width scaling, b, may … exert control over the long term channel". I would rather say that this unconstrained parameter has a primary role in the fact that the slope increases downstream until ~Adf instead of slightly decreasing.

Lines 476, 487: "the landscape evolution model presented here …". This model cannot be called a landscape evolution model because it is 1D and, above all, does not conserve water or sediments. Introducing progressive aggregation of larger sediment supply as we go donwstream in order to respect steady state erosion of the landscape would be the minimum. The introduction of gradual aggregation of larger sediment fluxes as one moves downstream (whatever it I achieved playing with M or Fdf) in order to respect steady-state landscape erosion would be the minimum requirement in that direction.

Line 487: "demonstrate" should be replaced by "propose"

Appendix A: Given that many parameters are jut arbitrary (for example Deff) , I don't see the point to describe this section since it is already done in Lague (2014). But if the authors prefer to keep that section for the reader, then the instantaneous incision law should be explicitly described or written.

Line 503: if Rc is a runoff, it should have units (m or m/yr)

Line 504: the equation should be provided with a number (A1 ?) and t should be subscripted into "kt".

Table B3: why M0 is not varied among the different parameters?

Tables B3 and B5: Problem of units for ke

Table B3 and B4: I would suggest putting in a different table the fluvial and forcing parameters, which are common to the two models (U, M0, ke)

Table 5: U is given as constant whereas it is supposed to vary over a certain range