Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., author comment AC1
https://doi.org/10.5194/esurf-2021-35-AC1, 2021

Reply on RC1

Philippe Steer

Author comment on "Short communication: Analytical models for 2D landscape evolution" by Philippe Steer, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-35-AC1, 2021

Reply to review by Liran Goren (Reviewer comments are in bold)

The manuscript presents the development of a landscape evolution model (LEM) based on analytic solutions to the stream power incision model (SPIM), a simplified representation of vertical fluvial incision. Despite its simplicity, the SPIM is widely used in landscape evolution models for its demonstrated capabilities in reconstructing fluvial relief in response to changing boundary conditions at a relatively large spatial scale (i.e., Venditti et al. 2020). Model simplicity lent itself to explicit analytic solutions. Recently, there has been a growing interest in the behavior of the analytical solutions as part of studies that explored the expected dynamics of river long profiles as a function of changing geomorphic parameters and boundary conditions (i.e., Royden and Perron, 2013) and inversion applications (i.e., Goren et al. 2014a, Fox et al. 2014, Rudge et al. 2015, and many more).

The manuscript is well-written and clear. The motivation behind the research is well justified: developing an infinitely accurate 2D forward model for the evolution of fluvial relief in response to changing boundary conditions (in the current study, only changing uplift rate is demonstrated).

I am very grateful to Liran Goren for her comments.

However, model implementation and its demonstrated usefulness remain somewhat immature and could benefit from additional exploration. Furthermore, the presentation of the model operation suffers from some biases. Here I specifically refer to (1) The physical meaning of the greedy algorithm that searches for acceptable topologies; (2) The application of "heterogeneous but constant uplift rate U(l)"; and (3) The claim that the model also solves for hillslope dynamics.

I below answer to these three main comments.

For any given topology of a fluvial drainage network and associated boundary conditions, the analytic solutions of the SPIM predict the 2D relief of all the nodes in the model for all times. There is no need for numerical iterations. The

donor-receiver relation is needed only to calculate the response time, tau(x), numerically (including for time-invariant and space variable erodibility and precipitation). Once the response time is known, the solution for each node can be derived independent of the other nodes (see, for example, solutions in eq. 6, Goren et al. 2014a, and eq. 13, Goren 2016, for the case where K and P vary in time). Therefore, for any given topology, Saleve produces a graphical representation of the analytic solution.

I fully agree with this comment which does not call for any change in the manuscript.

What the analytic solutions cannot predict is the drainage network topology. Here is where the model could become more significant. Saleve addresses it by updating the topology using a greedy algorithm that attempts to minimize chi (or potentially, chi*, Willett et al. 2014, eq. 5) gradients across divides (in the manuscript, presented as elevation gradients), by updating the topology following the steepest descent in consecutive "update steps". The execution of each of these steps and the number of steps needed to achieve a stable topology have no physical time associated with them. While this issue is acknowledged in the manuscript, I'm not sure it is sufficiently discussed. Updating the topology toward the steepest descent commonly mimics drainage reorganization. However, whether each node should drain in the direction of its steepest descent neighbor regardless of the model spatial resolution is questionable. I.e., for sufficiently large grid spacing, this is probably not a very good representation. Additionally, there is no process related to this reorganization and, as stated in the manuscript, no time scale associated with it. Many LEMs use the steepest descent criterion to update the network topology (not all though, the DAC LEM (Goren et al., 2014b), for example, uses a physical criterion to decide if reorganization should take place or the older topology should be maintained) without discussing its meaning and related time scale. This omission is particularly apparent in Saleve because topological convergence based on the steepest descent algorithm while aspiring to minimize delta z across divides is the main numerical operation of the model.

