Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-18-RC3, 2019

Geoscientific Model Development Discussions
Open Access

Interactive comment on "r.sim.terrain: a dynamic landscape evolution model" *by* Brendan Alexander Harmon et al.

Anonymous Referee #3

Received and published: 8 April 2019

This manuscript presents a useful new erosion modeling package. Some of the nice aspects of this contribution include:

- The package brings together a collection of alternative erosion and transport laws, which allows for inter-model comparison: an especially valuable thing given that the community does not seem to have a consensus on what are the 'right' rules to use in any particular setting. Furthermore, the manuscript provides a nice proof-of-concept demonstration in using this package to compare the predictions of different process formulations.

- As the title suggests, a novel feature of this modeling package is the capability of representing dynamic hydrology: that is, a representation of time-varying overland flow. As the authors note, this capability is not present in most landform evolution models (though there are a few that have tried to honor it in some form or another).

- The code and data are open source, and maintained in a version-controlled online repository. In fact, the authors have gone even further than this commendable practice by making their examples fully reproducible. Bravo!

- The package is embedded in the open-source GRASS GIS, which makes it easier to provide geospatial input data, and to analyze and display model output.

In my view, this is a very nice contribution to the soil-modeling and landscape-modeling ecosystem, and in terms of reproducibility, it sets the bar high for future modelers.

The main area for improvement of the manuscript, in my view, lies in the presentation of the governing equations. In several places that I have noted below, the relationship between different equations is murky, and there are places where the units seem to be inconsistent. I think these issues should be straightforward to address.

Detailed comments, keyed to page and line number (or in some places, equation, table, or figure number):

1/title - This is a total quibble, and please feel free to ignore it, but my first reaction to 'dynamic landscape evolution model' was to ask (rhetorically) 'is there any other kind'? Consider 'landscape evolution model with dynamic hydrology' as an alternative (admittedly a less pithy one).

1/5 'steady state or dynamic model' could be read as implying that the entire model is steady state, not just the surface water flow rates. Suggest re-wording: 'using either a steady state or dynamic representation of overland flow, ...'

2/2 I agree with the sentiment, but suggest rewording to 'a landscape evolution model that includes time-evolving surface water discharge', to avoid confusion over which aspect of the model is dynamic.

3/8 The phrase 'until water flow reaches steady state' suggests that the positive feedback (presumably between deepening/widening and attraction of more surface water flow) stops at this point. I don't think that is necessarily true; you could have a feedback between morphology and flow under steady runoff too.

3/11 Please explain what is meant by erosion-deposition regime.

3/14 Detachment vs transport capacity: this sounds backwards...

3/18-19 There are plenty of other papers that could be cited here, in which one or more of the listed methods was used to study gully erosion. (For example, here's a review paper that cites some TLS applications to gully erosion: Telling, J., Lyda, A., Hartzell, P., & Glennie, C. (2017). Review of Earth science research using terrestrial laser scanning. Earth-Science Reviews, 169, 35-68.)

Figure 2: please give location in caption. Also, numbers on color bars and scale bar are barely legible.

6/2 typo

6/2 I guess 'partial derivatives of the topography' means a numerical approximation of the derivative of the elevation field with respect to the two cardinal grid directions. Recommend more precision in wording here.

6/9 'steady state dynamics' - I think I understand what you mean here, but the phrase itself is awkward (it is self-contradictory)

Table 2: this is only a partial list of codes that have been published in, say, the last ten years. Why choose these particular ones?

Be careful about giving the spatial scale for these models. At least some of these codes have been used and published at a variety of different spatial scales, from say the size of a rilled hillslope to that of a small country; and in some cases (e.g., SIBERIA) is sometimes presented in a dimensionless mode in which no spatial scale at all is given or implied. As to temporal scale, I thought that at least some of these can also be run in 'event' mode.

Also, my understanding is that Landlab is not itself a model, but rather is a programming library that contains components that can be used to build various types of model, including landscape evolution. That said, people seem to have built landscape evolution models using Landlab (the Landlab website lists some of these). Maybe it would make sense to label this entry as 'Landlab-built erosion models' or something like that.

Section 2.1 generally: I like the way that this is carefully organised into sub-sections. However, the order of presentation confused me. Often, authors presenting a set of governing equations will start with the high-level conservation law(s), and then define each term more precisely. As noted below, there's an opportunity to do this at least partly in subsection 2.1.1.

Equation 2: it would be helpful to give some context and referencing. I think this idea comes from Foster and Meyer (1972), right? If I remember correctly, their key assumption was that the ratio of transport rate to transport capacity, plus the ratio of detachment rate to detachment capacity, sum to unity. Assuming I did the math right, this leads to a first-order reaction-like equation:

dz/dt = ds = sigma (qs - Tc)

I recommend presenting it this way here in section 2.1.1 (in addition to the definition given in eq 2), because this relates transport and detachment to the rate of change of elevation, and motivates the need for definitions for qs, Tc, and Dc.

