Earth Surf. Dynam. Discuss.,
doi:10.5194/esurf-2016-41-RC3, 2016

Earth Surface Dynamics Discussions

Interactive comment on "Steady state, continuity, and the erosion of layered rocks" *by* Matija Perne et al.

K. Whipple (Referee)

kxw@asu.edu

Received and published: 14 September 2016

This manuscript is well written and entails an important step forward in understanding the influence of rock strength variations in landscape evolution. The novel focus is on the influence of the slope exponent (n) in the stream power river incision model on landscape evolution in areas where sub-horizontal layered rocks with varying rock strength are exposed – extending beyond a recent treatment from my group (Forte et al., 2016, Earth Surface Processes and Landforms) that considered only the n = 1 case. It is remarkable that the venerable stream power model still holds surprises! Though of course it is always important to consider the degree to which processes and effects not encapsulated in the stream power model will alter the behavior of natural landscapes.

There is much value in the analysis and discussion presented. Reading and carefully reviewing this paper has notably advanced my own understanding of how landscapes described by the stream power model will evolve in the presence of layered rocks as a function of the relative strength between stronger and weaker layers, the relative thickness of strong and weak layers, and the dip of the contacts (only simple planar dip panels considered thus far) in cases with n<1 or n>1.

As part of the process of reviewing this paper I re-derived most of the key relationships and updated an existing 1d finite-difference solver to handle a series of dipping layers with variable erodibility (K in the stream power model) and variable thickness so I could test both the author's initially counter-intuitive results (such as the formation of cliffs in the weak units, not the strong units, if n < 1) and my own derivations. I find complete agreement with Figures 3, 4, 5, and 8. Similarly, though I would word some aspects differently (reflecting differences in my derivations described below), I agree with the points made in the discussion and conclusions. Thus I agree with all the findings in a qualitative sense. Likewise I see no problems with the numerical simulation results – both in 1d and 2d using FastScape.

However, I do not agree with some of the derivations and prefer a different approach to solving the problems discussed and explaining the interesting results of the 1d profile evolution models. As the only way I felt I could evaluate the derivations was to re-do them following my own intuition for how to pose the problem, I present alternative solutions below. Rather than working the derivations here, I outline the logic the present the solution. Hopefully this will prove an effective and constructive approach. The alternate derivation given below results in an identical solution for horizontal bedding (Eqn 5), which is good, but suggests differing sensitivities to the dip of contacts and the relative thicknesses of strong and weak units.

First, I don't much like the conceptual model in Figures 1 and 2. Most important, a problem only arises in the strong-over-weak case: overhangs cannot be sustained, as illustrated in Figure 2a. Conversely, as illustrated by Forte et al. (2016) and commonly seen in nature, weak rocks can readily be stripped off the top of strong rocks, leaving a tapering wedge of weak rock in the case of an upstream-dipping contact like that shown in Figure 2b. I also don't like the use of the word "continuity" for this, since in much of the geomorphic and fluid flow literature "continuity" means conservation of mass, though I appreciate that you are imposing a continuous profile with no overhangs.

I find it most useful to think about this problem in terms of the controls on the kinematic wave speed that characterizes the evolution of river profiles governed by the stream power incision model (Rosenbloom and Anderson, 1994): Ce = K Aˆm Sˆ(n-1). Key elements are (1) all else equal the kinematic wave speed is higher in weak rocks than strong, and (2) wave speed decreases with Slope for n<1, is independent of Slope for n=1, and increases with Slope for n>1. The surprising results in this paper all stem from the curious effect that wave speed decreases with Slope for n<1.

From study of the evolution of 1d river profiles cutting through layered rocks for cases n<1, n>1, and n=1 revealed in numerical simulations (as in Figures 4 and 5), I suggest below a set of fundamental controls on the development of profile shape (cliffs formed in the weak rock (n<1), the strong rock (n>1), or through each strong-over-weak couplet (n=1)), and the retreat rate of the slope-break knickpoint at the strong-over-weak contact.