I partly agree with this comment. First, I agree that despite its popularity the steepest descent algorithm is not optimal to model flow over a topography. However, I do believe this algorithm is relatively incorrect at most scales, including small and large ones. We are currently, with some collaborators, making some efforts to develop better algorithms to account for flow hydrodynamics in LEMs following the initial work by Davy et al. (2017) and Croissant et al. (2017). However, many current LEMs still (and will continue to) use the steepest descent flow algorithm thanks to its simplicity. The scope of this paper is not to upgrade the flow algorithm of these LEMs, nor to tackle the issue of the consistency of the steepest descent flow algorithm with natural flows, but rather to demonstrate that the numerical solvers that most LEMs use for erosion can be changed to analytical solutions in some specific scenarios.

Liran Goren is perfectly correct when she mentions the absence of a timescale associated to the “update steps”. However, this is not a major issue because:

1) in the steady-state mode, these “update steps” should not have any timescale associated to them (the model is not solving for a time evolution of a topography but for a static state at steady-state)

2) in the dynamic mode, the process of updating the flow network topology after each time-step is the classical approach that most LEMs follow (e.g. Braun & Willett, 2013), as also mentioned by Liran Goren.

I agree that other algorithms, such as the one used in DAC, could be used in future studies to decide whether to update the flow network topology.

- I now discuss (in section 7 – Discussion and conclusion) in more-depth the potential issue of using the steepest descent flow algorithm and mention that more-physical algorithms could be used instead, as suggested by Liran Goren.

Moreover, an interesting case study, which is not attempted in the current manuscript (the author might want to consider including it), is when U varies in space and time. A convergence issue might arise in such cases: Each topology is associated with different tau(x) for each node x and a different max(tau(x)). Each topology, therefore, samples a different time range of the uplift rate history (from the present to max(tau) in the past), both generally, for the whole landscape, and specifically for each node, following eq. 7. Is there a way to guarantee topological convergence for such cases? Couldn't there be scenarios where the topology jumps between different configurations that sample different U histories without converging?

If I agree this could represent an interesting additional test, although the paper is already long for a short communication. Therefore, I have added a discussion sentence (in section 7) on this scenario to mention that the algorithm might have some convergence limits.

Section 4 starts with a declaration that the model input would be "potentially heterogeneous but constant uplift rate U(l)". In practice, in this section, the model is run with a uniform and constant U. The outcomes do not differ from those of section 5 with a time-variable uplift rate. The reason is that section 4 implicitly assumes that before U = 10 mm/yr, U was 0. This means that there is a temporal change (and no spatial change) in U, that generates a knickpoint, much like those in section 5. In fact, Figure 4c shows 2 knickpoints: One that corresponds to the transition from U = 0 to U = 10 mm/yr and the second from U = 10 to U = 20. The present form of section 4 is therefore redundant.

I agree with this comment by Liran Goren that some redundancy exists between section 4 and 5. At the same time, the paper is also designed to add, step by step, some complexity. If it is correct that the model presented in section 4 already includes a temporal variation of uplift rate (as the model starts over a flat surface representative of U=0), section 4 is mostly designed to explain how the model, which is first used in a steady-state mode, can be used in a dynamic mode (which by essence is obtained by considering a change in uplift rate at the beginning of the simulation). Moreover, section 5 considers the general case of variations of uplift rate during the simulation itself which lead to a more complex solution (i.e. the "uplift memory map" approach) than the one presented in section 5. Section 5 is also focused on highlighting the quality of the model to track knickpoints in the Salève simulations, which is an application of the model developed in section 4.

- I have now clarified the role of each section by mentioning that section 4 already exhibits a case of time-variable uplift rate and that section 5 is an application dedicated to investigate the quality of the model to simulate and track knickpoints

Hillslopes are suggested to be represented by the SPIM with m=0, giving rise to a constant critical slope. To maintain a constant slope over the grid, with information propagation based on the response time, the erodibility of hillslopes, K_3, should be infinite. This is a reasonable approximation, although a physically weird concept, for critical angle hillslopes, but there is no need for a numerical

solver to represent it. These are just constant slope lines that adjust instantaneously to changes in elevation at their base (l_2).

I disagree with this comment.

