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Interactive comment on "How Hack distributions of rill networks contribute to nonlinear slope length–soil loss relationships" *by* Tyler H. Doane et al.

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Discussion paper

Comments on "How Hack distributions of rill networks contribute to nonlinear slope length–soil loss relationships"

October 19, 2020

The manuscript discusses results from the Scheidegger model in terms of the distribution of drainage area and the power scaling of drainage area and length (i.e., Hack's law). In general, I found the paper interesting; however, I have some concerns regarding the methods and derivation of the results. The main problem tackled in this work can be re-stated as finding the distribution of the area under a Brownian motion trajectory before its first return. However, I am not convinced-given the explanation in the current version-that this has been achieved. Especially the solution in Eq. (7), which is the basis of the majority of the results, is very problematic and needs a lot more explanation. In what follows, I try to highlight my main concerns about the presented derivation, followed by some general and minor comments.

Starting from Eq (5), this equation does not read as it meant "a unity probability with zero variance". It should be expressed as an atom of probability at $w = 1$ and zero for other w. Second, should it be $w = 0$ instead of $w = 1$? Right above this paragraph, it is stated that watersheds are closed at $w = 0$; thus they should also initiate at $w = 0$.

The transition to Eq (6) is somehow abrupt. Up to this point, the paper paints a discrete picture of the rills network with r being the scale. Now we see a diffusion equation that is indeed defined in a continuous domain. Does this mean $r \rightarrow 0$ is assumed here? If so, the initial and boundary conditions should take the form of the Dirac Delta. Perhaps write the stochastic equation for w and then Eq (6), which is the corresponding FP. Also, keep the s dependence of function f just to makes things clear. Eq (7) should be written as $f(w, s)$; s is a variable –similar to time- not a parameter. Otherwise, it becomes very confusing when the authors talk about initial conditions.

Line 113: It is not clear where this boundary condition comes from? Is this meant to model the closing (termination) of watersheds at $w = 0$. If so, I do not think this is valid. If we think of $w(s)$ as random trajectories which follow a Brownian motion and die out when $w(s) = 0$, there should be an atom of probability at $w = 0$ for each s (rather than zero probability) to model all trajectories that die out at that s. In line 123 it is stated that for "For an unrestricted Brownian random walk . . ." which makes me believe this section, including Eq 7, considers an unrestricted stochastic process. In that case, we have a diffusion equation,

$$\frac{\partial f(w,s)}{\partial s} = D\frac{\partial^2 f(w,s)}{\partial w^2}, f(w, s = 0) = \delta(w) \tag{1}$$

This equation gives a solution in the form of normal distribution rather than Rayleigh. This should be clearer. Line 117- "The Rayleigh distribution arises for the problem of the magnitude of the sum of two normally distributed variates". This statement is very confusing. The authors wrote Eq (7) for w, which indicates w is a Brownian motion and has no information whether it is a sum of two variables. Basically, w is a Brownian motion itself, and there is no reason it behaves differently than its constituents. Although the difference may come from the boundary conditions for which I expressed my concern earlier.

At some point, I got the impression that the authors meant to fix length "l" and basically look at all trajectories of w, which starts at $w(s=0)=0$ and end at $w(s=l)=0$. If this the intention, I am not sure if the linear diffusion still holds for those trajectories as we are sampling the trajectories in a very specific way.

Some parts of the paper give the readers the impression that the Scheidegger model is in the same class as, for instance, OCNs. This is not true and very misleading. OCN describes the steady-state solution of a sediment balance and satisfies both the continuity equation of water and sediment. However, the Scheidegger model only heuristically gives “a solution” for the water continuity equation in the form of a network that may not be achievable from any flux-based model.

Eq 11. It is not clear how this is achieved. Is it an empirical equation?

Fig 2- The label of the x-axis is missing.

Line 17- “Efficient” in what sense? Energy? Flux? How do we know they are efficient?

Rill flow length l needs to be defined in the intro.

Line 55- The second question is not clear! Try to rephrase.

Line 73- Optimal Channel Network (OCN)?

Line 75- “As such, . . .”: Is this referring to OCN? What is the “constraint”? OCN has a “clear rule” to construct networks.