Note that there seems to be a problem with units in one of the factors in eq 2: if Tc and Dc had the same units (as is listed), then sigma would be dimensionless. I suspect Dc is actually in kg mˆ-2 sˆ-1 (detached mass per unit area per time).

Equation 4: symbol v is used without being introduced. Presumably it is the depth-averaged flow velocity vector in (x,y). Either define v or use q (which you've defined already).

Also, whereas the paper is premised on the value of having a dynamic representation of surface-water hydrology (which eq 3 is), equation 4 is actually a steady solution, is it not? If the model indeed uses a fully time-varying flow model, the equations presented in this sub-section should show this. In addition, it would be helpful to provide a reference for this form of the diffusion-wave approximation (could be to a hydrology text that gives the derivation and assumptions).

Please give units of epsilon.

8/10 suggest specifying '...density in the water column', so it is clear that this is a mass concentration rather than a bulk density of resting sediment.

8/15 'steady state sediment flow with diffusion' - I'm confused by this. The equation is time-dependent, so how is it steady state? And the definition of qs above is advective, not diffusive.

8/17 So we need a definition for ds, which as suggested above, you could provide in section 2.1.1.

8/23 In the previous equation, you used a continuum formulation, whereas here you're giving a discretized-in-time form. Please be consistent. I suggest sticking with continuum forms, because these don't require you to make any statements about numerical approximation. And in fact, as noted above, I recommend putting equation 7 in section 2.1.1.

Equation 8: this equation is not dimensionally consistent. If you write it in continuum form,

dz/dt = -(1/rho) qs

you have m/s on the left and m2/s on the right. I'm also not convinced that the equation expresses the idea you want. I'm guessing that a detachment-limited regime would look more like dz/dt = -(1/rho) Dc. Then it becomes a question of what is your detachment capacity law? You've already introduced detachment capacity in Dc = sigma

Tc (eq 2). In order to close the equations, you need either a definition of Dc or Tc. Presumably these depend in some fashion on water discharge or velocity or boundary shear stress. Please specify (or, if I have misunderstood, explain why the equation set given is sufficient to describe the SIMWE module). Actually, after reading farther in the manuscript, I think the idea is that the RUSLE equation can be used for Dc in detachment-limited mode. If that's correct, then say something to the effect that the definition of Dc will be given in section so-and-so, and then use the symbol Dc in that section.

Regarding the role of qs, I suspect what you're after is the notion that qs is the up-stream/upslope integral of ds, is that right? If so, it would be helpful to present the math.

9/21 please give the functional form of this relationship

10/2 I get that there's a long tradition of practical empiricism in soil-erosion research. But what about pushing ever so gently back on it by presenting equation 10 in a slightly less brutally ugly form? Something like:

er / e_ref = 1 - a exp(-ir / i_ref)

where e_ref is reference energy equal to ... and i_ref is reference rainfall intensity equal to ...

10/7 shouldn't this be rainfall depth rather than volume?

Equation 11: again the units seem to be off here (apart from the oddity of having an 'index' that has [weird] units). I get the right side as being:

MJ haˆ-1 mmˆ-1 x mm x s = MJ haˆ-1 s???

11/3 the subsection is called 'Sediment flow' but it reads like an erosion rate. Though I guess it works given that you're defining it as mass flow per time per area.

Equation 13: again I'm struggling with units. I get:

(MJ mm ha^-1 hr^-1) x (ton ha hr ha^-1 MJ^-1 mm^-1) = (ha^-1) x (ton)

which are not the units given for E.

11/16-17 it's not clear to me how these equations relate. Maybe you mean that the definition of E in equation 13 is the SAME AS ds (or -ds) for transport-limited conditions, and Dc for detachment-limited conditions? In that case, it might suffice to simply call equation 13 the definition of Dc. You could then give the defintion of Tc as

sigma = Dc/Tc ==> Tc = Dc/sigma

(Note: it would be more intuitive to think in terms of a length scale, L = 1 / sigma, which is then the characteristic distance over which steady, uniform overland flow reaches its carrying capacity on a planar slope).

11/29 not clear to me what 'topographic component of overland flow' means

Equation 15: is T the same as Tc? Also, again, I'm not sure the units are correct here, please check, and correct if necessary.

16/1 intriguing comment about positive feedback loops, can you explain more?

Figures 5 and 6: why the different color schemes in two of the three comparisons (top and bottom rows)?

Figure 6: if the figure is meant to compare runs with two different rainfall intensities, which intensity was used for the upper and middle figures?

Software: I tested the model software by installing the latest stable release of GRASS GIS, then going to the GitHub repository for the model's source code. By following the "Basic Instructions" listed there, I was able to install the r.sim.terrain extension and run the example.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2019-18, 2019.