Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/esurf-2021-59-RC1, 2021

Comment on esurf-2021-59

Anonymous Referee #1

Referee comment on "The direction of landscape erosion" by Colin P. Stark and Gavin J. Stark, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-59-RC1, 2021

Overview

The manuscript considers the erosion of surfaces using a three-dimensional generalization of the Stream Power Model (Royden and Perron (2013)), which expresses the temporal change in surface elevation in terms of the drainage area and the stream gradient. The generalization treats the landscape as an implicit surface and describes the erosion in the normal direction as a function of the surface tilt (Eq. (24)). This extension has practical advantages, it allows for the handling of steep or concave geometry, which is not uncommon in geological settings.

The underlying assumptions (constant external forcing and erodibility) lead to a pure geometric PDE, in which the evolution of the surface is determined solely by the surface geometry. As the geometric PDE is of the Hamilton-Jacobi type, the corresponding Hamiltonian is presented and as it is common for the eikonal equation, it is found that the variational principle associated with the evolution is *least erosion time*. At the moment, the model is constrained to the 2D setting, in which the trick in (26) can be carried out. Hence, the numerical examples are solved only in 2D by using the ray tracing equations. The manuscript concludes that the existence of the Hamiltonian has some insights regarding erosion, and claims that the Hamiltonian setting clarifies the phrase ‘direction of erosion’.

The manuscript builds on well-established concepts and techniques ranging from classical mechanics to erosion models, (aparat from an elegant trick) the novelty of the presented material is uncertain.

About the erosion model

The application of the Hamiltonian machinery is elegant, however, it strongly relies on the erosion model introduced in Eq. (24), which is identical with the model in Royden and Perron (2013), but it is expressed in the normal instead of the vertical direction. Later, Eq. (76) defines the erosion model by specifying the flow component of Eq. (24). The physical background and interpretation of this formula are missing. Back to Eq. (24): why not assume the surface normal speed in the form (x)f(sin), where f:[-1,1], a suitably regular function?

Figure (5) shows an example of the ray tracing approach, but also introduces a physically different problem: fault slipping, which was not mentioned before in the text. It is unclear why it is chosen from the possible physical problems listed in the Introduction.

About the findings of the paper

The abstract suggests that one of the conclusions of the work is that "erosion takes the path of least erosion time" but later in Sect. 2.14, the concept is introduced as a proposal. It is unclear whether it was an assumption or an outcome of the model. The manuscript lacks any physical interpretation of the least erosion time, as a *variational principle*.

The significance of the rays is also unclear as the proposed equations could be solved with other numerical methods without difficulty. The manuscript points out that the slope exponent \eta and the direction of the rays are correlated, but the physical interpretation of this finding and the parameter \eta itself are missing. Figure (5) suggests that the ray tracing approach would not be appropriate if there were overhangs on the surface.

One of the findings of the manuscript is to show the *anisotropy* of landscape erosion, however, anisotropy is not defined in the manuscript. This emergence of anisotropy stems from the applied normal speed in Eq. (24). As the authors state in the introduction, surface erosion cannot be tracked by tracers lying on the surfaces, and therefore no true point pairs exist on the before-after states. It means, any *well-posed pairing* (i.e., a bijection) between the points of two, consecutive shapes in the evolution establishes an erosion model. Nonetheless, most models in the literature use the local normal to establish that bijection, and the present manuscript is not an exception. Still, the sentence in the conclusion '*the rate of surface erosion is a velocity normal to the surface: differential geometry tells us that no other direction has any meanin*g' is false and misleading.

The introduction suggests that no previous examples of modeling erosion as the evolution of implicit surfaces exist, but there are some papers on the evolution of implicit surfaces under erosion equations (Kraft et.al. (2011), Bencheikh et.al. (2020), The main result stating that the geometry of a surface determines its erosion is also a concept that constitutes the base of many works in the literature (Bloore (1977), Wilson (2009), Sipos et al. (2011), Domokos et al. (2014a), Domokos et al. (2014b)).

Although the paper presents a simple, two dimensional equation, it misses to compare the computational outcome to available experimental results. For example, the simple flume experiments on cuboid marble blocks in Wilson (2009) might be reproduced.

Other comments

Section 2 mixes topics that are tightly and loosely connected to the main ideas of the paper. It suggests that it gives us an overview of the background of the work, but it also contains statements about the model and the implementation (e.g., it refers to the Hamiltonian that is introduced in section 3.8). This makes it hard to differentiate between the known results, new ideas, related works, and suggestions for future investigations. Some of the sentences of this section would fit better in the conclusion section. It is hard to follow the line of thought of the text due to the interruption by remotely related topics. Although the beginning of Section 2 mentions the 2+1D stream-power law models, there is no explanation or mention of them.

There are many explanations and statements that are not used and it would be enough to reference them or move them to the appendix. Some textbook concepts (such as covectors in Section 3.1.) are superfluous and should be omitted entirely. Valid parameter ranges are not discussed in detail and regarded only with the jargon "on a shell."

Overall impression

Despite the strengths of the manuscript, the reviewer does not recommend publication in the present form. Although the methods and results are discussed and described in detail, the motivation and the significance of the work remain unclear. The erosional model is not new and the results seem to be consequences of the chosen solution technique without any physical significance.

References

Bencheikh, I., M. Nouari, and F. Bilteryst. "Multi-step simulation of multi-coated tool wear using the coupled approach XFEM/multi-Level-set." Tribology International 146 (2020): 106034.

J. Bloore, The shape of pebbles. J. Int. Assoc. Math. Geol. 9, 113–122, 1977

Domokos, Gábor, Gary W. Gibbons, and András A. Sipos. "Circular, stationary profiles emerging in unidirectional abrasion." Mathematical Geosciences 46.4 (2014a): 483-491.

Domokos, Gabor, et al. "How river rocks round: resolving the shape-size paradox." PloS one 9.2 (2014b): e88657.

Kraft, Susanne, Yongqi Wang, and Martin Oberlack. "Large eddy simulation of sediment deformation in a turbulent flow by means of level-set method." Journal of Hydraulic Engineering 137.11 (2011): 1394-1405.

Royden, L., and Taylor Perron, J. (2013), Solutions of the stream power equation and application to the evolution of river longitudinal profiles, *J. Geophys. Res. Earth Surf.*, 118, 497– 518, doi:10.1002/jgrf.20031.

Sipos, András A., et al. "A discrete random model describing bedrock profile abrasion." Mathematical Geosciences 43.5 (2011): 583-591.

Wilson A: Fluvial bedrock abrasion by bedload: process and form. Dissertation, University of Cambridge, UK, 2009