Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., author comment AC4
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Reply on RC4

Amilcare Porporato

Author comment on "Hydrology without dimensions" by Amilcare Porporato, Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-442-AC4, 2021

Dear Professor Hergarten,

Thank you for your kind review and useful suggestions. I've appreciated the positive criticism and made sure that the revised version clarifies some of these points (reported here in italic when necessary) as much as possible.

Regarding Eqs. (1) and (2) and the definition of the scaling factor lambda, the reviewer raises an interesting point that is worth better explaining in the revised version of the manuscript. Formally, the argument of the function f, especially if the function is transcendental, should be dimensionless (see e.g. Barenblatt 1996). On the other hand, lambda as a scale factor, should have the dimension of the inverse of x. The fact is that, as a matter of common use, we often omit writing explicitly unit factors which fix the dimensions in this regard. Thus, one either considers the scaling in Eq 1 with regard to dimensionless quantities (then lambda, x, and f are all dimensionless and things work well – I believe this what mathematicians have in mind when they speak of homogeneous functions like here), or uses dimensions and then either explicitly (but this becomes very cumbersome) or implicitly assumes the presence of these unit factors that convert the units. E.g.

f(lambda*x)=(1*lambda)^n f(1*x).

where the different '1' have different dimensions…

I guess, this is somewhat similar to the fact that log(x) only makes sense if x is dimensionless, so in practice it is log(x/1), but in common practice this is not done, also because if one then writes this as log(x)-log(1) then we start over again… or it is enough to think of power series expansions, where obviously there are omitted factors that make the dimensions consistent …

The revised version of the paper clarifies these points. Thank you.

(2) From my own background, Section 3.3 about landform evolution modeling is particularly interesting, and it nicely shows some recent work of your group. However, I am a bit wary about the concept of the specific drainage area (Eq. 28) and its application in the landform evolution model (Eq. 27). Let us assume a smooth (so with continuous derivatives) topography with a dendritic network of valleys. Then the flow pattern in a large valley consists of many fibers with different upstream lengths and thus with different

specific discharges. Due to the dendritic structure, the across-valley pattern of the specific discharges is quite irregular, and the fibers even come closer to each other downstream.

I fully agree with you here. This is a perfect description of the differential geometry of a (smooth) landscape and is exactly what happens in terms of streamlines and specific contributing area (see Bonetti et al. PRSA 2018).

For me, this concept differs from the "classical" idea of a river with a given width, so that the model with the specific discharge differs in its spirit from what was previously assumed in this context.

Defining a river is in my view a different story from what we do here and in general should not be confused with numerical (or theoretical) issues related to the solution of a given equation, which needs to be well posed. One should keep in mind that these minimalist models of landscape evolution have a very limited and rudimentary representation of the physical processes. For example, if one also considers the surface water flow-field, then one sees that there is water everywhere all the time, which obviously makes no physical sense. In our case, defining rivers is not the goal, but rather the understanding of the mechanisms of hierarchical branching and formation of valleys and ridges (of course one could define thresholds of certain water height to define rivers, but this would be quite unsatisfactory, I think).

I fully agree that the widely used version with the drainage area instead of the specific drainage area is inconsistent when proceeding from a discrete network to a continuous topography and leads to results depending on the spatial resolution of the grid. Unfortunately, it even seems that scaling relations were recently developed without taking care of this problem (Theodoratos et al. 2018). The alternative approach of considering a river as a line with a finite width (Howard 1994, Perron 2008) or rescaling the diffusion term (Pelletier 2010) are also not free of problem, which also applies to my own approach (Hergarten 2020).

I fully agree.

However, if you apply your model to a discrete grid, you practically integrate a^m (where a = specific drainage area). The result will differ from A^m (where A = drainage area). which means that the river consisting of fibers erode at a different rate than a river with a given discharge. So I am not completely convinced that your approach avoids the problem of the grid-spacing dependence unless the grid is fine enough to resolve all the small fibers (which would practically not be feasible).

The fact is that ‘a’, the specific contributing area is a variable defined in the continuum (i.e. pde) representation. Once the domain is discretized, then suitable operators should be found and for each grid point one has to decide what the contributing area is. This is an important numerical problem, but not a theoretical one.

Please do not get me wrong -- I do not want to criticize published work of your group too much. I just think that your reasoning about replacing A by a might be somewhat oversimplified and not free of caveats. I would be happy if you could add some discussion about these aspects. And in case I am wrong, please accept my apologies.

Thank you again for the interesting points of discussion and for spurring me to clarify and better explain these issues. In the revised version I’ve tried to clarify better these points and the derivation of the landscape evolution equation to show that ‘a’ is the right variables. No PDE in continuum mechanics has directly an extensive variable like A in their terms (it would be like having volume V of diameter D of the pipe directly in the Navier Stokes equations rather than using them as boundary conditions.

Barenblatt, G. I.: Scaling, self-similarity, and intermediate asymptotics: dimensional analysis and intermediate asymptotics, 14, Cambridge University Press, 1996.

Bonetti, S., Bragg, A., and Porporato, A.: On the theory of drainage area for regular and non-regular points, Proceedings of the Royal Society 670 A: Mathematical, Physical and Engineering Sciences, 474, 20170 693, 2018.