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Comment on hess-2021-442

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Referee comment on "Hydrology without dimensions" by Amilcare Porporato, Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-442-RC4, 2021

The article "Hydrology without Dimensions" by Amilcare Porporato addresses scaling laws and nondimensional properties in hydrology with emphasis on applying the Pi-theorem. As stated in the acknowledgments, the article is related to the Dalton medal lecture by the author. So the paper is somewhere between an original research paper and a review paper. Although somewhat unusual, such a focus makes sense, and I am sure that the community (including advanced students) will appreciate this paper.

Similar to the other reviewers (who were faster than me), my assessment of the manuscript is overall positive. I would suggest a publication with very few moderate adjustments, including one more check for typos.

(1) Right in the beginning of the theory, at Eqs. (1) and (2), I stumbled over the definition and the meaning of the scaling factor lambda. Formally, the argument of the function f should always have the same physical dimension, so that lambda should be nondimensional. And I would typically assume that lambda must be the same for all values of x, which makes lambda = 1/x problematic. Your point is clear, and I think it will not be a serious problem for the readers. But maybe you find another elegant formulation of the equations that avoids the issue.

(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. 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.

A 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). 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).

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.

Best regards,
Stefan Hergarten

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