Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2020-39-RC3, 2020


# *Interactive comment on* "Transport-limited fluvial erosion – simple formulation and efficient numerical treatment" *by* Stefan Hergarten

**Anonymous Referee #3**

Received and published: 18 July 2020

Review of Hergarten 2020 eSurf Transport-Limited

General comments:

This paper introduces a valuable new addition to the growing collection of efficient algorithms for computational landscape evolution models. Computational performance has long been a bottleneck for these models. Previous work by the author and by Braun and colleagues has yielded implicit $O(N)$ algorithms for wave-equation-like erosion laws, and more recently for the class of models that contain both erosion and sedimentation terms. Here Hergarten shows that slope-linear forms of the transport-limited and erosion-and-sedimentation models can be solved in $O(N)$ complexity using a direct implicit method. Bravo!



The extension to the erosion-and-sedimentation class of models makes the method broader than the title suggests, because that class of model can manifest a range of behavior across the detachment-limited to transport-limited spectrum. The author might therefore consider modifying the title to reflect this.

While it is true that Yuan et al. (2019) recently introduced an $O(N)$ algorithm for this class of models, there is certainly value in developing and comparing alternative numerical algorithms for the same problem. And appropriately, the author compares his new method with that of Yuan et al. (2019).

One aspect of the method that I find interesting is that the implicit algorithm should apply equally well for one-dimensional diffusion problems, provided that the flow in question is always oriented in one direction. That might not be tremendously helpful for people who want to model diffusion, since we already have numerous well-known solution methods in 1D and 2D for that particular problem, but it does suggest a way to test the proposed scheme under transient conditions. The manuscript notes that 'investigating the temporal behavior turned out to be quite complex'. Yet understanding temporal behavior is one of the reasons to use landscape evolution models in the first place. It is important to know something about the limits to accuracy and stability of a numerical scheme under transient conditions. I suggest therefore that the author try formulating a 1D, uni-directional diffusion problem and solving it with this implicit method for the transient case of a step change at one end of the domain. The analytical solution for that case is well known, so it seems like a good opportunity to test the properties of the proposed scheme under transient conditions. Basically, it would be a matter of having a 1D domain and a constant value of $A$.

Such a test might also make it possible to identify constraints on time-step size. The manuscript notes that implicit methods allow arbitrarily large time steps. Yes that's true in principle, but 'arbitrarily large' really just refers to stability. Two other considerations are: how does step size influence solution accuracy, and in particular for landscape models, to what extent does drainage network reorganization limit step size? These questions are undoubtedly hard or maybe impossible to answer in general, but some practical rules of thumb would be useful for those who wish to apply the algorithm in practice. So again I encourage the exploration of a transient case of simple 1D diffusion.

Comments by line number, equation, or figure:

14 the word ‘uplift’ has a long history of ambiguous usage among geoscientists. I recommend specifying ‘uplift of crustal material relative to a given datum’ or something like that.

16 I suggest adding some references here for the benefit of readers who are just getting into the topic. I am not sure of the provenance of the term ‘transport limited’, but I think it appears in Carson and Kirkby (1972) in the context of hillslopes. For the landscape evolution context, Willgoose et al. (1990, Water Resources Research) might be a reasonable reference, though I do not remember whether they actually used this phrase. As far as I know, the term ‘detachment limited’ was coined by Howard (1994, Water Resources Research).

26 Up to this point, you have not actually defined transport limited. This would be a good place to do so. I think of a transport-limited river reach as one in which the rate of bed erosion is limited by the ability of the flow to transport the eroded material downstream, rather than by the availability of potentially mobile sediment (feel free to use this wording if you like it).

For what it is worth, in my view, the definition is actually fuzzier than we sometimes pretend: the ability of moving fluid to transport sediment depends very strongly on the size and density of sediment on the bed. There is no such thing as a ‘transport capacity’ independent of bed sediment characteristics. Bed-load theory tells us that transport capacity depends on critical shear stress, which in turn depends on sediment size and density; suspended-load theory tells us that sediment concentration depends on near-bed sediment concentration and on settling velocity, both of which also depend



on size and density. But for purposes of this paper, the only real practical implication of this observation is that one should be cautious in using the phrase ‘transport capacity.’

31-33 Second-order derivatives only appear if $q$ is a function of topographic gradient. Suggest adding wording to clarify this, e.g., ‘Because $q$ is a function of topographic gradient, eq (1) contains...’