The authors come close to stating what I believe is happening in the case of horizontal contacts: (1) fundamentally cliffs are forming because all-else held equal the kinematic wave speed of profile segments within the weak unit exceeds that of segments within the strong unit, so there is a tendency to undermine, or to form consuming knickpoints at strong-over-weak contacts, but as described by the authors and illustrated in the numerical simulations, the river profile will evolve toward an equilibrium where the upstream migration rate of the strong unit matches that of the weak unit at the contact; (2) for n<1 wave speed decreases with increasing slope, so in response to the tendency to undermine, the profile steepens in the weak unit until the wave speed of the weak unit at the contact has slowed to equal the wave speed of the strong unit at the contact

(the strong unit maintains an equilibrium slope at the contact and the knickpoint at the contact migrates at the rate set by the wave speed of the strong unit on an equilibrium slope – this best describes the basal strong-over-weak contact, see below); (3) for n=1 wave speed is independent of slope, so the river profile has no way to respond, and a vertical cliff forms (50% in weak, 50% in strong unit) with retreat rate = wave speed of the weak unit – the cliff grows in height until the full strong-over-weak doublet is incorporated and the retreat rate is set by the wave speed of the weak unit; (4) for n>1 wave speed increases with increasing slope, so in response to the tendency to undermine, the profile steepens in the strong unit until the wave speed of the strong unit at the contact has increased to equal the wave speed of the weak unit at the contact (the weak unit maintains an equilibrium slope at the contact and the knickpoint at the contact migrates at the rate set by the weak unit on an equilibrium slope).

Once this realization is made, it is easy to write equations for the wave speed in each unit at the contact, set them equal, and solve for the ratio of the slope of the weak unit (Sw) to the slope of the strong unit (Ss). For horizontal beds, Equation 5 in the paper is recovered. So the derivation given is exact in the limit of horizontal contacts (also satisfies expectation for vertical contacts). However, in my analysis the derivation for the case of non-horizontal contacts (Eqn 2) appears to be incorrect. First, the solution only applies for strong-over-weak scenarios, so E1, S1 (downstream) could only be the weak unit. Second, if the derivation described above for horizontal contacts is generalized to account for planar dipping beds, a different solution is found.

In the case of dipping beds, the migration rate of the knickpoint at the strong-over-weak contact is not set only by the kinematic wave speed (Ce = E/S = K Aˆm Sˆ(n-1)) as it is for horizontal contacts, but must account for the slope of the contact (Sc). For example, if the contact were exactly parallel to the river bed, then the migration rate would approach infinity over that reach. Geometrically one can readily show that the local knickpoint migration velocity (Ce_kp) will be: Ce_kp = E/(S – Sc) (which correctly reduces to the kinematic wave speed for Sc = 0).

(Note here that the caption to Fig. A1 indicates that Sc in the paper is defined positive for an upstream dip, while channel slope S is positive downstream. I worry that this could prove very confusing. Here I instead define Sc as positive for downstream dip).

As noted above, the migration rate of the slope-break knickpoint at the contact is set by the equilibrium wave speed within the strong unit for n<1 (at least for the basal strong-over-weak contact), and within the weak unit for n>1 – the problem then is how the dip of the contact amplifies or reduces knickpoint velocity relative to the kinematic wave speed. Solving for the equivalent of Eqn 5 in the presence of dipping beds, I find (derivations available on request):

$Sw/Ss = (Kw/Ks)^{(1/(1-n))} * (1 - Sc/Ss)^{(1/(1-n))}$; for n<1 $Sw/Ss = (Kw/Ks)^{(1/(1-n))} * (1 - Sc/Sw)^{(1/(n-1))}$; for n>1

Note that Sc/Ss appears in the n<1 case because the retreat rate is set by the wave speed in the strong unit, and Sc/Sw appears in the n>1 case because the retreat rate is set by the wave speed in the weak unit.

These solutions, however, only obtain over a range of Sc/Ss or Sc/Sw – basically restricted to sub-horizontal conditions – as outlined below. In addition, as mentioned above, the n<1 solution applies best to the basal strong-over-weak contact: the over-steepening of the weak unit is damped up-section because the slope-break knickpoints at the strong-over-weak contacts act as a local baselevel, reducing local incision rate within the overlying strong unit (as happens in weak-over-strong contacts with n=1). This causes a decrease in the slope within the strong unit, which increases the kinematic wave speed and thus decreases the degree of over-steepening of the weak unit. This complicating phenomenon is restricted to the n<1 case.

I have tested these revised equations, and the limits on their applicability outlined below, against numerical simulations with satisfying results.

For n<1 and downstream-dipping beds (Sc is positive), the solution only applies for

$Sc/Ss < 1 - (Ks/Kw)^{(1/n)}$: for larger (more positive) downstream dips, an equilibrium profile results (Ss and Sw have equilibrium values = to the vertical contact case even though knickpoints are slowly migrating upstream over time). For n<1 and upstream-dipping beds (Sc is negative), preliminary comparison with numerical simulations indicates the solution is only valid for $abs(Sc/Ss) <\sim 1$. For steeper upstream dips, the profile transitions toward an equilibrium form (I have not studied this in detail).