First, K3 should not be infinite. Indeed, to guarantee that erosion rates are the same everywhere, the model must impose a continuity of erosion rates at the location A(l1) and A(l2), the transitions between hillslope, colluvial and fluvial domains. This imposes that:

E(l1)= K1 A(l1)^m1 S^n = K2 A(l1)^m2 S^n, and thus that K2=K1 A(l1)^(m1-m2)

E(l2)= K2 A(l2)^m2 S^n = K3 A(l2)^m3 S^n and thus that K3=K2 A(l2)^(m2-m3)=K2 A(l2)^m2 (as m3=0)

It gives, K3=K1 A(l1)^(m1-m2) A(l2)^m2, which is generally not infinite

Second, the benefits of using a response time for the hillslope domain, despite being a constant slope, is that it allows to manage all the model nodes using the same simple functions, which reduces model complexity. Moreover, it allows to consider potentially other formalism for hillslope erosion without requiring redeveloping a specific function. I agree that further developments could be made, such as using the interesting DAC approach for hillslopes (Goren et al., 2014), but I believe this is not mandatory.

- I now acknowledge, as a perspective, the coupling of Saleve with DAC at the end of section 6.

To summarize, the concept of an infinitely accurate LEM, based on analytic solutions is appealing. However, the implementation presented in the current contribution raises some doubts regarding the model's usefulness for the landscape evolution community.

I hope I have managed to convince Liran Goren of te usefulness of the developped approach. I thank here once again for her overall positive review.

Line comments

Page 7, line 4. The concept of "thresholding the response time so that for every node □□(□□, □□) = min(□□(□□), □□)." is not clear. If it meant to address the case when the response time is > max time for which there is information about the uplift, then this is like assuming U = 0 before information is available. This is a specific choice. Why assume U = 0 and not any other value?

Indeed, this is equivalent to assuming that U=0 before the simulation starts. This is what most LEMs also do when they start with a flat initial surface. The objective here is to reproduce the settings of these models.

- To clarify this point, I now explicitly mention in section 4 (just after equation 6) that "Thresholding the response time enforces that the uplift rate is considered null before the beginning of the simulation."

T**he discussion of the courant number (page 7, around line 20) is a bit over-emphasized. The analytic solution indeed has no inherent time limitations. As stated, it can be used to find the relief structure for any random time given the topology and the history of U.**

It is an important result of this new model to have no numerical limit on the time-step used.

References

Venditti, J. G., Li, T., Deal, E., Dingle, E., and Church, M., 2019. Struggles with stream power: Connecting theory across scales. Geomorphology, page 106817. https://doi.org/10.1016/j.geomorph.2019.07.004.

Willett, S. D., McCoy, S. W., Perron, J. T., Goren, L., and Chen, C.-Y., 2014. Dynamic Reorganization of River Basins. Science, 343(6175). 10.1126/science.1248765.

Fox, M., Goren, L., May, D. A., and Willett, S. D., 2014. Inversion of fluvial channels for paleorock uplift rates in Taiwan. Journal of Geophysical Research: Earth Surface, 119(9):1853–1875. 10.1002/2014JF003196.

Goren, L., Fox, M., and Willett, S. D., 2014a. Tectonics from fluvial topography using formal linear inversion: Theory and applications to the Inyo Mountains, California. Journal of Geophysical Research: Earth Surface, 119(8):1651–1681. 10.1002/2014JF003079.

Goren, L., 2016. A theoretical model for fluvial channel response time during time-dependent climatic and tectonic forcing and its inverse applications. Geophysical Research Letters, 43(20):10,753–10,763. 10.1002/2016GL070451.2016GL070451.

Goren, L., Willett, S. D., Herman, F., and Braun, J., 2014b. Coupled numerical–analytical approach to landscape evolution modeling. Earth Surface Processes and Landforms, 39(4):522–545. 10.1002/esp.3514. ESP-13-0028.R2.

Rudge, J. F., Roberts, G. G., White, N. J., and Richardson, C. N., 2015. Uplift histories of Africa and Australia from linear inverse modeling of drainage inventories. Journal of Geophysical Research: Earth Surface, 120(5):894–914. 10.1002/2014JF003297.