35 Change ‘In the last years’ to ‘In recent years’

36 ‘there seems to be a trend to’—I also share the impression that use of a detachment-limited stream erosion model in landscape evolution studies is common, but whether there has been a trend in that direction is harder to say. There are situations in which a transport-limited model is suitable, and plenty of literature on such models (e.g., Wickert and Schildgen, 2019; and a great deal of the work by Greg Hancock, Tom Coulthard, and colleagues). Suggest simply asserting that detachment limited is a common or popular choice.

40 Of the three suggested reasons for the widespread use of detachment-limited discharge-slope models, I think the second two are really the important ones. The first might be a bit misleading to readers, because any of the three flavors of model discussed in this paper can be related to a power-law slope-area relationship. As far as I know, the link between erosion/transport and slope-area was actually first identified in a transport-limited context. If I recall right, Howard (1980 in Thresholds in Geomorphology) articulated a slope-area relation based on a variety of different transport formulas, and Howard and Kerby (1983) followed up with a field-based study. Then Willgoose et al. (1991) and Willgoose (1994) really hit home the slope-area relation in a transport-limited context. So having a link with Flint’s law is not unique to the detachment-limited formulation. The solution I suggest is just to add a sentence, maybe after the sentence following eq (6), to the effect that ‘transport-limited and other types of erosion law can also be linked to Flint’s law (references), but the relationship is especially simple for the area-slope erosion law in eq (6)’.

49 ‘has become some kind of paradigm’—I think I understand what you mean here, but as written it is a vague statement (what exactly constitutes a paradigm? what ‘kind’ of paradigm?). Better I think to leave this comment out.

56 ‘little is known’—this statement is a bit unfair to researchers who have tried to pin it down. Suggest softening to something like ‘the effective value of $n$ is less well known’. You could also add something like: ‘some studies suggest a linear scaling (REFS), some sub-linear (REFS), and some super-linear (REFS)’.

62 There are quite a few other papers that report estimates of $K$ values, which could be cited here. I guess the ‘e.g.’ is meant to say ‘there are more papers than I feel like bothering to list here, but if you want a starting point, try these two’. I guess that’s ok, but you are likely to annoy the authors of the ones you left out. An alternative would be to find a recent paper or two that reports $K$ values and is reasonably comprehensive in its referencing, and cite as ‘So-and-so, 20xx, and references therein’.

76 The wording is a bit awkward here; suggest leaving out ‘despite increasing computing...’ (we all know computers have gotten faster).

87 ‘models treat’

100 confusing because you would choose either (2) or (8); how about ‘(1) and either (2) or (8)’

109 the upstream

eq (11) and preceding text: the way this is written seems to suggest that approaching the problem from the question of ‘how much sediment would you get from eq (6)’ is a requisite for deriving the method that follows. Actually, there are at least two other pathways that I can think of. I think you are more likely to ‘sell’ the approach more effectively if you point out that there are several lines of evidence to support the hypothesis that the long-term sediment flux should depend on slope and drainage area. You have articulated one of them, but it seems to me it is subject to the criticism that you are using



a detachment-limited concept (eq 6) to derive a transport-limited model. An alternative would be to state that previous studies have shown that sediment-transport formulas can be cast in the form of an area-slope power expression, and cite some references. You could also lean on Davy and Lague (2009) here, because when you combine their expression with a unit-stream-power detachment rate and $Q_w \propto A$ ($Q_w$ being water discharge), you end up with a transport capacity (if I recall right) that looks like $A^{3/2}S$ (more generally, $A^{m+1}S^n$). I think it is fair to say that there is uncertainty in the literature over how best to express transport capacity in models of stream profile evolution or landscape evolution. Some have $Q \propto Q_w S$, some (e.g., Willgoose, Howard, based on the empirical Einstein-Brown expression) have $Q \propto q_w^2 S^2$, and some include a transport threshold. Key point for your purposes is that $Q = KA^{m+1}S$ falls within the span of proposed laws.

130 change which to that (introduces a restrictive clause)

132-3 I do not understand this comment about Voronoi polygons. Normally in a finite-volume solution, you would integrate flux density over the width of a cell face, whether it is a square or a Voronoi polygon or some other shape. Using $Q$ instead of $q$, with an implied sub-grid-scale channel width (I suppose), you do not need to do this integration; but that is true regardless of the shape of your cells. Is your point that the discrete representation in eq 12 works in principle for any grid mesh, regular or irregular? Consider removing this statement, as it seems like a bit of a distraction.

137-8 See comment above about transport laws. Equations like (11) have been frequently used in the literature. In particular, in the work of Willgoose et al. (1991a,b,c) and subsequently, the slope-area relationship is used to estimate parameters for a transport law. If there is something very specific about eq (11) that you think is unique, then that should be pointed out. Otherwise, the statement carries the implication that transport laws have never before been derived from slope-area analysis, which is not correct.