For n>1 and downstream-dipping beds (Sc is positive), the solution only applies for Sc < Sw. At Sc = Sw, knickpoint velocity is infinite. For Sc > Sw, the strong-over-weak contact propagates downstream, invalidating the analysis. For n>1 and upstream-dipping beds (Sc is negative), the solution only applies for $Sc/Sw > 1 - (Kw/Ks)^{(1/n)}$: for larger (more negative) upstream dips, an equilibrium profile results (Ss and Sw have equilibrium values = to the vertical contact case even though knickpoints are slowly migrating upstream over time).

These solutions can be re-cast into the form of Eqn 2 (note I have inverted the relation here):

$E2/E1 = Es/Ew = (Ss - Sc)/Sw$; for n<1 $E2/E1 = Es/Ew = Ss/(Sw - Sc)$; for n>1

Thus Eqn 2 should have two forms, one for n < 1 and one for n > 1. (remember that Sc is defined here as positive downstream).

Section 3.2. I did not attempt to reproduce or critically evaluate Equation 8, but found no dependence of erosion rate patterns on H1/H2 in my numerical simulations. For horizontal beds, Equation 5 is exactly satisfied for a very wide range of H1/H2. I did not investigate whether a greater sensitivity to layer thickness emerges with dipping contacts.

Section 3.3. I don't see the profile as being "perturbed" at baselevel because, as the authors note on page 6, line 27, new river segments formed at baselevel always begin in equilibrium (E = U, and equilibrium slopes). The perturbations develop above as the differential wave speeds near contacts begin to manifest in deviations from equilibrium slopes and erosion rates. Thus I'm not enthused about the "damping length scale" terminology. However, the result appears robust – differential wave speeds are rapidly accommodated at the first strong-over-weak contact, with knickpoints at contacts quickly converging on a migration velocity set by the equilibrium wave speed of either the weak unit (n >= 1) or the strong unit (n < 1).

That said, I am confused by Eqn 10. First, there appears to be a typo in Eqn (10): as derived, the last term should be Aoˆ(m/n) not Aˆ(m/n). Further, Ce = kinematic celerity = horizontal migration rate of river "patch" (patch as used by Royden and Perron, 2012) = K Aˆm Sˆ(n-1). For a steady-state river patch, S = (U/K)ˆ(1/n) Aˆ(-m/n). Combining these, Whipple and Tucker (1999) showed that the horizontal migration rate of a steady-state river patch is Ce = Uˆ((n-1)/n) Kˆ(1/n) Aˆ(m/n) – this is the relation given for Eqn 10, so the equation appears to be correct, but the derivation (and the apparent typo) implies it is incorrect.

Finally, although widely appreciated, it seems worth stating that readers should beware the difference between the mathematics of the stream power model (SPM), insightful though they can be, and the physical reality of nature. Many processes are not represented in the SPM and therefore predictions may fail. Despite this, I am very supportive of publishing papers like this that explore model predictions because this allows one to: (1) generate testable hypotheses, constrain parameters, or recognize where models fail and why; (2) use any failures to improve the model; and (3) know what will happen in landscape evolution simulations based on the SPM under different conditions.

I have a few additional comments listed below with reference to page and line number.

1. Title: I suggest revising title to remove "continuity" as this will mean "conservation of mass" to many. Also I suggest emphasizing your key finding about the dependence on n, if you can find an effective wording.

2. Page 1, Line 21-22: This is not true. Many studies of bedrock channel morphology

are expressly seeking information about the history of climate, tectonics, or drainage divide migration recorded in non-steady state profiles (as you note on page 2, line 4).

3. Page 2, line 9: better to not call the stream power model (SPM) a "law".

4. Page 2, line 11-12: the SPM is widely used in modeling studies, but is not required as a basis of profile analysis – channel steepness and concavity can be measured and interpreted in terms of relative uplift rate, climate, or rock strengths independent of the river incision rule.

5. Page 3, line 3-4: as you show in your analysis, this is not true for n<1.

6. Page 3, line 5-6: I disagree. Where a weak layer overlies a strong layer, there is no constraint on the relative stream segment migration speed – the weak layer can be stripped off, leaving a bench on the underlying strong layer or a tapering wedge of the weak layer. Such forms are very common in nature.

7. Page 9, line 9: This sentence is confusing since channel segments formed at base-level are always initially at equilibrium (E=U, steady state form) in systems described by the SPM.

8. Page 9, line 16: "channel steepness" here would be better written as "channel slope" or "channel gradient", since "steepness" commonly refers to the steepness index.

Interactive comment on Earth Surf. Dynam. Discuss., doi:10.5194/esurf-2016-41, 2016.