144-5 I believe it is more than a matter of terminology. It is rather a matter of dimensionality. If you suppose $m = 1/2$, then the erodibility has dimensions of inverse time, whereas the transport coefficient has dimensions of length$^2$ / time.

153-157 Consider adding some more explanatory text here. At first glance, eq (15) looks like a Taylor expansion to first order. But if I am following this correctly, actually $Q_i^0$ includes the value of $Q_i$ at $t_0$ plus the partial derivative of $Q_i$ with respect to $H_i$ times the change in $H_i$ during one time step. That's a clever idea, and is consistent with the explanation on line 156, but it took me some time to work it out. Other readers might similarly mis-interpret $Q_i^0$ on a first look, and yet its definition is really key to whole scheme. Suggest devoting a full sentence or so to pointing out the definition and importance of it.

177 The challenge for readers is that the donor information is buried in the definitions of alpha and beta. Suggest adding, after the word donors, '(because $\alpha$ and $\beta$ depend on donors' $Q'$ and $Q$, respectively)'

187, 189 - reference to a recursive implementation is vague. Suggest referring to a published algorithm(s) for sorting by downstream order.

198-200 Please document somehow the specifications for the performance tests: for example, the number of iterations were run for each case.

207-8 With all due respect, I think this is a missed opportunity. As noted above, I suggest trying a solution with one row of grid nodes (so, strictly one dimensional) and a uniform drainage area. Then it reduces to a linear diffusion equation, which you could compare with the transient analytical solution for diffusion given a step change at one boundary.

213 Please explain the rationale for increasing $\delta t$ over time.

217-8 To avoid potential confusion, it would be useful to clarify that the two models are NOT equivalent, but rather their steady state solutions have the same slope-area



relationship. Either give the predicted slope-area equivalence, or quote a reference that does (or both).

223 and following: The tent-shaped uplift pattern is a clever test. I think the example would be easier to follow if you did two things. First, before referring to the results (figure 2), explain why you are using this tent-shaped uplift and what differences you expect to see between the two models. That way, the reader knows what to look for in Figures 2 and 3. Second, it would be very helpful to provide an analytical solution for the two models. You could simply use Hack's law to relate drainage area to distance (I would just make the exponent 2 for simplicity). Plot the predicted longitudinal stream profiles with a tent-shaped uplift pattern for each model, in chi space (you could do linear space too). That way there is a clear expectation for Figure 3 (actually, you could simply add the analytical profiles to Figure 3). If I have done the math right, the two profiles should be defined by

$dH/dx = (u_0(L - x)/K)x^{-hm}$

$dH/dx = (u_0/K)x^{-h(m+1)} \int_0^x (L - x)dx$

where $u_0$ is the uplift rate at the ridgeline, $h$ is the Hack exponent, and $L$ is the domain half-width. So, should be possible to plot these as analytical expectations.

251 $\phi$ and $\psi$ seem to be parameters rather than functions.

254 Davy and Lague deserve much credit for introducing this formulation in the landscape evolution context, and showing that it relates to the earlier Beaumont model except that the length scale varies with unit discharge. For the record, similar formulations with erosion/entrainment and deposition terms seem to be widely used in the sedimentation engineering and soil erosion communities.

eq (34) I like this alternative expression of phi. Presumably it would simplify calibration by removing a built-in correlation between the two parameters.

284-5 Would not $G \to \infty$ lead to $Q_i' \to 0$ by eq (31)?

286 missing ‘to’

287 extra ‘of’

294 ‘some kind of’ is a bit vague. Suggest re-wording to be more precise.

295-6 Can you articulate what process(es) this kind of formulation is meant to represent? Is the idea that some of the material is so fine-grained that it will not end up being deposited until it reaches the ocean or some kind of closed basin? I wonder whether an alternative would be to build this into $dQ/dH$.

303-4 This statement is not clear to me. From the references cited, I guess that by ‘scaling problem’ you mean the classic problem of grid-size scaling. Yet that wasn’t mentioned as an issue with the prior models (transport limited and linear decline), so why is it more of an issue with equation 35 than with, say, equation 26?

317-325 Are you suggesting to solve for diffusive flux in the flow directions using the implicit scheme, and the other directions using some other scheme? How would you avoid double-counting the fluxes in the cardinal flow directions? Overall, I think the sketch presented here for handling diffusion is not really convincing. I would recommend either deleting it, or expanding it to really demonstrate how it would work.

Finally: nice work!

---

Interactive comment on Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2020-39, 2020.